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Abstract

In this paper, we focus on a novel and potentially important aspect of the workfare policy in the Danish labor market, namely its effect on crime. We do this by exploiting two policy changes. First, 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. We exploit the differential introduction of workfare reform across different age groups as the exogenous variation. Second, 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 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.

Our results show a dramatic decline in the arrest rate among welfare recipients after the introduction of the stronger workfare requirements, both at the national level and in Farum. 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 on unemployment insurance or welfare[1], as a way of helping them into employment. Some activation programs such as job placement programs could be very effective in finding jobs to workers, while other programs, such as training programs may increase workers' human capital or merely getting them used to a disciplined lifestyle. The employment effects of active labor market policy in Denmark is mixed at best (see Rosholm and Svarer (2004)). 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 that active labor market programs increase regular employment 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 active labor market policy in reducing the welfare dependency is likely 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 problems. 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 active labor market policies for welfare recipients are not worth the cost, except

[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.

perhaps, the private employment programs applied to the more employable welfare recipients.

However, active labor programs may not only be good for employment. Participation in the programs may also help individuals abstain from criminal activity. If this is the case, this should be examined carefully and be taken into consideration when active labor market programs are designed and their cost benefit analysis are made. This is especially the case for the programs 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 are much more costly than the workfare policies. The cost of incarceration not only includes its physical cost, but also dynamic costs, which are the stigma of an arrest record and criminal human capital accumulation in prison. Bayer et al. (2008) 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 active labor market programs (in shorthand, 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. Donohue and Siegelman (1998) extensively survey evaluation studies of U.S. social programs on whether they reduce crimes. 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 of a criminal nature. One must also abide by minor rules, such as

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 %. In contrast to programs such as Job Corps, where participants not only self select in to the program but also were carefully screened, the Danish ALMPs apply to anybody whose stay on welfare has exceeded the passive period. That is, we are not evaluating a new program or a pilot program. We are pointing out a large and positive benefit that so far has been overlooked, of a program that has been around for more than 15 years. Hence, our results would not only help communities that already have adopted the ALMPs in better understanding their crime reduction effect, but also provide 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. There may exist a direct effect from the reduction in leisure due to work, training or education, which may simply leave less time for crime. Jacob and Lefgren (2003) measure the short run effects of a schooling day on crime. They estimate 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 are already enrolled to school and 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. What we measure is the effect of an extensive margin, when a local government assigns individuals who would have stayed home to activation programs. 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 likely to be the most criminally active.

The extensive margin may include the effect of removing individuals from a criminal lifestyle and place them in a working lifestyle without crime. In that sense, our work is related to the large body of empirical work that estimates the effect of unemployment on crime. The existence of a

strong relationship between unemployment and crime rates 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.

Even though the positive correlation between unemployment and crime is well established, 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. The example could be an adverse social event, such as membership in a gang, or family breakup. In order to identify the causal relation from unemployment to crime, researchers try to find exogenous variations that affect unemployment but not crime directly. Raphael and Winter-Ebmer (2001) use closing of military base as exogenous variation that affects unemployment. Notice that the events such as base closing or plant closing not only change the employment of the individuals working in the area but also negatively affect the local community, which may increase crime as well. 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 examine the effect of a series of national reforms on activation policy for young introduced during the 1990s. 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 the exogenous variation to estimate the policy effect. Second, we

analyze the effect of a radical workfare policy in a municipality. In 1987 a Danish municipality by the name Farum introduced immediate ALMP participation for all individuals who received social benefits. That is, when an individuals went to the welfare office in order to obtain benefits the person would at the same time be enrolled in a ALMP. In the rest of Denmark ALMP participation would normally not occur until individuals had received benefits continuously between 3 months and XXX months. We use the introduction of imminent activation in Farum as treatment and examine the causal effect on crime in Farum compared to the rest of Denmark.

In the study 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.

Our results show that in both estimation exercises using the national reform and the one in Farum, the workfare has a statistically and economically significant negative effect on crime, which leads us to conclude that the crime reduction effect of workfare programs is robust to the environment that is implemented. It comes from the reduction of crimes by unemployment uninsured, who were the target of the active labor market policy reforms. We also find that the effect is not only from the increase in regular employment of the unemployment uninsured, but the policy reduces crime also for the individuals who are uninsured and stay unemployed. That means, active labor market programs are beneficial to the society even if they do not lead to any transition to regular employment.

Even though the two policy variations can be considered exogenous, there still remain some sources of bias. First, 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. Thus, the treatment effect of later reforms could have a downward bias. Since Farum's reform was implemented for all ages, the estimated policy effect is not subject to the above dynamic

treatment bias. Second, 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 leaving welfare, which has been termed as "threat effect" by Black et al. (2003). This would also be a source of downward bias because those who leave welfare to regular employment or school are likely to be less criminally active. In the specification where we also estimate the policy effect on the sample of individuals that includes both unemployment insured and uninsured men, the estimated overall policy effect would include the threat effect. However, in Farum there is another threat effect: individuals can avoid 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 does not suffer from such source of bias.

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 has to either work for an employer, be self-employed, or participate in a training course or higher education for at least 18 months. Then, the worker can receive unemployment insurance payments when unemployed and actively seeking employment when he/she has been a member of the UI fund for more than one year and has worked full time for 26 weeks (6 months) during the last 3 years. Also, individuals who had just finished education or apprenticeship could become a member of UI fund and obtain benefits after only one month of membership and without the employment requirement. Once eligible, the workers 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 as long as the individual participated in activation program after the expiry of passive periods of unemployment insurance, up to 9 years.

After 1994, individuals who were unemployed for four years had to participate in the activation programs up to three years. Furthermore, the participation in activation programs did not make the unemployed eligible for another rounds of UI. Gradually after 1994, the first passive

periods of unemployment has been shortened. In 1996, the initial passive period was further reduced to two years, 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 do not receive unemployment insurance benefits or not employable individulas receive social assistance (welfare) from the government. Individuals with a personal fortune or an employed spouse can be subject to some restrictions, and potentially not be entitled to any assistance.

The recipients of social assistance are younger, are 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 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 follow strictly the national unemployment insurance policy. The recipients have to report to the local employment office, and it ensures that they are actively seeking jobs, as well as meeting the other demands for receiving benefits.

There have been a number of changes to the social assistance system both during the 1980s and the 1990s. From the mid 1980s more and more emphasis was put on workfare (activation), in particular for the young.

In 1990 the so called youth-benefit law("Ungdomsydelse") was introduced for youth below 20, and in 1992 (July), it was expanded to youth below 25. According to the law, in order to

receive the welfare benefit, the young individual 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 starts after 13 weeks of unemployment, and for those older than 25 it starts 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 1998 all individuals below the age 30 had to be activated after 13 weeks of unemployment.

However, these reforms were not tightly enforced. The actual implementation of the activation 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 implemented more ambitious activation schemes that started earlier and lasted longer than the national guidelines.

2.2 The case of Farum

From 1987, after the appointment of Lars Bjerregård as an employment consultant in late 1986, the municipality of Farum made a series of radical changes to its activation policy for recipients of social assistance. [2]

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

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

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 areas etc. (Birkbak, 1997:13)

After the employment of Bjerregård two drastic changes were made regarding social assistance and activation in Farum. First, for an unemployed individual to be able to receive social assistance, he/she had to be in some form of activation. This meant that unemployed were activated the very first day they applied for unemployment benefits at the municipality and had to report to the firm where they were activated or to local activation facility "Produktionshuset" (the Production House) every workday from then on – at 7.00 am. Second, the practice of activation was changed so that unemployed individuals mainly went to work in private firms at reduced wage 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 unemployed individual did not have the necessary skills, linguistically or otherwise, from May 1987 the individual 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 in order to receive benefits. It is also worth noticing 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 after a series of lawsuits from Danish labour unions, complaints from the ministry of employment, and the scandal of mayor Peter Brixtofte. In particular, there were allegations put forward by labor unions in 2000-2001 that activated workers were used as cheap labor by firms that had political connections to the mayor and were given contracts with the Production House. Those activities

were alleged to violate the laws protecting the workers and give those firms unfair competitive advantage. In 2000, 2001, Danish parliament started to discuss the matter. In 2002, after a series of newspaper reports on those 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 Farum experiment so that by 1998, young welfare recipients were under the 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 wit a public body. 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 extremely detailed panel data sets with, sometimes, weekly observations of the entire Danish population going back more than 20 years. The dataset made available to us includes data for the period 1981 to 2005 and covers the entire population in that time span. It includes information on a large number of 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 there are no problems of attrition.

Our focus will be men in the between age 18 and 30, since this group has the highest crime rate and crime rate is very low after the age 30. 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

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.

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 the municipality of Farum, and the 5 % random sample of the rest of Denmark.

We find that Farum has much higher rate of unemployment insured than the rest of Denmark. We also find that the young men in Farum have somewhat higher arrest rate than 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, but 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 developed countries, higher proportion of the young men in Farum are immigrants from developing 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. Notice also that 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 individuals. 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 unemployment insured are twice as likely to have them as the uninsured. It is interesting to note that in the rest of Denmark, relative size of Danish and developed country immigrants are higher for the unemployment insured than the uninsured, but in Farum it is the opposite. 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. We can see that 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 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 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 men who were not unemployment insured, we clearly see the effect of the three main activation reforms implemented over the 1990s.[3] 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 1995 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 1995, does not coincide with the peak year of unemployment, 1992. This is because the 1992 reform for the 20-24 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 1999 to 2002, 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 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

[3]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. Because of this, we cannot identify the exact age of crime before 1990, and since 1990 is the start of the welfare reform for 18-19 age group, for the analysis of the national welfare reform, we dropped any data before 1991 and only conducted the regression analysis for the reforms targeted for 20-24 and 25-30 age groups.

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 change 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. During the 90's Danish government made a series of reforms where it implemented Farum style tough activation policies, where uninsured workers have to attend training/job placement programs after the expiration of the initial grace period, which has been

steadily shortened. 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 because 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 in contrast to the unemployment rates of unemployment uninsured, 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 and thus they affect Farum and the rest of Denmark similarly. 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, hence 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 changed differently 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.

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. 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 crime rate to 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 verdict 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 unem-

ployment uninsured in Farum and the rest of Denmark. In Figures 15 and 16, we plot what we believe would be the most likely candidates 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, subsequently, for both Farum and the rest of Denmark. As we can see, during the policy period, the ratio of nonwestern immigrants have declined somewhat relative to that of the rest of Denmark. Similarly for the average years of schooling. 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

To estimate the effect of changes in national workfare policy, we use the following linear Difference-in-Differences model.

$$\begin{aligned} C_{it} &= X_{it}\beta + \sum_{a=19}^{29} I_{a,it}\gamma_a + \sum_{t=1991}^{2003} I_t\gamma_t \\ &\quad + \left[I_{a\in\{20,24\},it} \times I_{t\geq 1994}\right]\delta_1 + \left[I_{a\in\{25,29\},it} \times I_{t\geq 1998}\right]\delta_2 + \varepsilon_{it} \end{aligned}$$

where C_{it} is the number of verdicts of individual i in month t, X_{it} are variables representing individual's socio-economic backgrounds, such as whether he received higher education or not, whether he is married or not, whether he has children or not, etc. $I_{a,it}$ 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. The parameter δ_1 estimates the policy effect of 1994 reform for age group 20-24, and δ_2 the policy effect of 1998 reform for age group 25-29. We also estimate the above equation using fixed effects regression. Next, we use the following

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

$$C_{it} = X_{it}\beta + \sum_{a=19}^{30} I_{a,it}\gamma_a + \sum_{t=1982}^{2003} I_t\gamma_t + I_{F,it}\gamma_F + I_{F,it} \times I_{Pt}\delta + \varepsilon_{it}$$

where $I_{F,it}$ is the Farum dummy, which equals 1 if individual i lives in Farum at month t and 0 otherwise, I_{Pt} is the policy dummy which equals to 1 if the time period t belongs to the period when active welfare policy is conducted in Farum, and 0 otherwise. Here, we adopt the Difference-in-Differences estimation strategy, where the policy effect is identified by the parameter δ. The OLS estimator of δ then will be unbiased if ε_{it} is orthogonal to $I_{F,it} \times I_{Pt}$.

There could be two sources of bias. First, the policy effect for the unemployment uninsured could be entirely due to the fact that unemployment uninsured change their status from uninsured to insured during the policy period to avoid the possibility of activation. To deal with the issue, in the econometric analysis we first evaluate the policy effect on the entire population of men aged 18 to 30, both unemployment insured and uninsured, in Farum versus in the rest of Denmark. The estimated overall policy effect on both uninsured and insured would not be subject to the bias due to endogeneity of insurance choice. 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. To deal with this, we first estimate the Difference-in-Differences with fixed effects. That is, we add fixed effects to the above equation as follows.

$$C_{it} = X_{it}\beta + \sum_{a=19}^{30} I_{a,it}\gamma_a + \sum_{t=1982}^{2003} I_t\gamma_t + I_{F,it}\gamma_F + I_{F,it} \times I_{Pt}\delta + \alpha_i + \varepsilon_{it}$$

The OLS 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 controling 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.

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

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.

$$C_{it}=X_{it}\beta+\sum_{a=19}^{30}I_{a,it}\gamma_a+\sum_{t=1982}^{2003}I_t\gamma_t+\lambda_1\left(Z_{it}\right)I_{F,it}\gamma_{F1}+\lambda_2\left(Z_{it}\right)\left(1-I_{F,it}\right)\gamma_{F1}+I_{F,it}\times I_{Pt}\delta+\alpha_i+\varepsilon_{it}$$

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 due to selection. The exclusion restriction is that whether both parents live in Farum or not affects the decision of individuals whether or not to live in Farum but not whether the individual commits a crime or not in Farum. This could be violated if the children with a positive utility shock of committing a crime leave Farum to avoid activation and parents follow. Another possibility would be when parents of children on social assistance themselves are in social assistance and leave Farum to avoid activation. To minimize bias due to those possibilities, we only choose children whose parents are 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 the bias, 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.

5 Estimation Results

5.1 Results Based on National Reforms

In Table 2 columns 2 and 3, we present the OLS and fixed effects results where the sample includes monthly data from 1991 to 2003 on all unemployment uninsured men in Denmark of ages 18 to 29, who are not immigrants from nonwestern countries. In those regressions, ages and time periods are controlled for by age and time dummies, but to save space, we do not report their parameter estimates. In the second column, we report the OLS results. Notice that overall education is negatively related with crime, even though the coefficient on the higher education dummy is estimated to be positive and significant, because of the negative and significant years of schooling coefficient. Being married and having children are negatively related to crime as well. The effect of both reforms, targeting youth aged 20-24 and aged 25-29, on crime are estimated to be negative and significant.

We report the fixed effects results in the third column. It is intriguing to see the coefficients on years of schooling, on higher education dummy to be positive and significant at 5% level. This is likely due to the potential heterogeneity in crime dynamics. In estimating the dynamic model of crime on data on young men in Philadelphia, Imai and Krishna (2004) found two types: the criminal and noncriminal types. For both types the arrest rate declines after the age of 18, but it is the criminal type that has the steeper decline after the age 18. In Denmark, the peak of arrest is around age 20. If, similarly in Denmark, the decline in arrest afterwards is steeper for the criminals, who are likely to have lower schooling, then the fixed effect estimate would result in positive schooling effects. We believe that the marriage coefficient is positive and significant for similar reasons. The fixed effects estimates for the policy effects are estimated to be smaller than the OLS results. The policy effect for reforms targeting 20-24 year olds is estimated to reduce annual crimes by 0.008, which is still economically significant, given the average arrest

rate for 18-29 age group to be 0.11. On the other hand, the policy effect for the age group 25-29 is effectively zero.

In the fourth and fifth columns, we present results for the unemployment insured young men. The OLS and fixed effects coefficients are similar in signs as those for the unemployment uninsured. In the OLS, policy effect for 25-29 year olds is estimated to be small but negative and significant, but none of the fixed effects estimates of the policy effects are estimated to be negative. From those results, we can conclude that there is some policy effect for the unemployment uninsured but not for the insured.

There are several caveats to the above results. First, OLS, by comparing the crime rate of older individuals to younger individuals, cannot separate the cohort effects and the reform effects. That is, the earlier cohort, who will be old enough for the reforms targeting 20-24 and 25-29 olds may have lower crimes than the later and younger cohorts because they have received in the past treatment targeting 18-19 year olds. Hence, OLS estimates of the policy effect of older individuals may overstate the effect. Second, since the 20-24 year olds receiving their treatment period may have already experienced treatment, the treatment effect on them estimated from the fixed effect could only reflect the marginal effect of additional treatment. Then, it is reasonable to infer that the fixed effects understates the policy effect for older individuals. This is consistent with what we find in our estimation exercise: policy effects obtained by OLS are higher than those obtained by Fixed effects.

5.2 Results Based on the Reform in Farum

Next, we present the estimation results where we use the radical activation reform introduced in Farum in 1987 as the exogenous variation. Since from the beginning the policy applied to anybody regardless of age or gender, the estimation results from the policy experiment are much less subject to the cohort effect bias we discussed above. As we discussed earlier, we first

report the estimates of the overall policy effect for both unemployment uninsured and insured. The overall policy effect includes the change in arrest rate due to the switch in unemployment insurance status.

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 1991, the second from 1992 to 1997, and the third from 1998 to 2001. In column 1, we can see that men with more years of schooling, men that have more than highschool degree in education, married men and men with children have less arrest records. All of them are estimated to be significant at 5% level. Both for the first and second policy periods, the interaction term between Farum and time dummy are negative and significant at 5% significance level. For the third policy period, the coefficient is positive but insignificant. This could be due to the convergence of activation policies of Farum and the rest of Denmark from late 1990's. As we discussed earlier, during the late 1990's Farum's activation policy was under critical scrutiny, while rest of Denmark effectively followed Farum's emphasis of activation policy for social assistance program participants. To take that into account, we consider two policy period, one that starts in 1987 and ends in 2001, before the start of the formal investigation of Farum and, and the other that starts in 1987 and ends in 1997, before 1998 when, at the national level all unemployment uninsured individuals before the age 18 had to be activated after 13 weeks of unemployment. In column 2, we present the results where we estimate the difference of crime rates between Farum and the rest of Denmark over the entire policy period, which we define to be from 1987 to 2001. In column 3, we define the policy period to be from 1987 to 1997. We can see that in both cases the estimated policy effect is negative and significant.

In Table 5, column 2 (FE1) we present the results for fixed effects estimation for the $1987-2001$ policy period and in column 3 (FE2) we present the one with the $1987-1997$ policy period. Regardless of whether the policy period is set to be from 1987 to 2001 or 1987 to 1997, the policy

effect is estimated to be negative and significant at 5% level. The magnitude is estimated to be sizeable: -0.0158 for the longer policy period and -0.0181 for the shorter policy period, given that the average arrest rate of Farum during the sample period for both unemployment uninsured and insured men is 0.0720.

One potential source of bias would arise, when during the policy periods uninsured unemployed individuals or individuals who potential would become uninsured would leave Farum or stay out of Farum to avoid activation. Here, we cannot a priori assess whether individuals who leave or stay out of Farum would be more criminally active or not. In Table 5, columns 4 (FE3) and 5 (FE4), we report the results of the Fixed Effects estimates where we also include dummies which equal to one for individuals who left Farum before, during or after the policy period. Even though all coefficients are insignificant, we can see that individuals who left Farum before the policy period are committing relatively more crimes outside Farum than the ones who did during or after the policy period. This does not support the story that during the policy period individuals who would commit crimes would do so when they live outside Farum so that they would not lose their jobs and be activated. But one could also argue that individuals who left Farum before the policy period were the ones who tried to avoid immediate activation. Hence, those fixed effects regressions do not give us clear answers about the bias corrected policy effect.

In Tables 6, 7 and 8, we report results of the Heckman's two step estimation. In the second column of Table 6 (Step 1), we present the parameter estimates of the first stage Probit model. 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 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 significant, and that for the arrest rate in

rest of Denmark is positive and significant. 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. This corresponds to the estimated policy effect coefficient of -0.0120 for the longer policy period, being smaller in value to the fixed effects estimator presented in Table 5 (-0.0158). However, for the shorter policy period the effect is estimated to be -0.0209, whose value is larger than that of the fixed effects estimator in Table 5 (-0.0181). That is, the direction of bias for the policy effect is unclear. On the other hand, the Farum dummy for all specification is estimated to be around 0.25, much larger than the values in Table 5, which range from 0.015 to 0.02. This implies that the selection bias was mainly on the Farum dummy, not on the policy effect. Note that the effect of living together with parents, presented in Columns 2 (FE1) and 4 (FE2) is negative and significant, whereas the effect of living in the same municipalities with parents presented in columns 3 (FE2) and 5 (FE4) is insignificant. In Table 7 columns 3 (FE2)and 5 (FE4) we then present the second stage results where we also include interaction term with age dummies and the cohabitation dummy. There, the baseline age group is 18 to 19 year olds. We can see that the negative cohabitation effect is primarily on the younger individuals, as the sum of age specific dummy coefficient and the baseline coefficient becomes negative but small after the age 19. This may not just reflect the direct effect of current cohabitation on current criminal activity, but also that individuals who were cohabiting with parents around age 18 to 21 were more likely to have cohabited with parents when they were younger, and it is the cohabitation at younger ages that are important in reducing crime. Since we only have very small sample size on individuals at ages younger than 18, we cannot separately identify the two effects. Similar results also hold when we set the policy period to be from 1987 to 1997. On the other hand, living with parents in the same municipality at age 18-19 has a positive effect on crime, but afterwards, the effect becomes close to zero.

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 separate regressions for the unemployment uninsured and insured. If, during the policy period, crimes 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. Table 9 reports the OLS results for the crime equation for uninsured and insured individuals separately. We can see that higher level of education decreases crime (the coefficient of high education dummy is positive and significant for the uninsured workers, but not large enough to offset the negative years of schooling effect), and so are the effects of marriage, children, for both unemployment uninsured and insured. Being either Danish or an immigrant from developed countries reduces crime and being in Farum increases crime, but those effects are not significant, except for the Danish or immigrant from developed countries dummy for the unemployment insured in OLS3 and OLS4 and for the Farum dummy in OLS4. The policy effect is negative and significant for the unemployment uninsured at 5% but insignificant and small for the unemployment insured. The policy effect for the unemployment uninsured is quite sizeable, 0.0345 for $1987-2001$ policy period and 0.0404 for the $1987-1997$, given that the average arrest rate of unemployment uninsured in Farum is 0.1029. In Table 10, we report the fixed effects estimates. Notice that the parameter estimates of the effect of schooling are positive, and significant for the higher education dummy for the unemployment uninsured. Our explanation for the nonnegative schooling effect is similar to the one discussed when we reported the results for both the insured and uninsured. That is, after the peak age of crime, it is the criminal type that reduced crime rates more, and thus individuals with higher schooling, who are more likely to be noncriminal type, would reduce the arrest rates less, resulting in the nonnegative schooling coefficient for the unemployment uninsured. Similar discussions also hold for the coefficient estimates for the marriage dummy, which are also positive and insignificant.

On the other hand, the coefficient for children dummy is estimated to be negative, and significant for the unemployment insured. The policy effect has negative and significant coefficient for the unemployment uninsured but positive and insignificant for the insured. That is, the negative policy effect seems to primarily come from the unemployment uninsured. Again, the magnitude of the policy effect is sizeable, 0.0447 for $1987-2001$ policy period and 0.509 for the $1987-1997$, which is about half of the average arrest rate of unemployment uninsured in Farum.

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 estimation 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 endogeneities that could bias the fixed effects estimation of the policy effect. First, in order to avoid activation when unemployed during the policy periods, individuals could seek jobs that provide unemployment insurance. In Figure 17 we plot the ratio of individuals who are insured for Farum and for the rest of Denmark. We can see that the young men in Farum are less unemployment insured in Farum than 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 11, we report the results of the probit analysis, which estimates the probability of unemployment insurance choice. There, after controlling for the observables, during policy period the unemployment insurance probability is estimated to be higher, and it is 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 individuals in Farum during the policy period should increase the crime rates of the uninsured. Therefore, the resulting selection of insurance choice 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 albeit insignificant coefficient on the policy dummy. If we take the weighted sum of the policy effect for unemployment uninsured and insured, with weights being the unemployment uninsurance and insurance rates, then for the longer policy period the sum is -0.0169, which is very close to the overall policy effect estimate of -0.0158 and for the long policy period, the sum is -0.0210, which is also fairly close to the overall policy effect estimate of -0.0181.

In Tables 12, 13 and 14, we report results of the Heckman's two step estimation for the unemployment uninsured. In the first column of the table, we present the parameter estimates of the first stage Probit model. 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 the Second Step fixed effects regression, the results are very similar to the ones obtained using both unemployment insured and uninsured individuals. The inverse Mill's ratio terms, representing the selection bias are significant and the policy effects are negative and highly significant, and the effects are larger than the ones obtained when both uninsured and insured samples were used. The policy effect again is estimated to be large, around -0.04 for the long policy period and -0.055 for the short policy period. The Heckman 2 step estimation results confirm that it is the reduction of arrests of the unemployment uninsured

that is the main part of the policy effect.

5.3 Direct and Indirect Effects of Activation

So far, we have obtained results that indicate that active labor market policies for welfare participants are 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 employment, and the transition from unemployment or out of labor force to 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 and do not become formally employed. That would be a direct effect, which, to the best of our knowledge , has not been formally investigated. In Table 15, column 3 (FE1) we report fixed effects results, separately for uninsured individuals who were 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: from -0.23, -0.18 for the men who were on welfare more than 25 % of the time to -0.37, -0.36 for those who were on welfare more than 75 % of the time. The decrease in arrest rate is even higher than the overall 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 are the ones whose arrest rates are in the are relatively high even among the unemployment uninsured men. 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 who have been on welfare less than or equal to 50 %, and 75 %. There is a slight tendency for

the policy effect to become more negative from 25 % to 75 %. From those exercises, we can tentatively conclude that the negative effect of activation for the uninsured young men on crime not only comes from the increase in employment, but also from the reduction in crime for the activated but not employed. That is, the activation policy has strong negative impact on crime for those individuals for whom the activation program is considered to have no employment effect, because they are "locked in" in the program. And it seems that those are the individuals who are most active criminally. Thus, our results provide support for the labor market programs for the long term welfare participants. By reaching out to individuals with very low chances of regular employment, active labor market program could still improve the local community by reducing the criminal activities of those who are the most at risk of committing crimes.

5.4 Robustness Checks

So far, for the results involving the Farum experiments, we have presented mostly OLS and IV results. Since number of arrests per year is discrete, those methods in principle are not appropriate. Hence, in this section, we report the results where we have used Count data methods, i.e. negative binomial regressions and conditional fixed effects negative binomial regressions. We only report the policy effect estimates, which are summarized in Table 16. In Column 2 (UI & SA), we report policy effect estimates for both unemployment insured and uninsured. We can see that all the estimates are consistent with those of the OLS and Fixed effects estimates in sign and significance. They are all negative and significant except for the OLS estimate with policy period of 1998 to 2001. Similarly, the policy effect estimates for the welfare recipients presented in column 3 (SA) are also consistent with the OLS and FE results before. All of the policy effects are estimated to be negative and significant, with their effects to be larger than those of the unemployment uninsured and insured. In column 4 (UI), we report the estimated policy effects of the unemployment insured. There, just like the ones obtained earlier using the

OLS and FE, none of the estimates are significant, and some of them positive. In sum, our results of the policy effects are robust to the specification of the functional form used in the estimation.

6 Concluding Remarks

We have estimated the effect of workfare policy of the young male workers, both unemployment uninsured and insured, on crime. We exploited two policy changes. First, 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. Second, 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. We find the crime reduction effect to be both statistically and economically significant. We also find that the effect comes both from the increased employment and from a decrease in criminal activities by individuals that still remain unemployed and on welfare.

Nowadays in Denmark and many other countries in Europe, activation policies 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. For example, recently much active research has been done on the "threat effect" of the programs

(Black et. al. 2003). That is, there are results that the fear of future activation is what drives people to employment. However, fear of activation or the stress of actually going through the activation program could induce individuals to commit more crimes. Then, activation programs that carry less sticks 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. Those are the issues that we believe are left for future research.

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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 Uninsured	0.4425	0.4967	0.3419	0.4743								
Arrest Rate	0.0720	0.3594	0.0636	0.3393	0.1029	0.4346	0.1113	0.4755	0.0475	0.2836	0.03886	0.2360
Age	24.63	3.611	25.06	3.390	23.40	3.700	24.05	3.658	25.60	3.222	25.58	3.116
Years of Schooling	10.97	2.517	11.12	2.328	10.58	2.272	10.62	2.312	11.29	2.654	11.38	2.295
Higher Education	0.4508	0.4976	0.5154	0.4998	0.2740	0.4460	0.3218	0.4672	0.6159	0.4864	0.5912	0.4916
Married	0.2423	0.4285	0.1873	0.3901	0.1508	0.3578	0.1435	0.3506	0.3150	0.4646	0.2100	0.4073
Having children	0.2292	0.4203	0.2027	0.4020	0.1370	0.3438	0.1292	0.3355	0.3024	0.4593	0.2408	0.4276
Danish Citizen	0.8289	0.3766	0.9333	0.2495	0.8539	0.3533	0.8975	0.3034	0.8091	0.3930	0.9519	0.2140
Develped or Danish	0.8437	0.3632	0.9476	0.2229	0.8746	0.3312	0.9216	0.2688	0.8191	0.3849	0.9611	0.1934
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	22980		116454		10169		39812		12811		76642	

Table 2: Regression with National Data

	Unemployment Uninsured		Unemployment Insured	
	OLS	FE	OLS	FE
Dependent Variable	No. of Arrests	No. of Arrests	No. of Arrests	No. of Arrests
Yrs. of Schooling	−0.00283 (0.00003**)	0.00030 (0.00008**)	−0.00072 (0.00001**)	0.00008 (0.00007)
Higher Education	0.00068 (0.00010**)	0.00292 (0.00029**)	−0.00019 (0.00005**)	0.00014 (0.00024)
Married	−0.00173 (0.00018**)	0.00125 (0.00020**)	−0.00057 (0.00005**)	0.00048 (0.00009**)
Children	−0.00223 (0.00014**)	−0.00044 (0.00016**)	−0.00076 (0.00005**)	−0.00029 (0.00007**)
$\text{Policy}_{20\sim24}$	−0.00124 (0.00023**)	−0.00070 (0.00018**)	0.00009 (0.00008)	0.00008 (0.00010)
$\text{Policy}_{25\sim29}$	−0.00068 (0.00021**)	0.0000 (0.0002)	−0.00019 (0.00008**)	0.00037 (0.00011**)
R squares	0.0044		0.0013	
Sample Size	14392532	14392532	10461594	10461594

Note:

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

Standard errors are in parentheses.

Table 3: Count Data Regression with National Data

	Unemployment Uninsured		Unemployment Insured	
	OLS	FE	OLS	FE
Dependent Variable	No. of Arrests	No. of Arrests	No. of Arrests	No. of Arrests
Yrs. of Schooling	−0.3556 (0.0044**)	−0.1133 (0.0453**)	−0.00072 (0.00001**)	0.00008 (0.00007)
Higher Education	−0.1133 (0.0453**)	0.4019 (0.1588**)	−0.00019 (0.00005**)	0.00014 (0.00024)
Married	−0.3835 (0.0501**)	0.0029 (0.0466)	−0.00057 (0.00005**)	0.00048 (0.00009**)
Children	−0.1946 (0.0346**)	−0.0982 (0.0312**)	−0.00076 (0.00005**)	−0.00029 (0.00007**)
$\text{Policy}_{20\sim24}$	−0.1153 (0.0417**)	−0.0682 (0.0350*)	0.00009 (0.00008)	0.00008 (0.00010)
$\text{Policy}_{25\sim29}$	−0.0874 (0.0451*)	−0.0018 (0.0391)	−0.00019 (0.00008**)	0.00037 (0.00011**)
No. of Observations	4886485	808493	10461594	10461594

Note:

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

Standard errors are in parentheses.

Table 4: OLS

	OLS1	OLS2	OLS3
Dependent Variable	No. of Arrests	No. of Arrests	No. of Arrests
Yrs. of Schooling	−0.0193 (0.0005**)	−0.0193 (0.0005**)	−0.0193 (0.0005**)
Higher Education	−0.0021 (0.0020)	−0.0022 (0.0020)	−0.0021 (0.0020)
Married	−0.0216 (0.0022**)	−0.0216 (0.0022**)	−0.0216 (0.0022**)
Children	−0.0253 (0.0020**)	−0.0252 (0.0020**)	−0.0253 (0.0020**)
Developed	−0.0272 (0.0053**)	−0.0275 (0.0053**)	−0.0272 (0.0053**)
Farum	0.0136 (0.0043**)	0.0136 (0.0043**)	0.0140 (0.0037**)
$[87-91] \times Farum$	−0.0201 (0.0069**)		
$[92-97] \times Farum$	−0.0227 (0.0062**)		
$[98-01] \times Farum$	0.0017 (0.0083)		
$[87-01] \times Farum$		−0.0165 (0.0053**)	
$[87-97] \times Farum$			−0.0219 (0.0050**)
R Squares	0.0271	0.0271	0.0271
Sample Size	148044	148044	148044

Note: Standard errors are in parentheses.

Table 5: Fixed Effects

	FE1	FE2	FE3	FE4
Dependent Variable	No. of Arrests	No. of Arrests	No. of Arrests	No. of Arrests
Yrs. of Schooling	0.0011 (0.0021)	0.0011 (0.0021)	0.0011 (0.0021)	0.0011 (0.0021)
Higher Education	0.0231 (0.0081**)	0.0231 (0.0081**)	0.0232 (0.0081**)	0.0231 (0.0081**)
Married	0.0068 (0.0035*)	0.0068 (0.0035*)	0.0068 (0.0035*)	0.0067 (0.0035*)
Children	−0.0053 (0.0031*)	−0.0053 (0.0031*)	−0.0053 (0.0031*)	−0.0054 (0.0031*)
Farum	0.0158 (0.0048**)	0.0152 (0.0043**)	0.0205 (0.0060**)	0.0199 (0.0053**)
$[87-01] \times Farum$	−0.0158 (0.0054**)		−0.0191 (0.0059**)	
$[87-97] \times Farum$		−0.0181 (0.0052**)		−0.0216 (0.0055**)
$[80-86] \times moveout$			0.0149 (0.0081*)	0.0139 (0.0079*)
$[87-01] \times moveout$			0.0005 (0.0058)	
$[02-05] \times moveout$			0.0034 (0.0176)	
$[87-97] \times moveout$				−0.0016 (0.0061)
$[98-05] \times moveout$				−0.0107 (0.0113)

Note: Standard errors are in parentheses. Sample size is 148044

Table 6: Heckman 2 Step Estimation

	Step 1
Dependent Variable	Farum
Yrs. of Schooling	−0.0390 (0.0052**)
Higher Education	0.0996 (0.0281**)
Married	0.2068 (0.0264**)
Children	0.2278 (0.0264**)
Parents outside Farum	−0.3978 (0.0248**)
Parents in Farum	0.5140 (0.0325**)

Note: Standard errors are in parentheses. Sample size is 61108.

Table 7: Heckman 2 Step Estimation, 2nd Step

	FE1	FE2	FE3	FE4
Dependent Variable	No. of Arrests	No. of Arrests	No. of Arrests	No. of Arrests
Yrs. of Schooling	0.0027 (0.0021)	0.0024 (0.0021)	0.0027 (0.0021)	0.0024 (0.0021)
Higher Education	0.0115 (0.0088)	0.0088 (0.0087)	0.0115 (0.0088)	0.0089 (0.0087)
Married	0.0052 (0.0062)	0.0052 (0.0062)	0.0050 (0.0062)	0.0050 (0.0062)
Children	−0.0158 (0.0053**)	−0.0155 (0.0053**)	−0.0159 (0.0053**)	−0.0157 (0.0053**)
Farum	0.2475 (0.0839**)	0.2396 (0.0838**)	0.2523 (0.0838**)	0.2445 (0.0836**)
Parents same Home	−0.0151 (0.0074**)	−0.1189 (0.0378**)	−0.0151 (0.0074**)	−0.1188 (0.0378**)
Parents same Muni.	−0.0026 (0.0063)	0.1015 (0.0409**)	−0.0027 (0.0062)	0.1019 (0.0409**)
Inverse Mill's Ratio λ_1	−0.2142 (0.0701**)	−0.2152 (0.0691**)	−0.2127 (0.0699**)	−0.2132 (0.0690**)
Inverse Mill's Ratio λ_2	0.1052 (0.0494**)	0.0979 (0.0493**)	0.1078 (0.0493**)	0.1006 (0.0491**)
$[87-01]\times$Farum	−0.0124 (0.0075*)	−0.0120 (0.0075)		
$[87-97]\times$Farum			−0.0209 (0.0074**)	−0.0206 (0.0074**)

Note: Standard errors are in parentheses. Sample size is 61108.

Table 8: Heckman 2 Step Estimation, 2nd Step, Continued

	FE1	FE2	FE3	FE4
Dependent Variable	No. of Arrests	No. of Arrests	No. of Arrests	No. of Arrests
$PSH \times [A20 \sim A21]$		0.1048 (0.0375**)		0.1047 (0.0375**)
$PSH \times [A22 \sim A23]$		0.1078 (0.0393**)		0.1073 (0.0393**)
$PSH \times [A24 \sim A25]$		0.1422 (0.0407**)		0.1419 (0.0407**)
$PSH \times [A26 \sim A27]$		0.1095 (0.0417**)		0.1094 (0.0417**)
$PSH \times [A28 \sim A30]$		0.1090 (0.0437**)		0.1088 (0.0437**)
$PSM \times [A20 \sim A21]$		−0.1193 (0.0417**)		−0.1194 (0.0417**)
$PSM \times [A22 \sim A23]$		−0.0948 (0.0413**)		−0.0949 (0.0413**)
$PSM \times [A24 \sim A25]$		−0.1168 (0.0425**)		−0.1172 (0.0425**)
$PSM \times [A26 \sim A27]$		0.1080 (0.0421**)		−0.1086 (0.0421**)
$PSM \times [A28 \sim A30]$		−0.1146 (0.0422**)		−0.1153 (0.0422**)

Note: Standard errors are in parentheses. Sample size is 61108.

Table 9: OLS for Unemployment Uninsured and Insured Workers

	Uninsured Workers		Insured Workers	
	OLS1	OLS2	OLS3	OLS4
Dependent Variable	No. of Arrests	No. of Arrests	No. of Arrests	No. of Arrests
Yrs. of Schooling	−0.0360 (0.0011**)	−0.0360 (0.0011**)	−0.0089 (0.0004**)	−0.0089 (0.0004**)
Higher Education	0.0267 (0.0045**)	0.0265 (0.0045**)	−0.0104 (0.0021**)	−0.0104 (0.0021**)
Married	−0.0430 (0.0059**)	−0.0431 (0.0059**)	−0.0110 (0.0019**)	−0.0110 (0.0019**)
Children	−0.0241 (0.0059**)	−0.0241 (0.0059**)	−0.0160 (0.0018**)	−0.0160 (0.0018**)
Developed	−0.0173 (0.0099*)	−0.0169 (0.0098*)	−0.0228 (0.0050**)	−0.0228 (0.0050**)
Farum	0.0152 (0.0081*)	0.0131 (0.0068*)	0.0063 (0.0042)	0.0097 (0.0037**)
$[87-01] \times Farum$	−0.0345 (0.0100**)		−0.0009 (0.0053)	
$[87-97] \times Farum$		−0.0404 (0.0095**)		−0.0078 (0.0052)
R squares	0.0307	0.0308	0.0192	0.0192
Sample Size	53886	53886	94158	94158

Note: Standard errors are in parentheses.

Table 10: Fixed Effects for Unemployment Uninsured and Insured Workers.

	Uninsured Workers		Insured Workers	
	FE1	FE2	FE3	FE4
Dependent Variable	No. of Arrests	No. of Arrests	No. of Arrests	No. of Arrests
Yrs. of Schooling	0.0039 (0.0025)	0.0038 (0.0025)	−0.0009 (0.0017)	−0.0009 (0.0017)
Higher Education	0.0363 (0.0105**)	0.0362 (0.0105**)	0.0093 (0.0069)	0.0093 (0.0069)
Married	0.0096 (0.0089)	0.0095 (0.0089)	0.0024 (0.0030)	0.0024 (0.0030)
Children	−0.0009 (0.0085)	−0.0010 (0.0085)	−0.0066 (0.0027**)	−0.0065 (0.0027**)
Farum	0.0271 (0.0123**)	0.0246 (0.0110**)	0.0020 (0.0058)	0.0039 (0.0049)
$[87-01] \times Farum$	−0.0447 (0.0142**)		0.0052 (0.0068)	
$[87-97] \times Farum$		−0.0509 (0.0151**)		0.0027 (0.0061)
Sample Size	53886	53886	94158	94158

Note: Standard errors are in parentheses.

Table 11: Probit Estimation for Insurance Choice

	Probit1	Probit2
Dependent Variable	UI	UI
Yrs. of Schooling	−0.0569 (0.0022**)	−0.0569 (0.0022**)
Higher Education	0.8147 (0.0104**)	0.8147 (0.0104**)
Married	0.0407 (0.0115**)	0.0410 (0.0115**)
Children	0.2882 (0.0111**)	0.2886 (0.0111**)
Developed and Danish	0.1453 (0.0142**)	0.1441 (0.0142**)
Farum	−0.2656 (0.0155**)	−0.2505 (0.0133**)
$[87-01] \times Farum$	0.0916 (0.0194**)	
$[87-97] \times Farum$		0.0856 (0.0187**)
Sample Size	148044	148044

Table 12: Heckman 2 Step Estimation, 1st Step

	Step 1
Dependent Variable	Farum
Yrs. of Schooling	−0.0286 (0.0073**)
Higher Education	0.0667 (0.0399*)
Married	0.1265 (0.0408**)
Children	0.2917 (0.0427**)
Parents outside Farum	−0.5138 (0.0379**)
Parents in Farum	0.5112 (0.0425**)
Sample Size	26584

Note: Standard errors are in parentheses.

Table 13: Heckman 2 Step Estimation, 2nd Step

	FE1	FE2	FE3	FE4
Dependent Variable	No. of Arrests	No. of Arrests	No. of Arrests	No. of Arrests
Yrs. of Schooling	0.0024 (0.0041)	0.0020 (0.0041)	0.0024 (0.0041)	0.0020 (0.0041)
Higher Education	0.0312 (0.0162*)	0.0279 (0.0162*)	0.0311 (0.0162*)	0.0278 (0.0162*)
Married	0.0028 (0.0138)	0.0031 (0.0137)	0.0024 (0.0138)	0.0027 (0.0137)
Children	−0.0172 (0.0132)	−0.0176 (0.0131)	−0.0174 (0.0132)	−0.0178 (0.0131)
Farum	0.4545 (0.1778**)	0.4529 (0.1780**)	0.4600 (0.1772**)	0.4587 (0.1774**)
Parents same Home	−0.0336 (0.0146**)	−0.1328 (0.0575**)	−0.0333 (0.0146**)	−0.1323 (0.0574**)
Parents same Muni.	0.0014 (0.0134)	0.1062 (0.0591*)	0.0006 (0.0133)	0.1060 (0.0591*)
Inverse Mill's Ratio λ_1	−0.3376 (0.1422**)	−0.3496 (0.1407**)	−0.3329 (0.1423**)	−0.3442 (0.1407**)
Inverse Mill's Ratio λ_2	0.2206 (0.1032**)	0.2136 (0.1032**)	0.2260 (0.1028**)	0.2194 (0.1028**)
$[87-01]\times$Farum	−0.0404 (0.0154**)	−0.0397 (0.0155**)		
$[87-97]\times$Farum			−0.0546 (0.0163**)	−0.0541 (0.0164**)

Note: Standard errors are in parentheses. Sample size is 26584.

Table 14: Heckman 2 Step Estimation, 2nd Step, Continued

	FE1	FE2	FE3	FE4
Dependent Variable	No. of Arrests	No. of Arrests	No. of Arrests	No. of Arrests
$PSH \times [A20 \sim A21]$		0.1019 (0.0562*)		0.1015 (0.0562*)
$PSH \times [A22 \sim A23]$		0.1161 (0.0627*)		0.1153 (0.0627*)
$PSH \times [A24 \sim A25]$		0.1619 (0.0644**)		0.1615 (0.0643**)
$PSH \times [A26 \sim A27]$		0.1188 (0.0667*)		0.1189 (0.0667*)
$PSH \times [A28 \sim A30]$		0.0661 (0.0807)		0.0654 (0.0807)
$PSM \times [A20 \sim A21]$		−0.1258 (0.0597**)		−0.1257 (0.0597**)
$PSM \times [A22 \sim A23]$		−0.1004 (0.0624)		−0.1006 (0.0624)
$PSM \times [A24 \sim A25]$		−0.1309 (0.0636**)		−0.1317 (0.0637**)
$PSM \times [A26 \sim A27]$		−0.1232 (0.0635*)		−0.1241 (0.0635*)
$PSM \times [A28 \sim A30]$		−0.0980 (0.0661)		−0.0988 (0.0661)

Note: Standard errors are in parentheses. Sample size is 26584.

Table 15: 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	fill in

Note: Standard errors are in parentheses.

Table 16: Robustness Checks, Count Data Results

	UI & SA	SA	UI
Dependent Variable	No. of Arrests	No. of Arrests	No. of Arrests
$[87-91] \times Farum$ (OLS)	−0.2370 (0.1106**)		
$[92-97] \times Farum$ (OLS)	−0.3642 (0.1067**)		
$[98-01] \times Farum$ (OLS)	0.0261 (0.1171)		
$[87-01] \times Farum$ (OLS)	−0.2278 (0.0823**)	−0.3506 (0.1046**)	−0.0246 (0.1268)
$[87-97] \times Farum$ (OLS)	−0.3101 (0.0862**)	−0.4068 (0.1104**)	−0.1753 (0.1339)
$[87-01] \times Farum$ (FE)	−0.1664 (0.0702**)	−0.3016 (0.0927**)	0.0954 (0.1254)
$[87-97] \times Farum$ (FE)	−0.2119 (0.0682**)	−0.3528 (0.0911**)	0.0307 (0.1204)
$[87-01] \times Farum$ (FE & MV)	−0.1924 (0.0755**)	−0.3338 (0.1005**)	0.0653 (0.1351)
$[87-97] \times Farum$ (FE & MV)	−0.2385 (0.0725**)	−0.3977 (0.0972**)	0.0109 (0.1283)

Note: Standard errors are in parentheses.

Figure 1: Unemployment Rate of Young Men

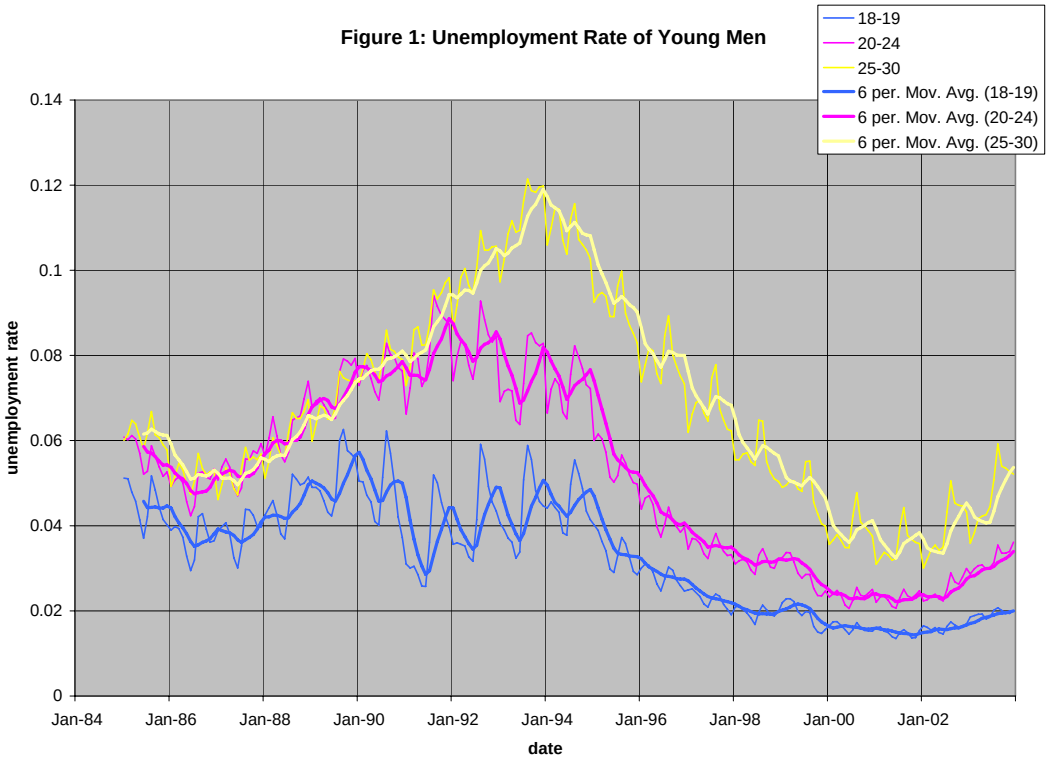

Figure 2: Ratio of Young Men Employed in Regular Job or at School

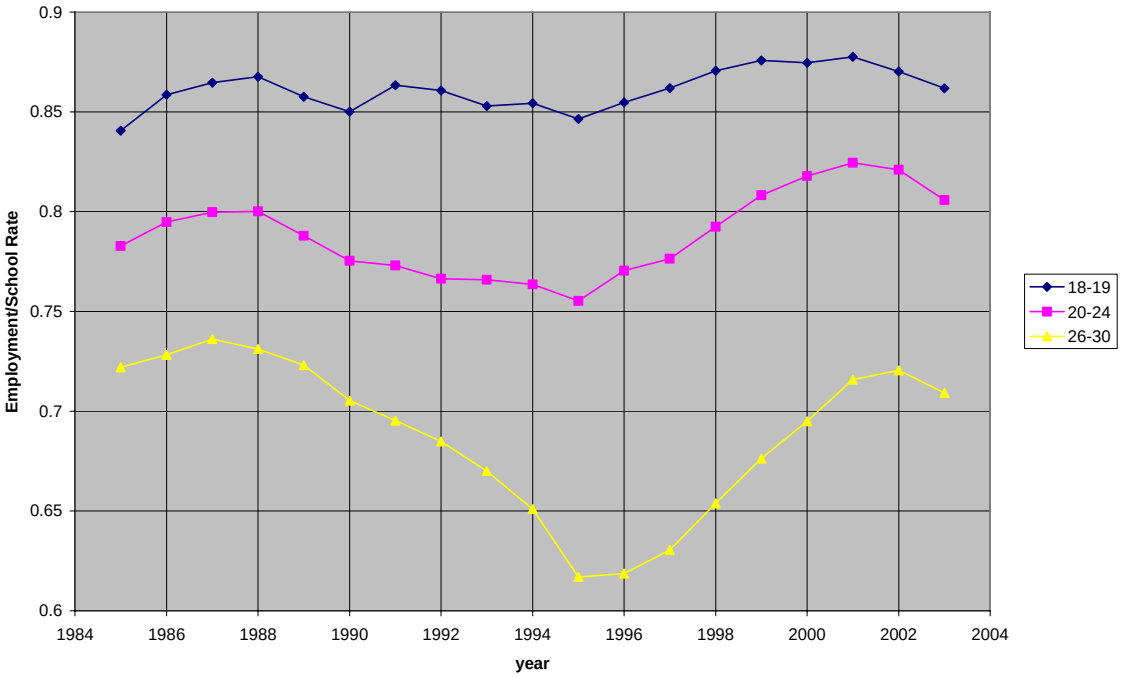

Figure 3: Property Crime Arrest Rate of Unemployment Uninsured Danish Citizens and Western Immigrangs, 12 months moving average

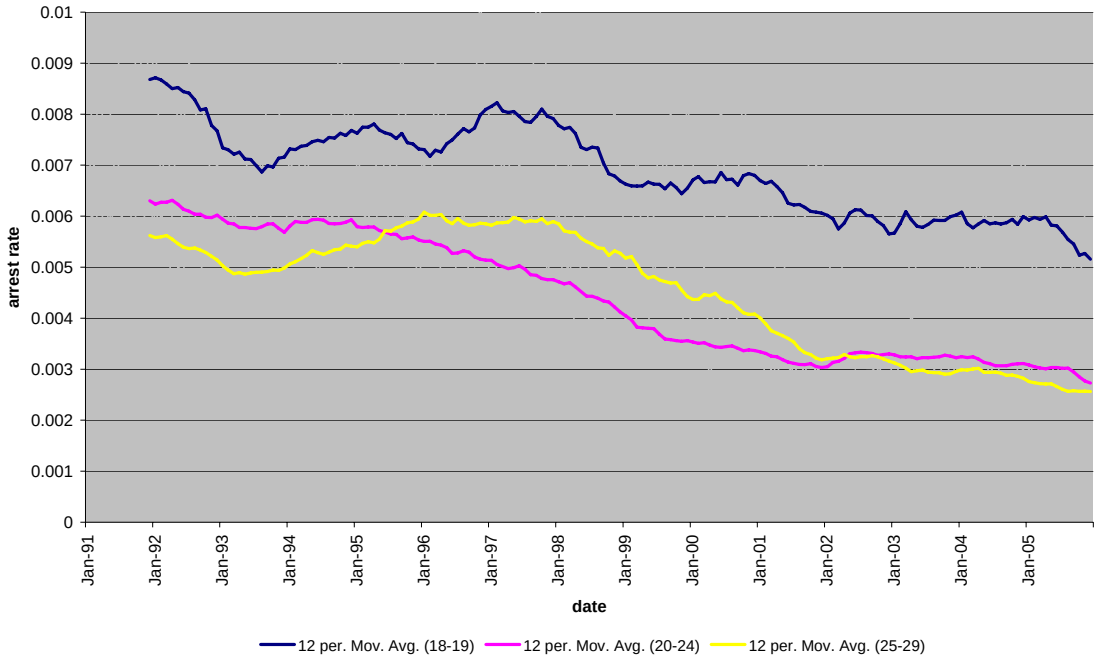

Figure 4: Violent Crime Arrest Rate of Unemployment Uninsured Danish Citizen and Western Immigrants, 12 months moving average

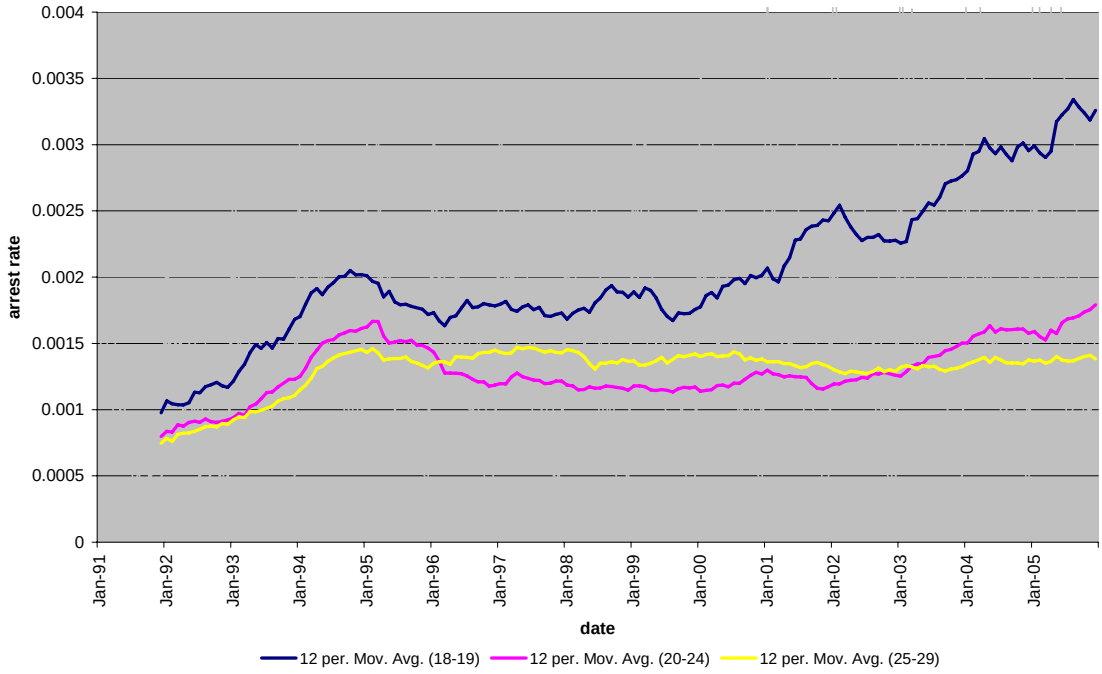

Figure 5: Property Crime Arrest Rate of Unemployment Insured Danish Citizens and Western Immigrants 12 months moving average

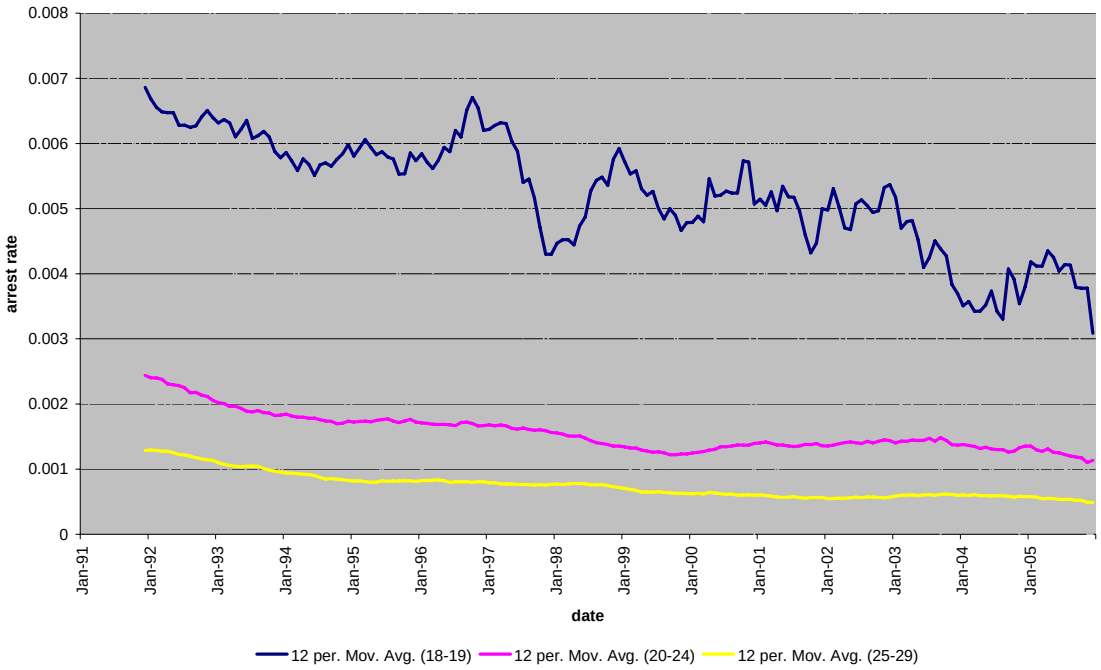

Figure 6: Violence Crime Arrest Rate of Unemployment Insured Danish Citizens and Western Immigrants,12 months moving average

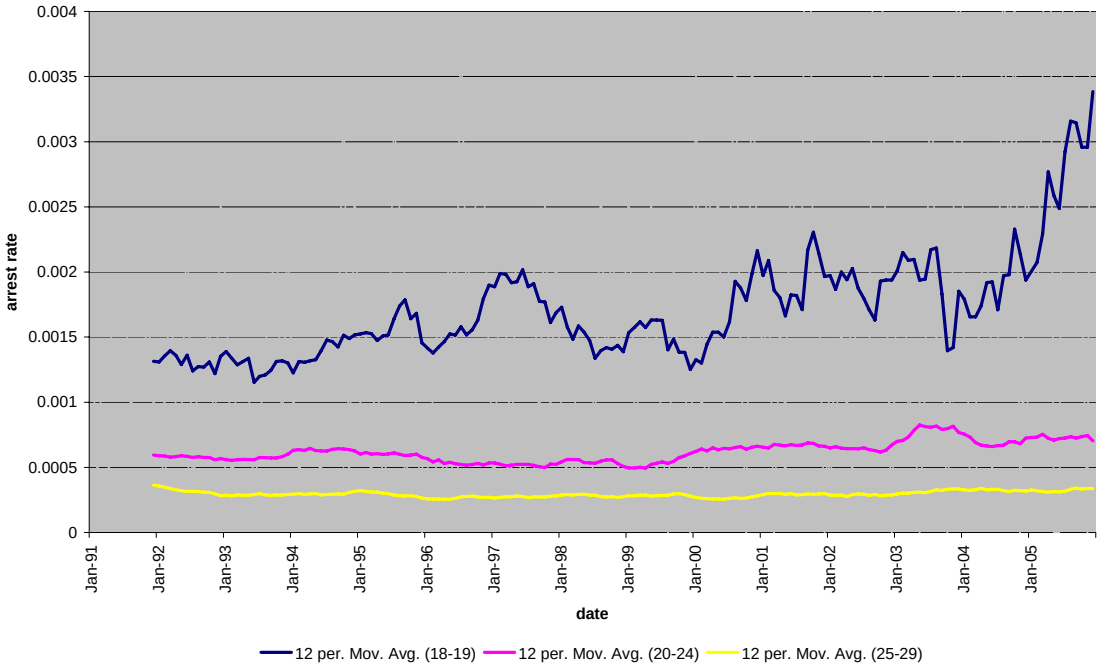

Figure 7: Jobless Rates of Unemployment Uninsured Males, between Ages 18 to 30

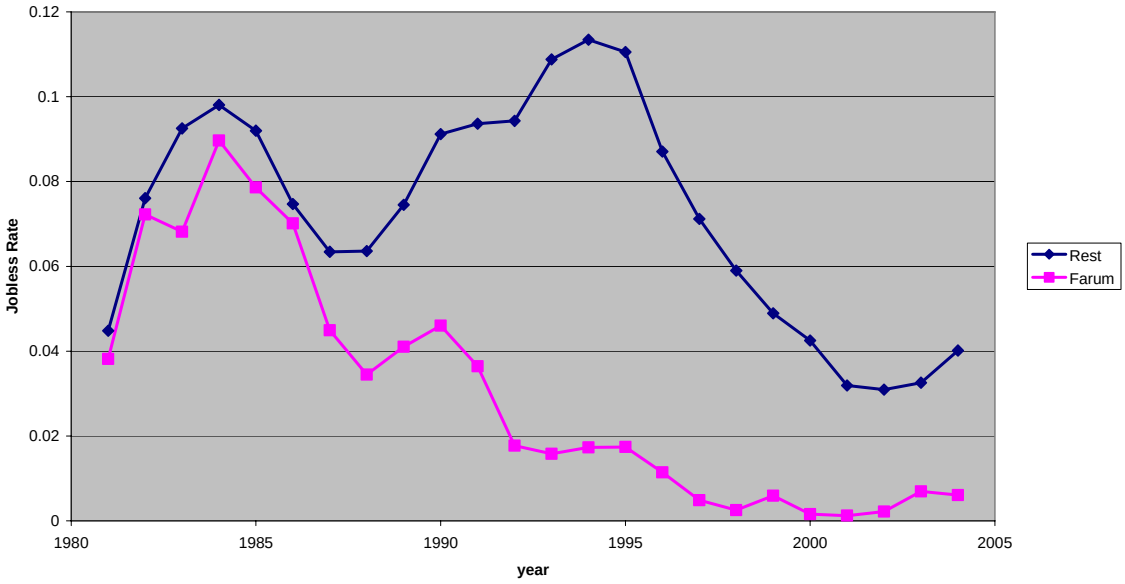

Figure 8: Jobles Rates of Unemployment Insured Men between Ages 18-30

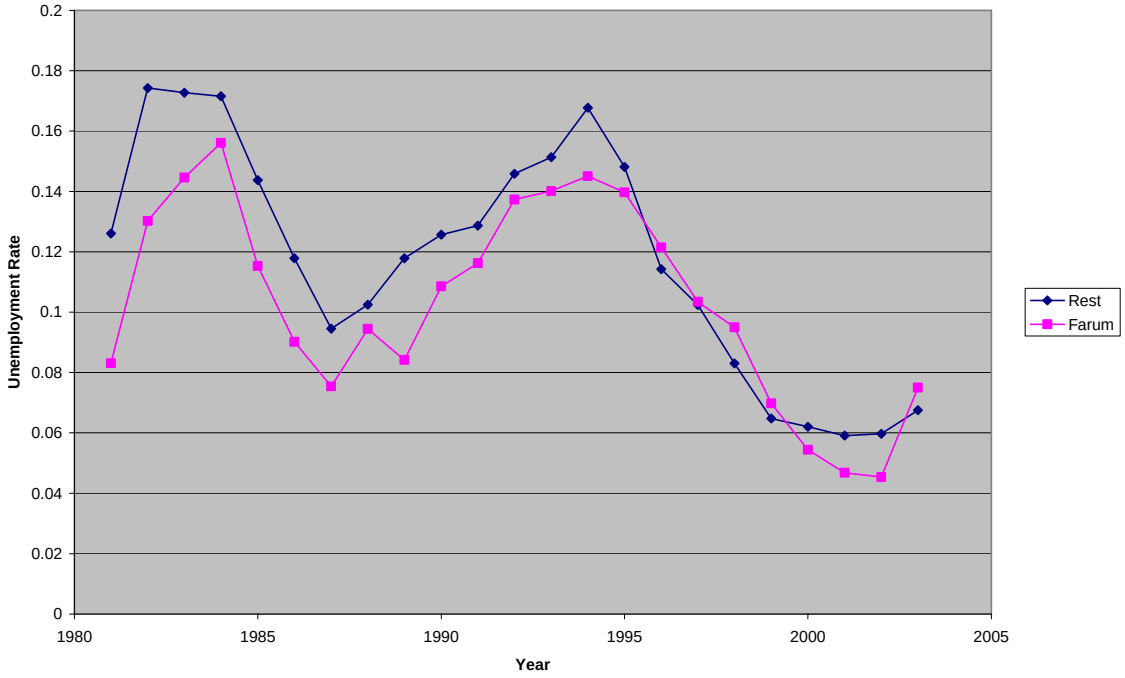

Figure 9: Arrest Rates of
Unemployment Uninsured Men between Ages 18-30

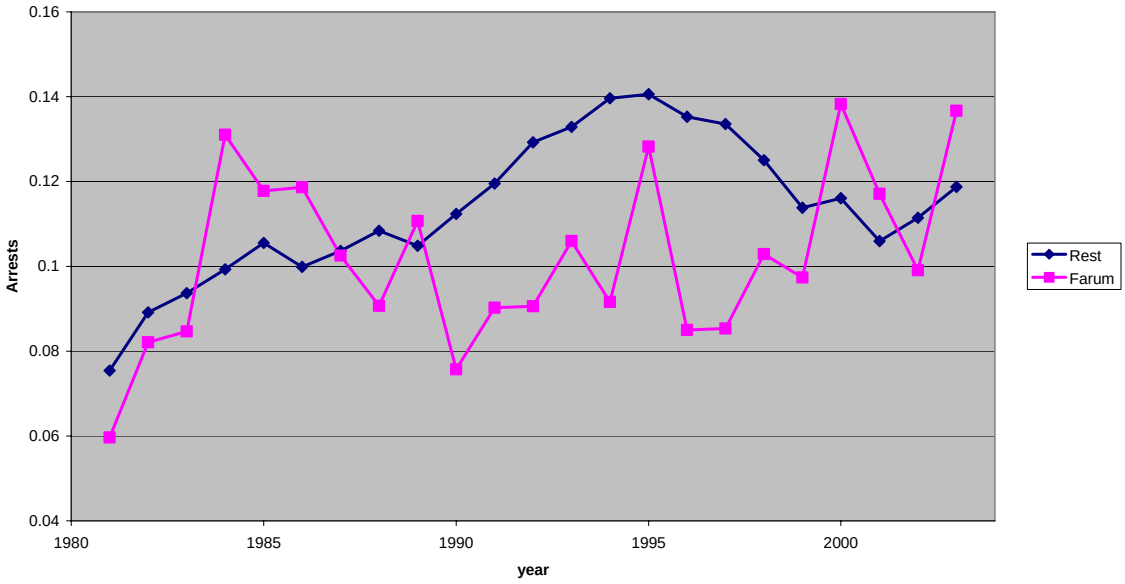

Figure 10: Property Crime Arrest Rates of
Unemployment Uninsured Men between Ages 18-30

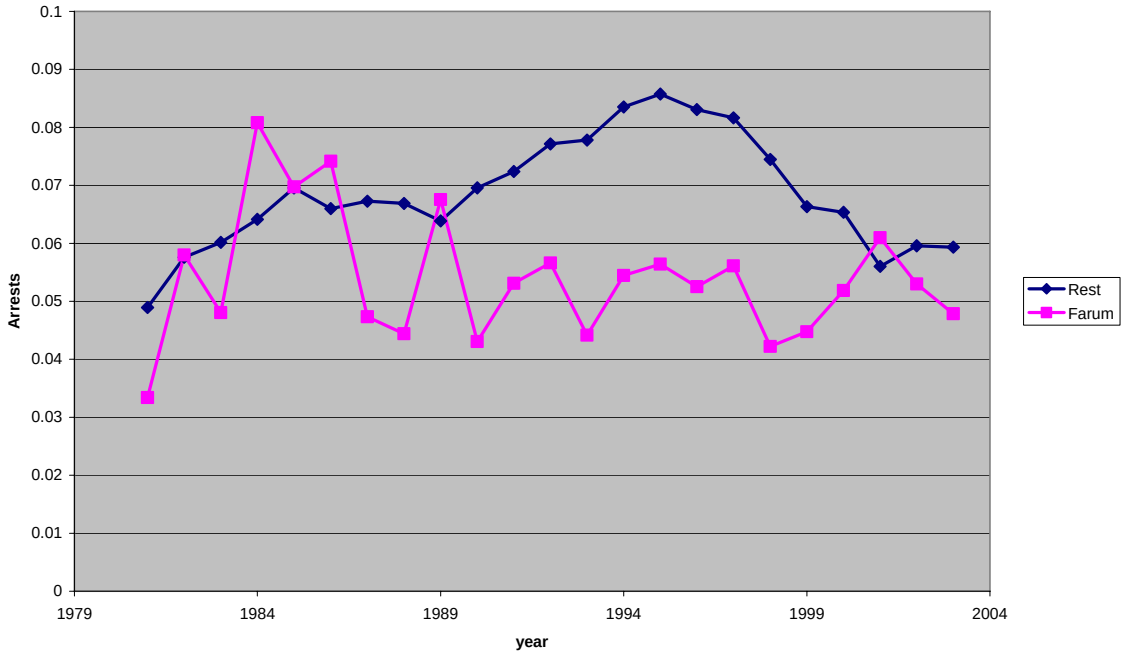

Figure 11: Violent Crime Arrest Rates of
Unemployment Uninsured Men between ages 18-30.

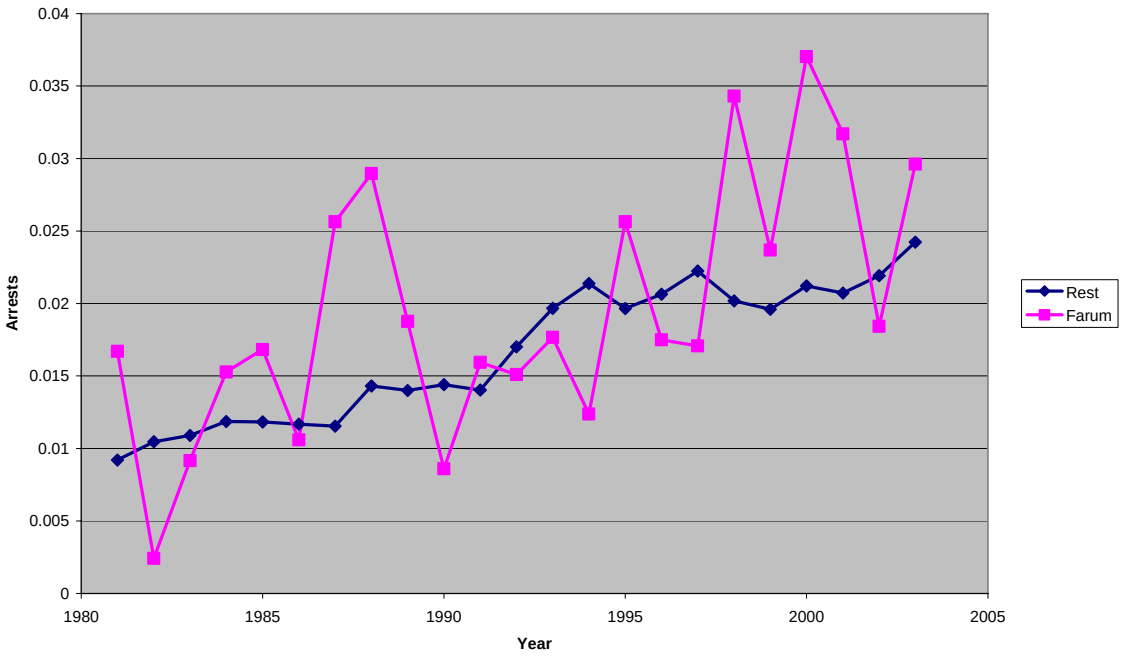

Figure 12: Arrest Rates
of Unemployment Insured Men between Ages 18-30

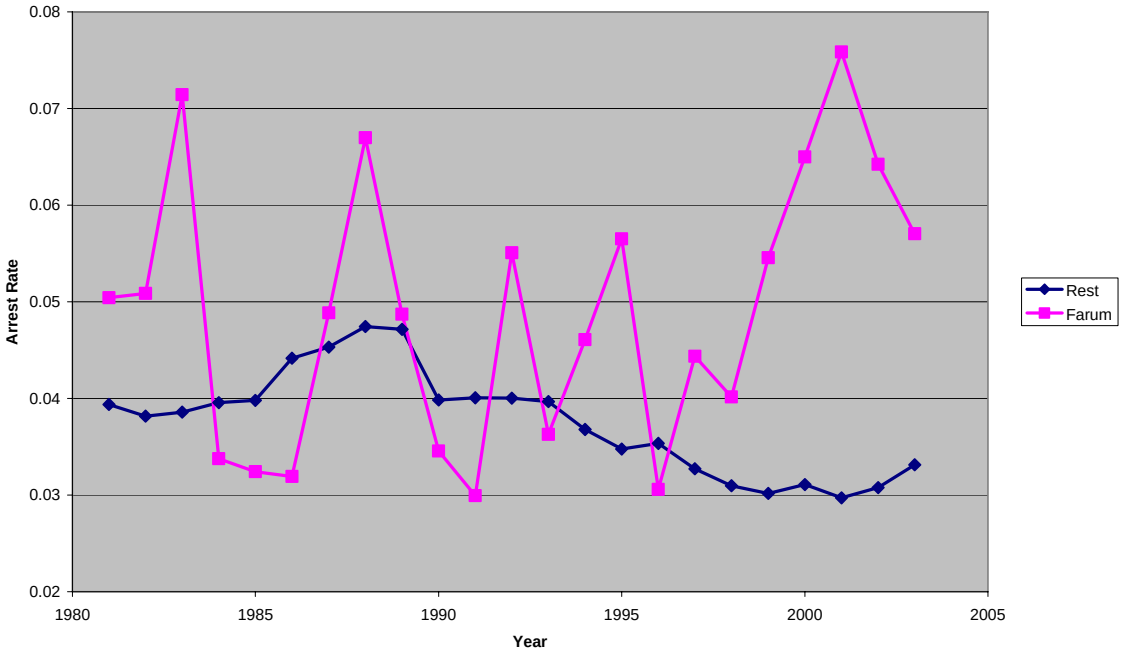

Figure 13: Property Crime Arrest Rates
of Unemployment Insured Men between Ages 18-30

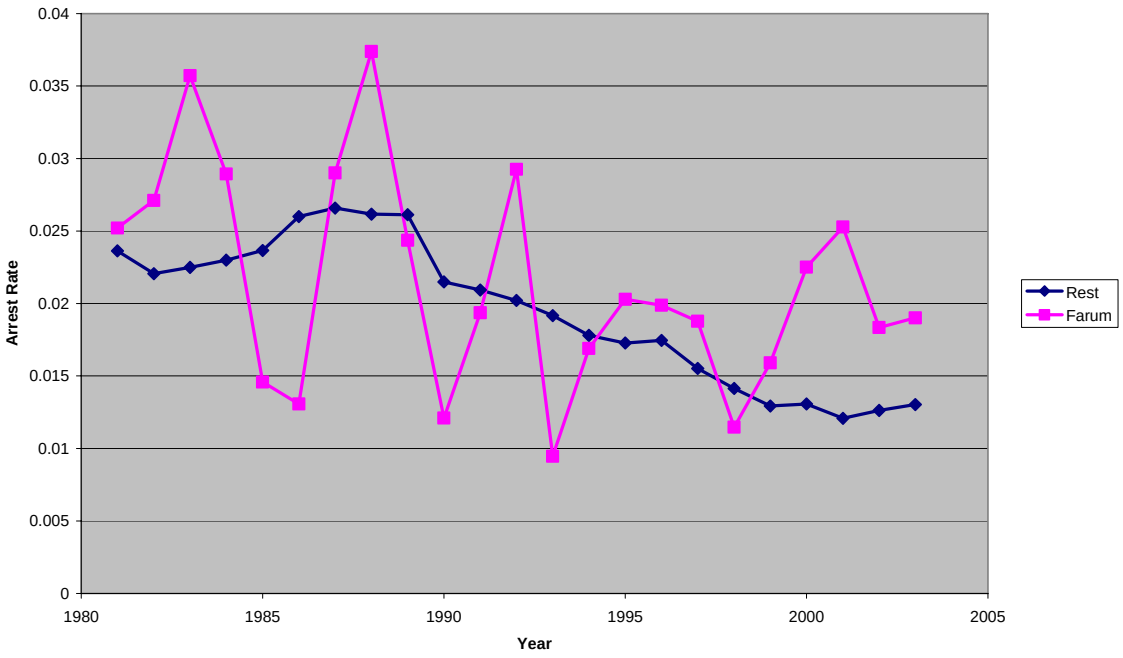

Figure 14: Violent Crime Arrest Rates
of Unemployment Insured Men between Ages 18-30

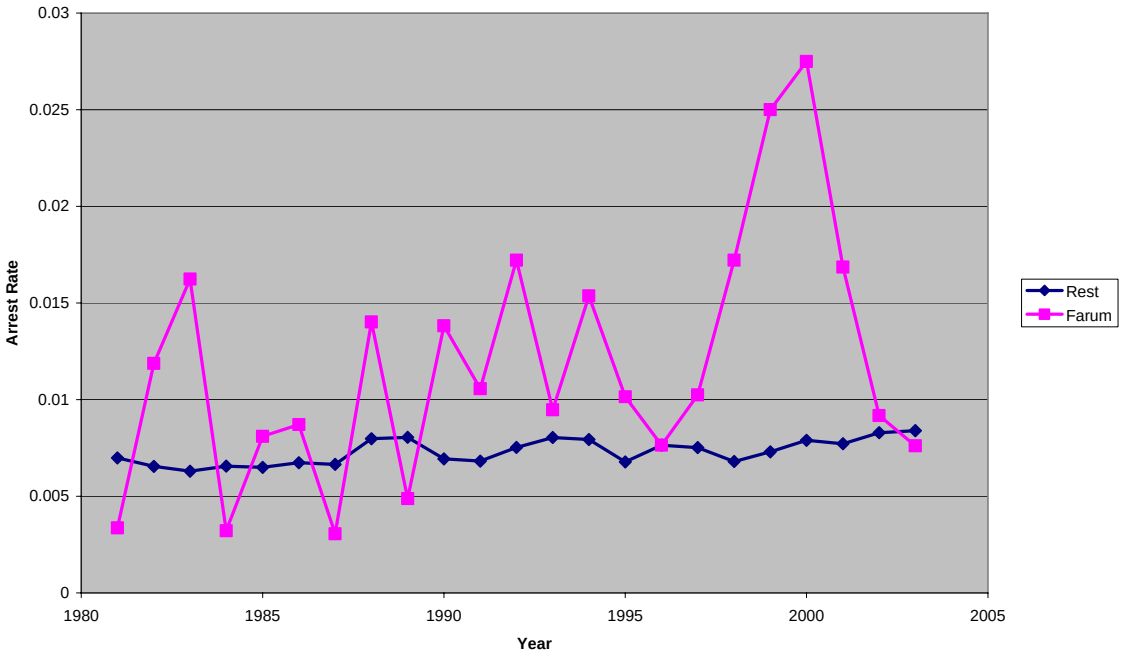

Figure 15: Ratios of Nonwestern Origin Immigrants of Unemployment Uninsured Men between Ages 18-30

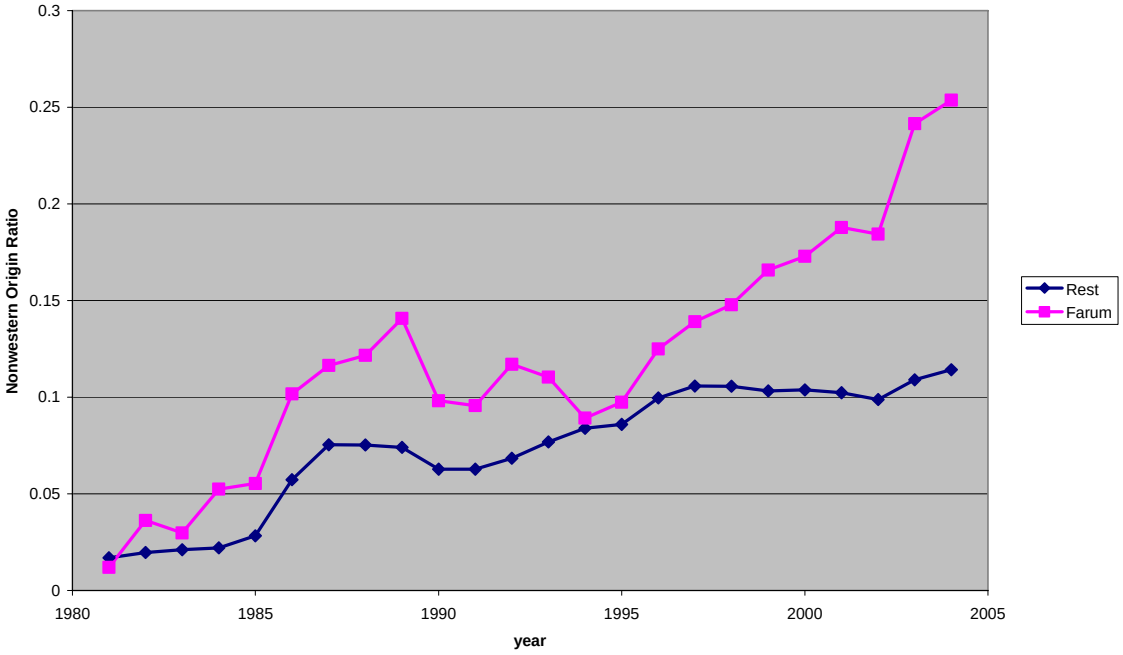

Figure 16: Average Years of Schooling of Unemployment Uninsured Men between Ages 18-30

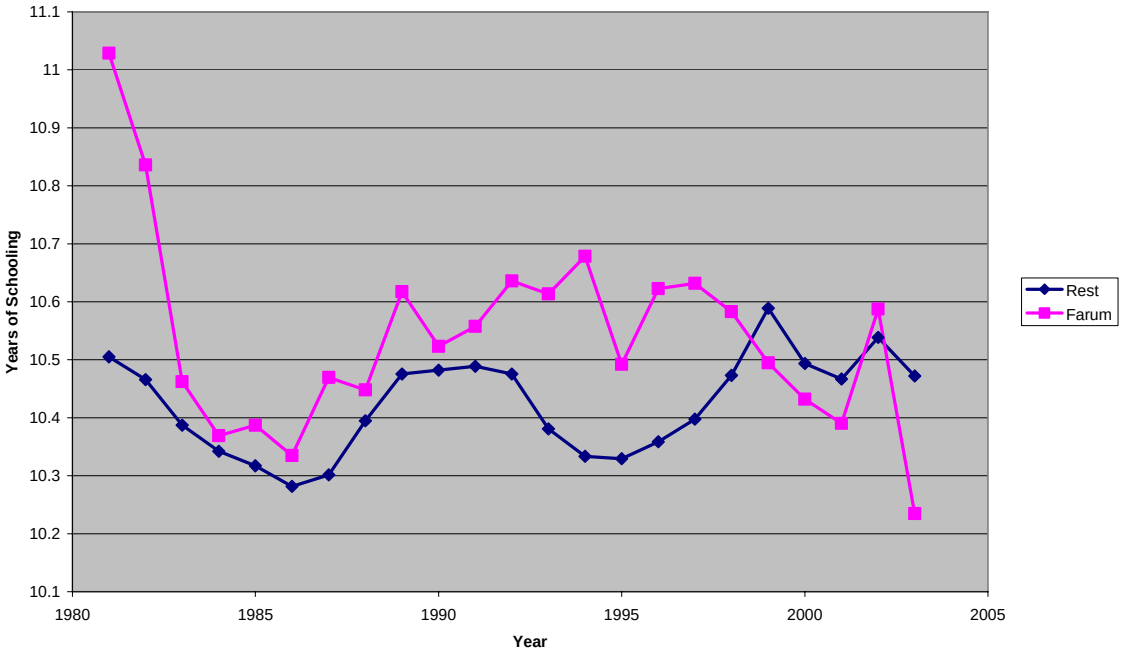

Figure 17: Ratio of Unemployment Insured Men between Ages 18-30