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Essays in Labour Economics

By

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Professor David E. Card, Chair
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Abstract

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This dissertation contains three chapters. The first studies police killings in USA and its determinants. Among other things, it finds that federal data collected on police killings undercount the number of kills by at least two-thirds. Additionally, we find evidence that some policy relevant levers such as the racial diversity of the force affect the number of police killings in an area.

Chapters two and three both relate to the importance of new firms to a local labour market. Studying the region of Veneto, Italy, I find that new firms benefit the local regions they set up in through the creation of new jobs, many of which go to local inhabitants including those not previously employed. I find evidence that a large new firm opening can affect the hazard rate of reentry into the labour force by 1%. However, I find limited evidence of spillovers in wages. New firms mechanically raise the wages of their workers but do not affect the overall region's wages. However, they enable the flow information that allows those workers not initially hired by new firms to find better paying jobs, demonstrating the importance of social networks in wage growth and job creation.

To

Amma, Appa and Vinayak

and to

Ajay and Susan.

Contents

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Chapter 1

Introduction

This thesis compiles evidence on two broad sets of topics. The first is the use of deadly force by police officers against civilians in USA. The second is on the impact that the opening of large new manufacturing firms has on local labour markets in Italy. Chapter 2 deals with the first topic while Chapters 3 and 4 deal with the second. The remainder of this Chapter provides a brief introduction to the ones that follow.

1.1 Police Killings

Chapter 2 attempts to compile facts on civilian deaths at the hands of police officers in USA. While some work in the popular press has been done on this subject, the primary data source has been the Uniform Crime Reports (UCR) Supplementary Homicides (SH) files, which provides the federally collected number of police killings of civilians, coded as "justifiable homicides". These data suggest that there are roughly 500 civilian deaths at police hands per year. This number has been remarkably stable over twenty years despite the fact that the number of violent crimes committed in USA has plummeted in the same period.

Investigation into the UCR SH data reveal that large parts of the USA are either not covered or very undercovered in the survey. For example, large states such as Florida and Alabama do not report any police killings in years 2013, 2014 and 2015. In the same time period, the SH files suggest that many small towns and cities, especially in the south, and some large but important cities such as New York City report no killings as well.

We compare these findings against data collected on police killings for the same period, but from a privately maintained collaborative source curated by journalists called `www.fatalencounters.org` (FE). Information on deadly use of force is submitted by bystanders, witnesses and others from the local community. This is cross checked with news sources, coroner's reports and other local information to verify that the event took place, that it has not already been reported, and to ascertain basic facts about both the victim and the police department responsible.

The FE data suggest that the true rate of police killings of civilians is closer to 1,500 per year. Comparing FE with SF over the same period, we find that every state has more reported killings in the FE data compared to SH. Furthermore, states that are missing are often in the highest quintile of killings, nationally. Drilling down into cities reveals the presence of incidents in smaller towns and cities across USA that go unreported into the federal system, especially from southern states. Larger cities like New York City, Los Angeles, Chicago and Houston all report substantially more kills in the FE data compared to SH.

The FE data also provide important information on victim characteristics. Unsurprisingly, victims are predominantly male. While information is captured on the race of the victim, it is often missing on unreliable. For this reason, we do not enter into the debate on the racial skew of victims of police violence. Victims are overwhelmingly killed by guns, followed by police vehicles. We match 90% of the data from FE to a police department's federal tracking number, the ORI. This allows us to merge federal data on police departments as well as other demographic information to individual incidents.

The Law Enforcement Administration and Management Survey (LEMAS) 2013 collects information on the hiring and training practices of police departments. Additionally, it collects information at the police department level on community policing initiatives as well as the department's guidelines on how force may be used by sworn personnel. We merge killing data aggregated to the level of a police department to the LEMAS data. Additionally, we merge information on the crime rate at an ORI level, the racial demographic at a Census place level as well as information on the racial and gender make up of the top levels of the civilian and law enforcement establishments at the city. In doing so, we limit our analysis to cities that are sampled in LEMAS, which include those with police departments with more than 100 personnel and a randomly selected set of others.

We run cross sectional regressions on our merged dataset to see if departmental practices can predict or explain any of the trends we observe in killings. Due to our lack of a clear identification strategy, the low incidence of police violence in general and further cuts to our data for to have a clean merge, we are unable to find many strong predictors. Still, some things stand out. First, we find that a 10% increase in the fraction of the police force that is black is associated with a reduction of about 1.5. Second, we find that authorising the use of neck restraints, commonly called "choke-holds" is associated with an increase in kills of about 2. To provide context, the population weighted average number of kills per ORI, our unit of observation, is almost 5.

1.2 New Firms

There is a large literature that concerns itself with demand shocks to local labour markets. This is motivated by police questions relating to the effect of policies to promote the birth and growth of large firms in regions. Some believe that this will provide employment, wage growth and productivity spillovers to the region. While there is some evidence at the aggregate level of employment and wage effects and consensus on the short-run effects, there is less known at the micro level or some mechanisms through which these effects appear.

In Chapters 3 and 4, I study the opening of large new firms, a particular channel of demand shocks to the local labour market. At this level, there is some literature on aggregate changes to Total Factor Productivity (TFP) in the region, but less on changes to employment. Chapter 3 studies the effect of new firms opening on employment in the region and documents the composition of hires at these new firms.

Specifically, I study the Veneto region in Italy over the period 1985-2015. Using a unique administrative dataset, I am able to identify individual workers as well as their employers at a yearly level. I match this data to create a person-year (PY) panel of workers across firms in the region. The data also provide demographic information on workers as well some descriptive information on firms. I use this panel to identify the opening of new firms.

A major concern in the identification of new firms is that their presence may just be artefacts of data. For example, ownership changes and tax-status changes can trigger changes to underlying firm identifiers. To correct for this, others using similar administrative data have proposed a series of corrections based on worker flows to appropriately identify new firms. I impose a modified version of these constraints to my setting. At the end of this process, I identify roughly 200 new firms with over 200 workers in the year they open. These are relatively large shocks to their local labour markets.

On the set of firms identified as new firms, we find that they are less likely to hire women, have an older workforce, pay a higher wage but provide shorter job tenure compared to all firms in the region. In addition, I identify those firms that pay in the top quartile of wage premiums in the region. Those firms that are both new and high wage firms have an even older workforce, mechanically pay even more, but are otherwise similar.

In terms of hiring practices, we find that new firms mostly hire workers from within the region rather than from outside. Additionally, a non-trivial fraction of new hires comes from non-employment, including a large chunk of workers who are working for the first time. New firms that are also high wage firms have similar hiring patterns except for the fact fewer first time workers get jobs with them.

Further, I find weak evidence from regression estimates that new firms and new high wage firms do not poach their local employees from the labour force. Rather, the opening of a new high wage firm seems to explain a positive change in the hazard rate of reentering the labour force of about 1%. These high wage firms are more effective than new firms at raising this rate. Running the same regression on all firms suggest no effect.

To complement the analysis in Chapter 3, I also study the impact of new firms, but now specifically on wages in Chapter 4. This contributes to a large literature. There is already some weak evidence on the impact of new firms on factor prices, including wage. I find that workers moving to new firms are rewarded in terms of persistently higher wage premia, especially those moving to new high wage firms. However, running regressions similar to those already run in Chapter 3 find little effect on wages at the level of a province when a large new firm opens. While this is mechanically likely to happen, especially with new high wage firms, I use this evidence of wage agins for workers that move to investigate spillover effects of this wage gain on the broader population.

The literature on economics and social networks offering several competing theories on how social networks can affect both employment and wage. In my setting, between 15% to 20% of the workers hired by new firms had a previous colleague working there already. At the same time, workers that join new firms often leave behind many workers who do not get hired by those same firms, or any other new firms.

Event studies on the wages of ex-colleagues of those that move to new firms show steep and persistent declines in the years after their colleagues move. However, much of this driven by those workers who themselves do not find employment at new firms. Limiting myself only those who do tells a very different story. Ex-colleagues who eventually work for a new firm see their wages rise. This supports the view that new firms do not exert much competitve pressure on incumbents and also that social networks and referrals are an important source of information on new jobs and wage growth for workers.

Chapter 2

Understanding Civilian Deaths at Police Hands: Evidence from Crowdsourced Data[1]

2.1 Introduction

The federal government collects and publishes counts of officer involved civilian killings as part of the Uniform Crime Reports (UCR) Supplementary Homicides (SH) dataset. These data have been used extensively in reporting on police-civilian encounters by outlets such as FiveThirtyEight, the Washington Post and the New York Times.

We additionally collect data on police killings from a dataset maintained at `www.fatalencounters.org` (FE). The FE data are collected by a team of journalists. They are provided or find information on officer involved deaths, identify if the event actually took place, the police department involved as well as information about the victim. They also ensure that records are de-duplicated so that there is only one record per event. We further hand clean and verify the FE data.

It is important to note that determinations of "reasonableness" of use of force are subjective and we are unqualified to make decisions on a case by case basis for either FE or SH data. As a result, we pool all data on all killings for both datasets. However, as Figure 2.1 suggests, this may not be a substantial problem. We compare FE and SH data at the national, state and city levels. Nationally, we see that

[1]Jointly authored with Justin McCrary and Deepak Premkumar

the federally obtained series on police killings has remained flat or has slightly increased in the twenty years from 1995 to 2015. During that same period, the rate of violent crimes in USA plumetted from 700 to below 400. Alarmingly, for the period 2013 to 2015, FE data suggest that the true number of police killings is three times higher than the federal number.

At the state level, we find some possible explanations for the divergence in numbers between the FE and SH nationally. Large states like Florida and Alabama report no police killings through the SH database. Almost without exception, FE numbers are larger than their corresponding SH numbers. A quick glance at Figures 2.2 and 2.3 suggest that the underreporting in federal data is not simply a question of scale uniformly across all states.

Finally, we match each police department named to its ORI. We successfully match 90% of FE records to an ORI. Using this matched data, we investigate FE kills associated with an ORI to their SH counterparts. Again, we find further evidence that the discrepancy between the two sources is not uniform. SH data continuously misses smaller towns and cities especially in the south, as well as some large cities such as New York City.

Descriptively, we find that FE victims are overwhelmingly male, overwhelmingly killed by guns and mostly black but with over 20% of the data having unreliable race information. For this reason, we do not investigate questions of the racial makeup of the population of victims. Nonetheless, our results speak to the likely importance of race in these matters.

As we link our FE data to federal sources of data on police practices, we lose a large portion of our sample. In particular, we require that ORIs in our sample must be cities. This is due to the better quality of data from these jurisdictions as well as our higher confidence in the quality of the match between the kill and police department. However, this causes us to mechanically oversample ORIs with larger populations. Regardless of whether we use our limited sample, or the universe of all matched kills, we find that 10% of all police departments have killed a civilian in at least one of the years we observe data on them. Limiting ourselves to our more urban sample, that number rises to 60%, with 7% having at least one kill attributable to them in every year of our study.

We run poisson regressions of the total number of kills on departmental characteristics controlling for state level variation, city level demographics, department level crime and the racial and gender break up of the leadership of civilian and law enforcement units in the area. We find that the elasticity of kills to total population is roughly one, and that marginal changes to the size of the police force have little impact. Rather than the absolute number of police offers, it is their racial composition that matters significantly. A 10% increase in the fraction of sworn officers that are black reduces the number of kills in a city by 2. To give this context, the population weighted average number of kills by ORI is 4.6. Community policing initiatives, which receive a large amount of federal funding, do not seem to individually have much effect, but all point towards reducing the number of kills and may jointly have some power. The same is true for departmental choices of equipment and techniques, with the notable exception of the authorisation of neck restraints or "choke-holds" which stands out independently and significantly increasing the number of kills by a police department by almost 2.

The remainder of this paper is organised as follows. Section 2.2 lays out the differences between federal and crowdsourced data on kills. Section 2.3 provides detailed information on our crowdsource dataset. Section 2.4 provies information on the merge between FE data and our police department data as well as descriptive information on departmental practices. Section 2.6 provides regression estimates and Section 2.7 concludes.

2.2 Differences in Federal and Crowdsourced Data

As mentioned above, the official source for information on police killings is collected in the UCR SH file. The UCR SH file "provides detailed information on criminal homicides reported to the police". Data are either sent by police departments directly to the Federal Bureau of Investigation (FBI) or to their state UCR programs. The data are not perfect and some state UCR programs mandate the collection of this information. Unfortunately, it is not known from the UCR SH data themselves, which states do so and which do not. Among others fields, the UCR SH data count "justifiable homicides". In particular, they count justifiable homicides where a "felon is killed by police". We use these counts as the number of civilians killed by a police department.

These numbers are likely to be underestimates, not including those events where a police officer was not justified in such killings and many events where a determination is yet to be made. In addition, they likely suffer from systematic underreporting at both the state and PD level. Still, these are the only nationally available numbers from the government on police killings. More importantly, these are the numbers that city and police administrators often stand behind in official statements.

Figure 2.1 highlights some interesting trends. For the years in which we have overlapping coverage, we find that federal statistics undercount killings compared to crowdsourced data by a factor of three. Additionally, we see that the federal series is remarkably stable over time while the rate of violent crimes have plumetted drastically in the same time period.

Figures 2.3 and 2.2 attempt to decompose the discrepancy between the FE and SH sources highlighted in Figure 2.1. Only the District of Columbia and Maine have a federally reported number that is higher or equal to the crowsourced data. Including them, only fourteen states in the Union have a federal count that is less than twice the crowdsourced count. Alabama, Florida, Massachusetts, North Dakota and New Hampshire report no police killings over the three year period from 2013 to 2015, whereas crowdsourced data reports fifty eight times the number of federal killings in Mississippi and twenty six times the corresponding number in Arkansas.

We probe further, trying to identify discrepancies at the city level. Figures 2.4 and 2.5 show the killings at the city level for USA in the year 2013 according to crowdsourced and federal data respectively. The City of New York is notably absent, as it does not report any justifiable homicides during the period 2013-2015. Large cities seem to consistenly underreport, while smaller cities do not report at all especially in the south.

2.3 Descriptive Information From FE

We now turn to understanding the FE data better. There are other sources of information collected on the Internet about police killings. We choose FE for its completeness and the thoroughness of the team of journalists in ensuring that events actually took place. Additionally, the FE team claim to include credible information collected from other sources such as www.police-killings.net and the Washington Posts's dataset of police involved deaths. We do not use these other sources in our analysis and it is planned in future work.

Table 2.1 provides the state-wise breakdown of kills per year. Table 2.2 provides the age breakup of victims along different dimensions. Table 2.3 breaks down total kills by gender and cause. 36% of victims are white, while 23% are black. However,

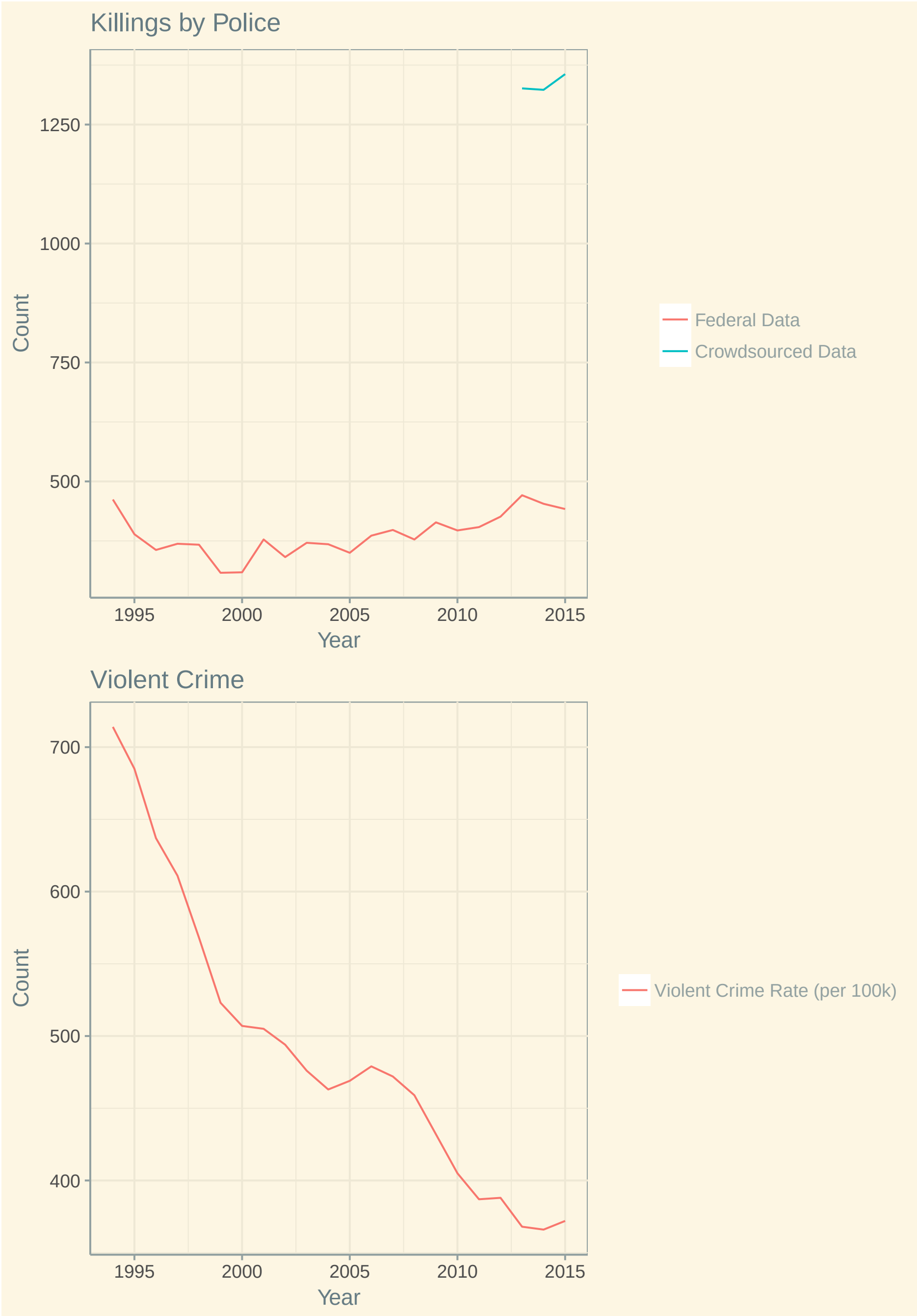

Figure 2.1: Police Killings vs Violent Crime in USA

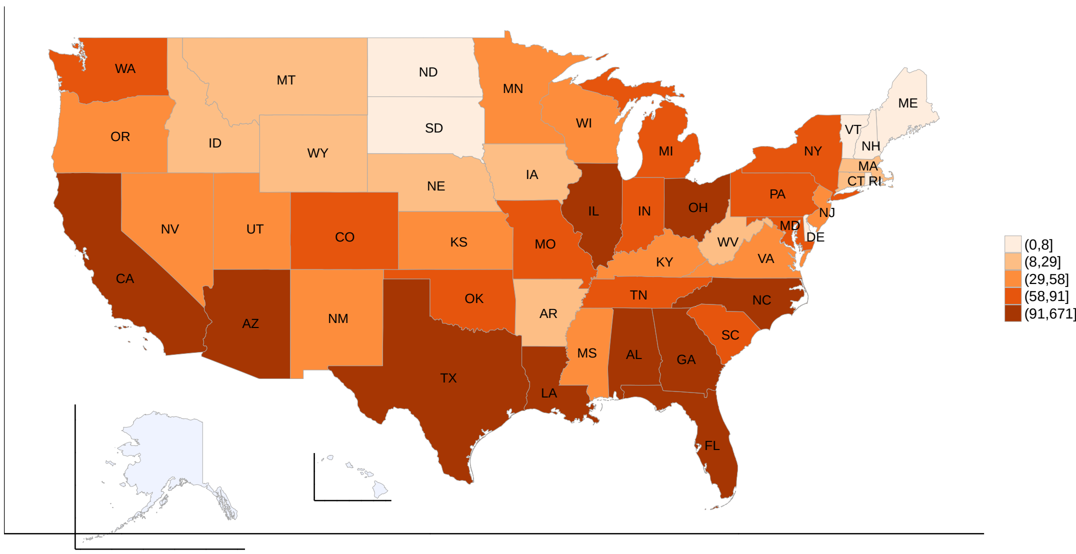

Figure 2.2: Statewise Kills FE

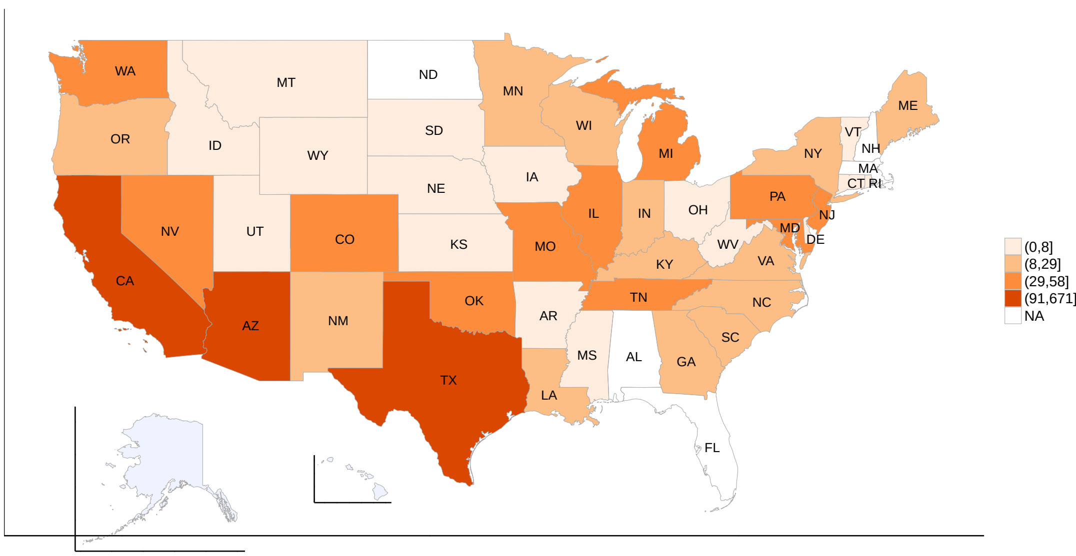

Figure 2.3: Statewise Kills SH

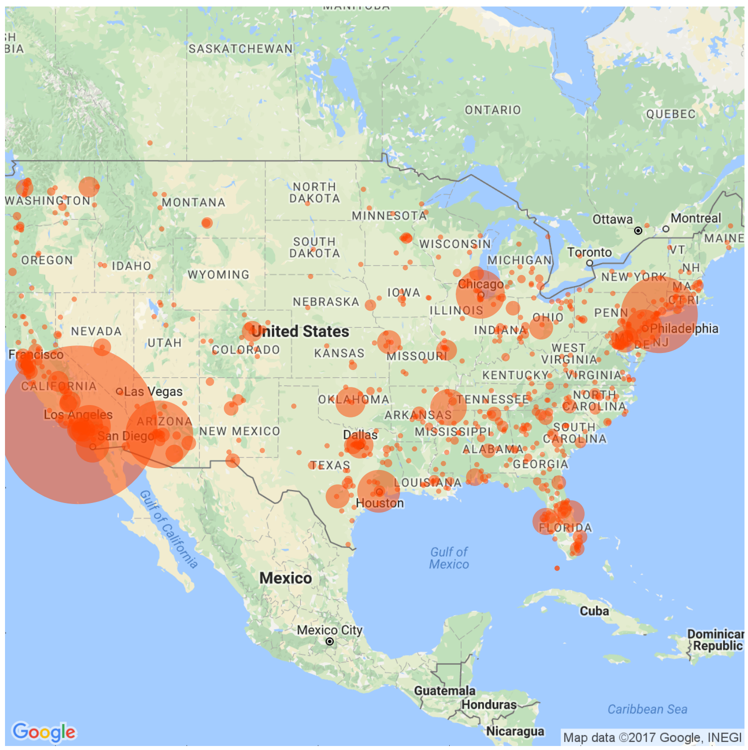

Figure 2.4: Citywise Kills FE

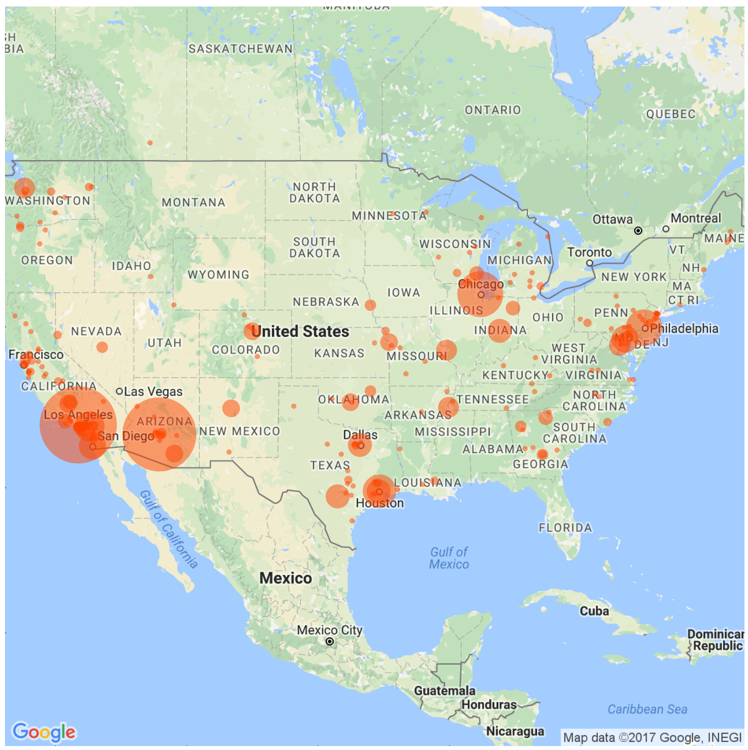

Figure 2.5: Citywise Kills SH

we have unreliable information on a full 23% of our population whose race is stored as missing. Almost all the victims are male, and white victims are much older than their black counterparts. Victims are most likely to die due to gunshot, especially males. We refrain from producing a racial breakdown like Table 2.3 due to the spottiness of our race data.

Table 2.1: Victims: States and Years

State	Year of Incident			
	2013 No.	**2014** No.	**2015** No.	**Total** No.
AL	39	26	31	96
AZ	56	43	42	141
AR	15	7	4	26
CA	241	195	235	671
CO	23	21	31	75
CT	11	4	4	19
DE	2	2	4	8
DC	5	3	5	13
FL	104	93	74	271
GA	34	30	40	104
ID	5	4	9	18
IL	30	38	25	93
IN	25	15	21	61
IA	9	12	6	27
KS	7	19	14	40
KY	5	15	11	31
LA	20	41	38	99
ME	4	1	3	8
MD	34	22	20	76
MA	10	6	13	29
MI	15	27	21	63
MN	17	13	14	44
MS	14	25	19	58
MO	25	28	21	74
MT	9	6	5	20
NE	5	7	9	21
NV	13	20	20	53
NH	2	0	3	5
NJ	14	16	25	55
NM	14	21	22	57
NY	37	27	25	89
NC	32	41	31	104
ND	2	2	1	5
OH	31	30	41	102
OK	21	32	38	91
OR	14	15	14	43
PA	32	13	16	61
RI	1	1	1	3
SC	19	20	24	63
SD	2	3	3	8
TN	24	26	24	74
TX	101	122	120	343
UT	5	16	11	32
VT	1	1	1	3
VA	18	13	20	51
WA	34	38	18	90
WV	6	4	6	16
WI	15	10	11	36
WY	1	3	7	11
AK	3	1	3	7
HI	3	3	2	8
Total	1209	1181	1206	3596

Source: Fatal Encounters (2013 – 2015)

Table 2.2: Victims: Gender, Age and Race

	Mean Age
Race	
1. White (36%)	39.0
2. Black (23%)	31.7
3. Hispanic (14%)	32.6
4. Other Race (2%)	34.2
5. Race Unknown (23%)	38.5
Total (100%)	36.1
Gender	
1. Female (7%)	37.6
2. Male (92%)	36.0
3. Unknown Gender (0%)	20.0
Total (100%)	36.1
Cause of death	
1. Gunshot (79%)	36.4
2. Vehicle (12%)	33.3
3. Taser (3%)	38.2
4. Other (3%)	38.3
5. Cause Unknown (0%)	37.4
Total (100%)	36.1

Source: Fatal Encounters (2013 – 2015)

Table 2.3: Victims: Gender and Cause

	Gender			
Cause of death	**1. Female**	**2. Male**	**3. Unknown Gender**	**Total**
1. Gunshot	165	3,021	1	3,187
2. Vehicle	126	379	3	508
3. Taser	3	130	0	133
4. Other	5	153	0	158
5. Cause Unknown	0	19	0	19
Total	299	3,702	4	4,005
1. Gunshot	5.2%	94.8%	0.0%	100.0%
2. Vehicle	24.8%	74.6%	0.6%	100.0%
3. Taser	2.3%	97.7%	0.0%	100.0%
4. Other	3.2%	96.8%	0.0%	100.0%
5. Cause Unknown	0.0%	100.0%	0.0%	100.0%
Total	7.5%	92.4%	0.1%	100.0%
1. Gunshot	55.2%	81.6%	25.0%	79.6%
2. Vehicle	42.1%	10.2%	75.0%	12.7%
3. Taser	1.0%	3.5%	0.0%	3.3%
4. Other	1.7%	4.1%	0.0%	3.9%
5. Cause Unknown	0.0%	0.5%	0.0%	0.5%
Total	100.0%	100.0%	100.0%	100.0%

Source: Fatal Encounters (2013 – 2015)

2.4 Linking Police Practices to Killings

2.4.1 Sources of Data on Police Practices

We draw on multiple sources of information on police departments. As mentioned earlier, the unique identifier for police departments is the ORI. The UCR Police Employment (PE) 2013 file contains the universe of all ORIs, and constitutes our backbone. We merge kills data from FE to their respective ORIs by hand. We also rely heavily on the FBI's Law Enforcement Management and Administration Survey

Table 2.4: Merge Statistics

Group	PDs	Population (m)	Kills
.	.	.	4,005
PE	22,206	317.3	3,596
PE, Cities Only	15,482	214.5	2,506
PE, LEMAS, Cities Only	1,925	122.2	1,740
PE, LEMAS, UCR OK, Cities Only	1,925	122.2	1,740
PE, LEMAS, UCR OK, Census, Cities Only	1,911	121.8	1,739
PE, LEMAS, UCR OK, Census, EEO4, Cities Only	1,232	115.3	1,692

(LEMAS) 2013. LEMAS 2013 contains information on personnel, hiring practices, community policing measures, and the use of force by police, among other things. Additionally, we merge in additional information about underlying crime trends at the ORI level using the UCR Offences Known (OK) 2013 database. Finally, we hand merge Census places to ORIs for demographic information at the ORI level and information on the racial composition of civilian and law enforcement administration from the Equal Employment Opportunity Commision's EEO4 (2013) survey. Importantly, we focus only on city police departments as kills attributable to county and state departments are harder to pin down geographically. Additionally, unliked the PE file, LEMAS '13 does not sample all police departments. It samples those departments employing more than 100 people and a randomly selected set of the others. Information on the merge, and loss of coverage are given in Table 2.4. Our preferred sample consists of 1,232 police departments. This amounts to studying over 50% of the population in cities in USA and roughly 40% of all recorded killings in FE. We are forced to make these tradeoffs due to data limitation issues.

Tables 2.5, 2.6 and 2.7 highlight the geographical skewness imposed by our sample selection. Our sample selection criteria make us oversample Pacific and West South Central states and undersample Middle Atlantic and East South Central states relative to the universe of police departments. In terms of the underlying political units, our sample strategy necessarily oversample cities, especially those with 100,000 inhabitants or less.

Table 2.5: Divisions

Division	All (Num)	All (Pct)	In Sample (Num)	In Sample (Pct)
Possessions	5	0.02	0	0
New England States	1,231	5.54	97	7.87
Middle Atlantic States	3,563	16.05	151	12.26
East North Central States	3,577	16.11	216	17.53
West North Central States	2,435	10.97	96	7.79
South Atlantic States	4,569	20.58	234	18.99
East South Central States	1,929	8.69	78	6.33
West South Central States	2,161	9.73	141	11.44
Mountain States	1,173	5.28	79	6.41
Pacific States	1,559	7.02	140	11.36
Total	22,202	100.00	1,232	100.00

Table 2.6: PD Groups

Group	All	In Sample
0 Posessions	5	0
1A Cities 1,000,000 or over	10	10
1B Cities from 500,000-999,999	25	24
1C Cities from 250,000-499,999	45	41
2 Cities from 100,000-249,000	219	176
3 Cities from 50,000-99,999	490	275
4 Cities from 25,000-49,999	907	250
5 Cities from 10,000-24,999	1,976	251
6 Cities from 2,500-9,999	4,369	168
7 Cities under 2,500	7,436	37
8A Non-MSA counties 100,000 or over	4	.
8B Non-MSA counties from 25,000-99,999	296	.
8C Non-MSA counties from 10,000-24,999	670	.
8D Non-MSA counties under 10,000	2,428	.
8E Non-MSA State Police	100	.
9A MSA counties 100,000 or over	171	.
9B MSA counties from 25,000-99,999	499	.
9C MSA counties from 10,000-24,999	289	.
9D MSA counties under 10,000	2,152	.
9E MSA State Police	111	.
Total	22,202	.

Table 2.7: PD Groups

Group	All	In Sample
0 Posessions	0.02%	0.00%
1A Cities 1,000,000 or over	0.05%	0.81%
1B Cities from 500,000-999,999	0.11%	1.95%
1C Cities from 250,000-499,999	0.20%	3.33%
2 Cities from 100,000-249,000	0.99%	14.29%
3 Cities from 50,000-99,999	2.21%	22.32%
4 Cities from 25,000-49,999	4.09%	20.29%
5 Cities from 10,000-24,999	8.90%	20.37%
6 Cities from 2,500-9,999	19.68%	13.64%
7 Cities under 2,500	33.49%	.
8A Non-MSA counties 100,000 or over	0.02%	.
8B Non-MSA counties from 25,000-99,999	1.33%	.
8C Non-MSA counties from 10,000-24,999	3.02%	.
8D Non-MSA counties under 10,000	10.94%	.
8E Non-MSA State Police	0.45%	.
9A MSA counties 100,000 or over	0.77%	.
9B MSA counties from 25,000-99,999	2.25%	.
9C MSA counties from 10,000-24,999	1.30%	.
9D MSA counties under 10,000	9.69%	.
9E MSA State Police	0.50%	.
Total	100.00%	100.00%

Table 2.8: Population Weighted Mean and SD of Kills per ORI

Variable	All	Sample
FE Kills	1.97	4.60
	(4.78)	(6.84)
SH Kills	0.97	2.30
	(2.66)	(3.89)

Table 2.9: Number of Years a Police Department Kills At Least One Civilian

Years of Killings	Number (All)	Percent (All)	Number (Sample)	Percent (Sample)
0	20,071	90.35%	504	40.91%
1	1,616	7.27%	443	35.96%
2	387	1.74%	198	16.07%
3	140	0.64%	87	7.06%
Total	22,214	100%	1,232	100%

2.5 Police Killings and Departmental Practices

Table 2.8 provides the population weighted number of kills per police department according to both federal and crowdsourced sources. Even at the level of the ORI, where we limit our sample necessarily to those FE kills for which we can find a clear ORI match as well as only city kills, FE kills are consistently higher than SH kills. Table 2.9 breaks down police departments based on the number of years they kill civilians. In terms of all police departments, one in every ten police departments in involved in at least one kiling of a civilian in the three year period of our study. In our sample of interest, six out of every ten police departments has an officer that killed at least one civilian and seven out of every hundred departments kill at least one civilian each year that we have data.

Finally Tables 2.10, 2.11, 2.12 provide means of variables of interest used in our regressions. Our sample, like the universe of police departments has relatively few women and sworn officers from racial and ehtnic minorities. In sample departments tend to be larger and better staffed, administratively. In sample departments are much more likely compared to the universe to employ a range of incentive pay programs to attract talent. In sample departments are far more likely to have community policing mandates written into their mission statments, and to provide their officers with community police training. Sampled departments are also more likely to authorise a broader range of equipment and practices for their officers to use, which is likely to be a consequence of their being richer departments as well. In terms of demographics, in sample departments are much more likely to police areas with more racial and ethnic minorities and higher crime.

2.6 Cross Sectional Regressions

We run a cross sectional poisson regression to relate our measure of total kills to departmental practices, while controlling for regional and demographic differences. In particular, we regress total kills on a set of LEMAS '13 variables with state fixed effects, place demographics, departmental level crime controls, and place and departmental level racial and gender breakup of top-level administration. We present either regression coefficients or margins derived from those coefficients as appropriate. All estimates can be interpreted as percentage changes except for dummy variables where estimates express the difference between 0 and 1. Estimates are presented in Tables 2.13 through 2.19. In few cases, it appears as if all possible values of a categorical variable are included. In those cases, the ommitted group is the level "missing". Columns 1 through 5 indicate varying levels of controls.

The elasticity of kills with repsect to population is roughly one, after controlling for which, a larger police force seems to make almost no difference. Women in leadership positions in both civilian and law enforcement administration seem to have large and negative, but statistically noisy effects on the total number of killings. Importantly, even the most restrictive set of controls bears out the clear trend that increasing the fraction of black sworn officers on the force by 10% can reduce the number of kills in a jurisdiction by two. This is a very large effect, bearing in mind the fact that the population weighted average number of kills per police department in our sample is 4.6.

Interestingly, having a written mission statement over none at all seems to have a large negative effect, but having a special component on community policing seems to have almost none. There is some mixed evidence that additional hours

Table 2.10: Descriptive Statistics

Variable	All	Sample
Personnel		
ln(Full Time Sworn Officers)	2.30	4.25
Fraction of Full Time Sworn Personnel		
Female	0.05	0.07
Black	0.03	0.07
Hispanic	0.05	0.07
Other Race	0.03	0.04
Supervisory	0.22	0.19
Patrol	0.55	0.50
Full Time Civilian Staff		
Per Sworn Officer	0.17	0.26
Pay and Benefits		
Pay Incentives		
Educational Achievement	0.33	0.56
Special Skills or		
Vocational Training	0.18	0.26
Bilingual or Multi-		
Lingual Ability	0.06	0.23
Special Duty Assignments	0.25	0.54
Hazardous Duty Assignments	0.03	0.16
Shift Differential	0.24	0.40
Residential	0.03	0.04
Merit	0.18	0.25
Unionized		

Table 2.11: Descriptive Statistics

Variable	All	Sample
Community Policing		
Mission Statement		
No	0.17	0.05
Yes, but No Community Policing Component	0.10	0.09
Yes, With Community Policing Component	0.68	0.83
8 Hours of Community Police Training for New Recruits		
NA	0.25	0.14
All	0.31	0.50
Half or More	0.03	0.04
Less than Half	0.07	0.09
8 Hours of Community Police Training for In-Service Officers		
NA	0.07	0.04
All	0.37	0.28
Half or More	0.08	0.12
Less than Half	0.14	0.23
Patrol Officers Engaged in SARA Type Problem Solving	0.31	0.53
Patrol Officers Evaluated on Collaborative Problem Solving Skills	0.28	0.42
Problem Solving Partnership With Local Organisation	0.31	0.50
Same Patrol Officers Assigned Beats	0.38	0.67

Table 2.12: Descriptive Statistics

Variable	All	Sample
Use of Force		
Techniques & Equipment Authorised		
Batons	0.86	0.92
Other Impact Weapons	0.41	0.50
Soft Projectiles	0.32	0.70
OC Spray	0.93	0.96
Other Chemical Agents	0.19	0.43
Tasers	0.79	0.84
Neck Restraints	0.15	0.22
Takedown Techniques	0.89	0.93
Open Hand Techniques	0.91	0.95
Closed Hand Techniques	0.84	0.90
Leg Hobbles	0.55	0.65
Miscellaneous		
Educational Requirement		
High school or lower	0.85	0.78
Some college or greater	0.12	0.17
ln(Budget)	12.11	14.63
Employees Furloughed	0.06	0.14
Census Controls		
Fraction Black	0.08	0.14
Fraction Hispanic	0.08	0.14
Fraction Other	0.03	0.06
EEO4 Controls		
Civilian Administration		
Fraction Black	·	0.04
Fraction Hispanic	·	0.02
Fraction Female	·	0.16
Police Administration		
Fraction Black	·	0.08
Fraction Hispanic	·	0.04
Fraction Female	·	0.11

Table 2.13: Regressions

Variable	(1)	(2)	(3)	(4)	(5)
Personnel					
ln(Full Time Sworn Officers)	-.1492	-.1215	-.5411	-.0848	-.1492
	(.1751)	(.1654)	(.1576)	(.1827)	(.1751)
Fraction of Full Time Sworn Personnel					
Female	-2.3900	-1.9370	-.3043	-.2461	.0384
	(1.4403)	(1.1553)	(1.0346)	(1.1139)	(1.2552)
Black	-.3716	-1.7865	-1.0924	-1.6976	-2.2243
	(.4057)	(.7083)	(.6483)	(.7216)	(.7400)
Hispanic	-.3671	-1.3192	-.2297	-.5604	-.7917
	(.3830)	(.6258)	(.5775)	(.5940)	(.6222)
Other Race	-.1819	-.4257	-.4264	-.2582	-.2372
	(.2988)	(.3027)	(.2777)	(.2597)	(.2718)
Demographic Controls	No	Yes	Yes	Yes	Yes
Crime Controls	No	No	Yes	Yes	Yes
State FEs	No	No	No	Yes	Yes
EEO4 Controls	No	No	No	No	Yes

of community policing training may have some effect in reducing the number of kills. Popular programs such as SARA type problem solving skills and assigning patrol officers to specific beats all seem to point in the “right” direction, but we are underpowered to pick up statistically significant effects.

In terms of equipment and techniques authorised for use by sworn personnel, it is not surprising that those departments that authorise potentially deadly equipment ranging from batons to soft projectile weapons may be more likely to kill civilians than those that do not. While most effects are not significant, the use of neck restraints or so-called “chokeholds” are statistically significant. While EEO4 variables control for the racial makeup of civilian and law enforcement managment, only the fraction of female supervisory staff in the police force appears to be negative and statistically significant.

Table 2.14: Regressions

Variable	(1)	(2)	(3)	(4)	(5)
Personnel					
Supervisory	-4.7958	-3.6928	-2.7449	-2.9395	-3.8028
	(1.3815)	(1.2276)	(1.1139)	(1.0029)	(1.0339)
Patrol	-1.0215	-1.1171	-.3271	-.0218	-.1881
	(.4222)	(.3749)	(.3725)	(.4398)	(.4412)
Investigative	1.6253	.8800	1.0525	1.2195	1.7101
	(.6872)	(.6505)	(.6431)	(.7296)	(.7245)
Full Time Civilian Staff Per Sworn Officer	-.2135	.3379	-.4113	.0961	.1666
	(.3575)	(.3362)	(.3155)	(.3265)	(.2984)
Female Police Chief	-.3861	-.3598	-.2775	-.0956	-.1408
	(.2150)	(.1935)	(.1695)	(.1944)	(.1905)
Demographic Controls	No	Yes	Yes	Yes	Yes
Crime Controls	No	No	Yes	Yes	Yes
State FEs	No	No	No	Yes	Yes
EEO4 Controls	No	No	No	No	Yes

Table 2.15: Regressions

Variable	(1)	(2)	(3)	(4)	(5)
Pay and Benefits					
Pay Incentives					
Educational Achievement	.8950	-.2564	-.6582	-1.4900	-1.2215
	(1.1129)	(.9650)	(.8222)	(.9743)	(1.0109)
Special Skills or Vocational Training	-1.8348	-3.5832	-2.1355	-1.0542	-1.3306
	(1.2229)	(1.0525)	(.9197)	(.9057)	(.9790)
Bilingual or Multi-Lingual Ability	3.4350	3.3036	3.2438	1.1325	1.1146
	(1.2927)	(1.2183)	(1.0941)	(1.2021)	(1.2436)
Special Duty Assignments	1.7656	2.4985	.6209	-.4733	-.6201
	(1.1296)	(.9161)	(.9196)	(1.1043)	(1.1603)
Hazardous Duty Assignments	1.9961	1.3497	.6266	-1.2612	-.7413
	(1.0546)	(.9377)	(.8567)	(.8693)	(.8195)
Shift Differential	.6681	1.0052	-.0077	.9515	1.1010
	(.9543)	(.9069)	(.8199)	(.9774)	(.9734)
Residential	-.3229	4.5924	4.0461	.4853	.2565
	(2.5493)	(2.4077)	(1.8782)	(1.6376)	(1.5333)
Merit	-.6427	.4440	-.5062	-.4451	-.5942
	(.8480)	(.8151)	(.7494)	(.7937)	(.7861)
Demographic Controls	No	Yes	Yes	Yes	Yes
Crime Controls	No	No	Yes	Yes	Yes
State FEs	No	No	No	Yes	Yes
EEO4 Controls	No	No	No	No	Yes

Table 2.16: Regressions

Variable	(1)	(2)	(3)	(4)	(5)
Pay and Benefits					
Collective Bargaining Agreement					
None	5.2703	2.8063	1.4004	1.4449	1.2763
	(2.7764)	(2.2038)	(1.9078)	(2.0193)	(2.0568)
Expired	-.5453	.4792	.6386	1.7189	1.7010
	(1.7803)	(1.5850)	(1.6201)	(1.6775)	(1.7258)
Active	3.1828	3.9768	2.3353	1.2643	1.8681
	(1.7088)	(1.5505)	(1.4916)	(1.7549)	(1.7894)
Demographic Controls	No	Yes	Yes	Yes	Yes
Crime Controls	No	No	Yes	Yes	Yes
State FEs	No	No	No	Yes	Yes
EEO4 Controls	No	No	No	No	Yes

2.7 Conclusion

This Chapter finds and documents a series of compelling facts on police killings. Additionally, it provides some evidence on the possible mechanisms and policy levers that drive these acts. First, and most importantly, it highlights and important discrepancy between federal statistics and the true level of violence. While we are unable to accurately assess the true level of killings, we adduce a large mass of evidence on governmental underreporting. We do this by first highlighting the patterns of missing data within the federal data themselves as well as discrepancies compared data collected from the Internet. Further research on this undoubtedly requires greater funding and mechanisms in place to collect accurate information on civilian deaths at the hands of police officers.

Second, we find some interesting patterns between reported uses of deadly force and police departmental practices. Regression estimates indicate strong relationships between racial composition, authorised tactics and the number of kills by a police department. We are limited in our analysis due again to shortcomings in the data. The LEMAS survey, which was supposed to a panel survey to be conducted on a regular basis has often been missed, apparently due to cuts in federal funding. Rather, LEMAS needs to be conducted as often as planned, if not more

Table 2.17: Regressions

Variable	(1)	(2)	(3)	(4)	(5)
Community Policing					
Mission Statement					
No	.	.	.	.	.
Yes, but No Community Policing Component	-7.9296	-9.4002	-8.5445	-3.3274	-4.6792
	(3.7444)	(3.6820)	(3.1759)	(2.5197)	(2.7254)
Yes, With Community Policing Component	-3.6359	-4.6413	-5.2145	.6391	-.0554
	(3.3898)	(3.4044)	(2.8429)	(2.2082)	(2.4038)
8 Hours of Community Police Training for New Recruits					
NA	2.6243	2.5901	3.3252	2.9665	2.8244
	(2.1856)	(2.0535)	(1.7766)	(1.9253)	(1.9020)
All	1.8056	1.5865	2.6122	1.5198	1.7005
	(1.7489)	(1.8156)	(1.5919)	(1.5890)	(1.5498)
Half or More	2.5795	6.2031	5.4894	.0435	1.2444
	(2.4402)	(2.5679)	(2.4629)	(3.1718)	(3.1590)
Less than Half	-1.0851	.5354	3.9178	5.1029	5.0091
	(3.1638)	(2.7678)	(2.3072)	(1.9671)	(2.1121)
Demographic Controls	No	Yes	Yes	Yes	Yes
Crime Controls	No	No	Yes	Yes	Yes
State FEs	No	No	No	Yes	Yes
EEO4 Controls	No	No	No	No	Yes

Table 2.18: Regressions

Variable	(1)	(2)	(3)	(4)	(5)
Community Policing					
8 Hours of Community Police Training for In-Service Officers					
NA	-5.1899	-3.0068	3.2263	5.8144	5.3888
	(4.0094)	(3.6033)	(2.8549)	(2.6023)	(2.7862)
All	.4859	.8961	1.3549	1.6845	2.1503
	(1.2047)	(1.1904)	(1.0594)	(1.1701)	(1.1708)
Half or More	-1.0092	.2551	2.7489	2.5670	1.6934
	(1.5419)	(1.5349)	(1.3852)	(1.4424)	(1.5229)
Less than Half	-.8296	-1.4587	-.3224	-.7392	-.6826
	(1.4357)	(1.3156)	(1.1928)	(1.2713)	(1.2293)
Demographic Controls	No	Yes	Yes	Yes	Yes
Crime Controls	No	No	Yes	Yes	Yes
State FEs	No	No	No	Yes	Yes
EEO4 Controls	No	No	No	No	Yes

Table 2.19: Regressions

Variable	(1)	(2)	(3)	(4)	(5)
Community Policing					
Patrol Officers Engaged in					
SARA Type Problem Solving	-1.5650	-.9489	-1.7897	-1.9804	-1.9491
	(.9439)	(.9710)	(.8231)	1.0500	(1.1435)
Patrol Officers Evaluated on					
Collaborative Problem Solving					
Skills	-1.9689	-1.9735	-1.3811	-.8494	-1.0821
	(1.1532)	(1.0205)	(.9447)	(.9681)	(.9652)
Problem Solving Partnership					
With Local Organisation	-1.5823	-1.8740	-1.3975	-.2458	-.4624
	(1.4908)	(1.1800)	(1.0139)	(.8853)	(.9337)
Same Patrol Officers Assigned					
Beats	-.4548	-.9178	-.1161	-.7961	-2.0839
	(1.7640)	(1.4824)	(1.3980)	(1.4979)	(1.5353)
Demographic Controls	No	Yes	Yes	Yes	Yes
Crime Controls	No	No	Yes	Yes	Yes
State FEs	No	No	No	Yes	Yes
EEO4 Controls	No	No	No	No	Yes

Table 2.20: Regressions

Variable	(1)	(2)	(3)	(4)	(5)
Use of Force					
Techniques & Equipment Authorised					
Batons	5.6021	4.9282	5.8920	2.6433	2.8962
	(2.5082)	(2.3799)	(2.4621)	(2.1982)	(2.2578)
Other Impact Weapons	1.9347	1.7770	1.9435	1.1347	1.2605
	(1.1765)	(.8963)	(.7813)	(.8149)	(.8095)
Soft Projectiles	1.5258	.9978	1.4540	1.0547	1.7136
	(1.6665)	(1.7195)	(1.5832)	(1.8435)	(1.8969)
OC Spray	-9.4615	-11.3358	-8.1145	1.8008	1.2052
	(2.2687)	(2.1628)	(1.9195)	(2.3093)	(2.4443)
Other Chemical Agents	-2.6757	-1.7517	-.9951	.0740	.1760
	(1.1740)	(1.0732)	(.9711)	(.8691)	(.8590)
Tasers	7.6625	1.5579	3.2958	.2245	-.1481
	(2.0501)	(1.9804)	(2.0267)	(1.9545)	(2.0130)
Demographic Controls	No	Yes	Yes	Yes	Yes
Crime Controls	No	No	Yes	Yes	Yes
State FEs	No	No	No	Yes	Yes
EEO4 Controls	No	No	No	No	Yes

Table 2.21: Regressions

Variable	(1)	(2)	(3)	(4)	(5)
Use of Force					
Neck Restraints	1.5444	1.9417	2.0007	1.5771	1.9628
	(1.2513)	(1.1254)	(1.0757)	(.9857)	(.9261)
Takedown Techniques	4.8423	5.8593	2.2748	-.5723	-.1459
	(3.2546)	(3.2195)	(3.1239)	(2.7581)	(3.1332)
Open Hand Techniques	3.9858	2.5089	.9338	-1.5905	-.0785
	(2.2623)	(2.0759)	(2.0431)	(2.5880)	(2.7183)
Closed Hand Techniques	-3.0152	-3.7984	-3.2863	-1.7069	-1.9836
	(1.8438)	(1.5938)	(1.3826)	(1.9351)	(2.1205)
Leg Hobbles	-1.4759	-.8282	.0946	.4455	.0691
	(1.4138)	(1.0705)	(.9003)	(.8775)	(.9034)
Demographic Controls	No	Yes	Yes	Yes	Yes
Crime Controls	No	No	Yes	Yes	Yes
State FEs	No	No	No	Yes	Yes
EEO4 Controls	No	No	No	No	Yes

Table 2.22: Regressions

Variable	(1)	(2)	(3)	(4)	(5)
Miscellaneous					
Educational Requirement					
High school or lower	.	.	.	.	.
Some college or greater	-1.1365	-.1885	.8904	2.4839	2.8791
	(1.1560)	(1.0013)	(.8670)	(1.0165)	(1.0167)
ln(Budget)	.4368	.1665	.3843	.1514	.1490
	(.1394)	(.1480)	(.1409)	(.1491)	(.1489)
Employees Furloughed	3.0356	2.5285	2.0244	-.0027	.6900
	1.2901	(1.1760)	(1.0972)	(1.0526)	(1.0021)
Demographic Controls	No	Yes	Yes	Yes	Yes
Crime Controls	No	No	Yes	Yes	Yes
State FEs	No	No	No	Yes	Yes
EEO4 Controls	No	No	No	No	Yes

often. Additionally, LEMAS has seen cuts to survey questions. Previous versions were far more detailed and research on this topic can only benefit from the richness of earlier survey designs. Finally, LEMAS itself needs to expand its scope to cover more departments. From our descriptive information, it is clear that a large fraction of killings happen in small cities and towns. Understanding the dynamics in those jurisdictions will only help in our understanding of the determinants of police violence.

Table 2.23: Regressions

Variable	(1)	(2)	(3)	(4)	(5)
<u>Census Controls</u>					
Fraction Black	·	2.1989	-.0761	.8513	1.2166
	·	(.5875)	(.5476)	(.5622)	(.6275)
Fraction Hispanic	·	.8426	.0278	1.2161	1.7095
	·	(.4639)	(.4551)	(.4844)	(.5010)
Fraction Other	·	-.9707	-.3911	.3836	.2883
	·	(.6398)	(.6617)	(.7190)	(.7805)
ln(Population)	·	.9633	1.1216	.9362	.9550
	·	(.1542)	(.1015)	(.1622)	(.1383)
Demographic Controls	No	Yes	Yes	Yes	Yes
Crime Controls	No	No	Yes	Yes	Yes
State FEs	No	No	No	Yes	Yes
EEO4 Controls	No	No	No	No	Yes

Table 2.24: Regressions

Variable	(1)	(2)	(3)	(4)	(5)
EEO4 Controls					
Civilian Administration					
Fraction Black	·	·	·	·	8.2396
	·	·	·	·	(4.7326)
Fraction Hispanic	·	·	·	·	10.1873
	·	·	·	·	(5.3805)
Fraction Female	·	·	·	·	-3.1532
	·	·	·	·	(2.0278)
Police Administration					
Fraction Black	·	·	·	·	-1.3514
	·	·	·	·	(2.3364)
Fraction Hispanic	·	·	·	·	4.6302
	·	·	·	·	(3.1701)
Fraction Female	·	·	·	·	-4.0265
	·	·	·	·	(1.9751)
Crime Controls					
ln(CM Index)	·	·	.6781	.5361	.5426
	·	·	(.0629)	(.0676)	(.0720)
Demographic Controls	No	Yes	Yes	Yes	Yes
Crime Controls	No	No	Yes	Yes	Yes
State FEs	No	No	No	Yes	Yes
EEO4 Controls	No	No	No	No	Yes

Chapter 3

New Facts About New Firms

3.1 Introduction

The simplest and most common spatial model of labour markets can be found in Roback (1982). Imporantly, the model assumes perfect mobility of capital and labour as well as perfect information among agents. It generates the prediction that any difference in wages across labour markets will be arbitraged away by the free movement of labour. Most importantly, local demand shocks will not affect local labour market outcomes in this model as any improvements will be immediately nullified by migration, with the only adjustment being along the margin of the cost of land.

Despite the implications of the Roback Model, local policymakers spend considerable sums of taxpayer money on subsidies such as free land and tax breaks to entice firms to set up manufacturing facilities in their areas. Some argue that this is completely to take advantage of "agglomeration externalities" (Greenstone, Hornbeck, and Moretti 2010). Indeed, Greenstone et al. (2010) find evidence that there are significant spillovers in terms of Total Factor Productivity (TFP) of a large new firm on incumbents in the region. Moreover, they argue that these spillovers are through industry linkages rather than direct geographic proximity. As a consequence, they claim that factor prices (including the cost of labour) increase to adjust for these spillovers.

Others, however, show that local politicans consider plans to incentivise firms to locate in their regions specifically to create jobs (Molotch 1976 and Bartik 2015). A large literature has focused on trying to investigate this objective, most notably

Bartik (1991) and Blanchard and Katz (1992). Theoretically, even monospsonistic firms should benefit some workers if the labour demand curve is upward sloping. The extent to which local workers will benefit will depend on the labour supply curve to new firms (Glaeser 2001).[1]

There is consensus on the short-run effects of shocks to local employment, finding an increase in both wages and employment rates for locals, but disagreement on the longer-run effects. Bartik (1993) argues that long run effects are produced through hysterisis effects – that local residents that are employed in the short-run learn on the job and can signal more effectively to employers even in the future. On the other hand, Blanchard and Katz (1992) argue that in-migration causes these gains to disappear in within six years.

In this paper, I attempt to get at some of the micro-foundations of the literature on local demand shocks. In particular, I study one particular kind of local demand shock viz. the opening of large new firms in Italy. I investigate the hiring practices of these new firms and the extent to which the local labour force benefits directly from job creation by them. Like Greenstone et al. (2010), I know the identity of large new firms opening in the region. In addition, I have administrative data at the worker-firm-year level which allows me to learn exactly to whom employment benefits go.

Unlike Greenstone et al. (2010) however, I do not know anything about the firm's decision making and consequently do not select a compelling counterfactual region. The determinants are likely to include costs of factors of production, quality of the workforce, and the presence of other firms. While acknowledging the endogeneity issues of a particular province being selected for the location of a new firm, this Chapter still provides some useful descriptive facts on their effect on a region.

At the firm level, I see large hirings in the first two years to get a facility up and running, followed by replacement hiring to fill vacancies in subsequent years. These new firms are large and pay slightly better than existing jobs in the region. In the first years of operation, workers are hired predominantly from the province the firm sets up in. As years go by, a larger fraction of hires go to migrants as is predicted by models of spatial economics. At the regional level, I find results that are consistent with the short run findings of Bartik (1991) and Blanchard and Katz (1992) and some weak evidence in favour of Blanchard and Katz (1992) in the

[1] Glaeser (2001) also suggests other theories motivated by fiscal considerations for why local governements may choose to incentivise firms, but I do not focus on them here.

medium to long run.

On the macroeconomic side, there is a connected literature that focuses aggregate employment growth. For example, many economists were puzzled by the slow growth in jobs as the US economy emerged from the recession (Beaudry, Green, and Sand 2013). Haltiwanger, Hyatt, and McEntarfer (2015) find evidence that workers moving from one job to the next usually experience modest wage gains, and that the movement of workers is very procyclical. They further argue that this "job ladder" broke down in the most recent recession. Importantly, they find that hiring practices of high and low wage firms differ during contractions, with lower firms hiring more out of non-employment in these periods. The economic conditions in Italy between 1985 and 2001, the period of my study, were not characterised by a downturn. However, I find in my case that a small and non-signficant portion of hirings to new firms come from non-employment and first time workers. Regression estimates suggest that one large new firm can have a temporary effect on non-employment and first time hires of almost 1%. New firms may provide a way to reconnect the firm-wage job ladder during recessions.

The remainder of the Chapter is organised as follows. Section 3.2 provides information on Vento, the region of Italy I study as well as a brief overview of the data used. Section 3.3 provides more detailed information on the new firms I study. Section 3.4 looks at the hiring practices at these firms and Section 3.5 concludes.

3.2 Data and Background

Veneto is an administrative region in the North East of Italy with roughly 5 million inhabitants making it the 8th most populous of the 20 administrative regions of Italy. The region has seven provinces, namely Belluno, Padua, Rovigo, Treviso, Venice, Verona and Vicenza. Since the 1970s, Veneto has been growing into a strong industrial hub. The region is the third richest in Italy today.

My primary source of data is the Veneto Workers History dataset (VWH). The VWH contains information on all private sector personnel in Veneto. The data have complete worker histories for every worker in the region and I use the data from the period between 1985 and 2001. During this period, Veneto had nearly full employment, a positive rate of job creation in manufacturing and positive migration flows (Tattara and Valentini 2010 and Serafinelli 2017).

The VWH data contain three separate sources of information. The first contains

Figure 3.1: Veneto. CC BY-SA 3.0, `https://commons.wikimedia.org/w/index.php?curid=303866`

Table 3.1: Veneto – PY-weighted Province Composition

Province	Number	Percent	Cumulative Percent
BL	658,873	4.42	4.42
PD	2,774,152	18.63	23.05
RO	570,220	3.83	26.88
TV	2,655,233	17.83	44.71
VE	2,505,686	16.83	61.54
VI	3,110,053	20.88	82.42
VR	2,617,774	17.58	100.00
Total	14,891,991	100.00	.

Table 3.2: Veneto – PY-weighted Sector Composition

Sector	Number	Percent	Cumulative Percent
Production	7,537,242	50.61	50.61
Handicrafts	3,044,366	20.44	71.06
Agriculture	48,736	0.33	71.38
Banking and Insurance	415,379	2.79	74.17
Services	3,846,268	25.83	100.00
Total	14,891,991	100.00	.

information on all firms in Veneto. The second contains information on workers who have ever worked in Veneto and the third contains information on all job spells for each worker in the worker file. Importantly, the third file has complete worker histories including spells outside Veneto. Using these three sources, I construct a matched worker-firm yearly panel from the VWH data. For details on the panel's construction, refer to Appendix A.

My final dataset contains 19.6 million person-year observations. By construction, 75% of the observations are from Veneto as many workers never leave the region. Of that 75%, the province wise distribution is given in Table 3.1. For the most part, the production sector is the largest employer followed by the services sector. The PY weighted sectoral composition of workers can be found in Table 3.2.

Table 3.3: Worker Characteristics Across Firms

Variable	All Firms	HWFs	New Firms	New HWFs
Fraction Female	.3411	.2935	.2414	.2439
	(.4740)	(.4554)	(.4279)	(.4294)
Age	34.61	35.30	37.50	38.01
	(10.58)	(10.70)	(10.20)	(10.61)
Experience	5.88	5.85	8.30	7.77
	(4.59)	(4.57)	(4.48)	(4.41)
ln(Daily Wage)	4.75	4.90	4.93	5.00
	(.4275)	(.4125)	(.3748)	(.3618)
Move Premium	.0101	.1548	.0582	.1200
	(.2866)	(.2616)	(.1984)	(.1971)
Job Tenure	4.16	4.58	3.82	4.22
	(3.58)	(3.80)	(3.01)	(3.21)

3.3 New Firms and New Workers

I define New Firms to be firms in the production sector that employ at least 100 workers in the year of their starting. To ensure that these are genuinely new firms, and not artifacts of the data, I analyse worker flows similar to Hethey-Maier and Schmieder (2013) and impose corrections to firm identifiers so that my final dataset contains firm identifiers that uniquely identify firms. More information on the identification of New Firms can be found in Appendix B. In total, I identify 211 New Firms in my sample period. Additionally, I identify High Wage Firms (HWFs) as those firms which are in the top quartile of the wage gain distribution for job switchers. By construction, some New Firms are also HWFs. See Table 3.3 for detailed information on the set of workers in all firms, New Firms, HWFs and New HWFs.

HWFs hire fewer women than all firms while New Firms and New HWFs hire even fewer. HWFs hire slightly older workers than all firms. New Firms hire slightly older workers than HWFs while New HWFs hire even older workers. New Firms and New HWFs hire workers with more experience than all firms and all HWFs. While we expect HWFs to pay more than all firms, New Firms pay more

Table 3.4: Births and Deaths of New Firms

Year	Firm Births	Firm Deaths
1985	8	2
1986	6	2
1987	10	1
1988	8	0
1989	15	1
1990	27	2
1991	10	3
1992	9	6
1993	10	10
1994	7	8
1995	10	1
1996	13	10
1997	13	5
1998	17	8
1999	24	12
2000	9	5
2001	15	5

than all HWFs while New HWFs pay the highest overall. However, New Firms only give workers a 5% wage premium on hiring, meaning that they draw from a higher wage pool of workers. HWFs and New HWFs give workers a significant wage premium of over 10% while regular firms pay workers a negligibly small premium.

On average, there are approximately 12 New Firms that come into existence every year and 5 New Firms that die each year. See Table 3.4 for a detailed breakdown of the yearly birth and death rates and Table 3.5 for their surival rates. At the end of my sample period, there are 130 New Firms still surviving.

Geographically, these New Firms are concentrated mostly in the provices of Padova, Verona and Vicenza which also account for the lion's share of the region's population. This trend is even pronounced for New HWFs. See Tables 3.6 and 3.7 for the raw and PY weighted density of firms across provinces.

Table 3.5: Survival Rates of New Firms

Year	Survival	Survival Fraction
0	211	1.00
1	181	0.86
2	167	0.79
3	136	0.64
4	105	0.50
5	90	0.43
6	80	0.38
7	68	0.32
8	60	0.28
9	54	0.26
10	47	0.22
11	40	0.19
12	24	0.11
13	13	0.06
14	10	0.05
15	5	0.02
16	3	0.01

Table 3.6: Geographic Spread of New Firms

Province	Raw Count	PY-weighted Count
BL	3	1,710
PD	44	77,607
RO	6	4,798
TV	29	29,391
VE	17	24,010
VI	79	132,445
VR	33	53,299
Total	211	323,260

Table 3.7: Geographic Spread of New HWFs

Province	Raw Count	PY-weighted Count
BL	0	0
PD	17	38,516
RO	2	305
TV	4	4,190
VE	8	4,582
VI	17	35,480
VR	11	15,107
Total	59	98,180

3.4 Hiring Practices of New Firms

New Firms are chosen by construction to be those that hire at least 100 workers in their first year of operations. Consequently, this inflates the average number of workers working in New Firms. They have an average first year employment of 209.32 and a median first year employment of 174. The smallest firms are censored at 100 but the largest 1% of firms have more than 900 workers in their first year.

New Firms' year-by-year average hiring is given in Table 3.8. Table 3.8 also shows the fraction of workers joining the firm in that year who migrated provinces for their job, who join the New Firm after a spell of non-employment and who join the New Firm as their first job respectively.

An open question about the welfare impacts of New Firms is the extent to which they really bring employment to the region. It could be that they bring their workers with them, providing relatively few job opportunities to the local region they operate in. Conversely, it could be that New Firms hire a majority of their workers from the region, contributing to the net number of jobs that workers from the region can be hired for. Table 3.8 suggests that the fraction of migrants from other provinces is not that large and that New Firms actually hire workers from within the provinces they operate. Restricting the definition of migrants to people who move across regions and not provinces makes the fraction of migrants almost negligible.

A further question to be asked is whether, conditional on hiring from the local

Table 3.8: Characteristics of Hires at New Firms

Year	New Workers	% Migrant	% Non-Employed	% First Job
0	209.32	16.82	04.09	14.15
1	85.28	27.15	07.38	19.64
2	30.34	31.88	08.76	28.93
3	28.99	30.15	08.07	27.70
4	25.97	33.33	10.02	27.59
5	25.98	29.34	08.69	26.58
6	27.97	32.14	08.09	33.84
7	29.91	31.17	09.66	26.30
8	32.65	35.83	08.96	27.23
9	33.09	41.25	07.91	26.75
10	41.78	36.89	09.06	23.47

labour market, these New Firms are simply hiring workers who are already employed at other establishments or whether they are creating *new* jobs. Table 3.8 suggests that a non-trivial fraction of their workforce does not come from a previous job. Roughly 14% of all new workers in the first year of hiring are new to the labour force. An additional 4% are members of the labour force who had no job in the immediately previous year. Cumulatively, this accounts for almost 20% of the labour force in New Firms being coming from non job-to-job transitions in their first year of operations.

The same questions can be asked of another set of firms, viz. New HWFs. Descriptive statistics on hiring at New HWFs can be found in Table 3.9. Notably, New HWFs start smaller but hire longer than New Firms. Their propensity to hire migrants is roughly the same as New Firms as is their likelihood to hire out of non-employment. However, they differ quite substantially from New Firms in their hiring of first time workers, hiring at a rate of roughly two-thirds the New Firm level. This is consistent with Haltiwanger et al. (2015).

The numbers in Tables 3.8 and 3.9 may be an underestimate of the total impact of New Firms on hires from non-employment in a region as they may also induce further hiring among the Incumbent Firms they poach workers from. I try test whether these New Firms are able to move the needle in terms of the hazard rate

Table 3.9: Characteristics of Hires at New HWFs

Year	New Workers	% Migrant	% Non-Employed	% First Job
0	178.13	17.65	05.39	10.94
1	114.83	20.08	04.83	14.16
2	28.63	28.85	08.00	23.21
3	32.22	26.55	07.03	26.03
4	29.74	33.04	08.77	31.82
5	32.66	30.30	07.08	23.76
6	36.59	30.53	07.23	26.29
7	37	32.42	05.16	21.06
8	37.18	39.98	08.66	24.87
9	40.8	40.56	08.05	32.96
10	35.13	31.07	11.32	21.44

for people to transition back into or join the labour force for the first time.

In particular, I run cross sectional regressions of the form:

$$Y_{kt} = \alpha + \beta \# Firms_{kt} + \gamma_k + \tau_t + \epsilon_{kt} \tag{3.1}$$

Where Y is the logged number of reentries into the labour force as well as the logged number of first time entries into the labour force in province k and year t. The variable of interest is $\#Firms_{kt}$, which is either the number of New Firms that come into existence in year t and province p or the total number of New Firms in year t and province p. I also run the regression specified by Equation 3.1 on the number of New HWFs as well as all firms (new and old, high wage and low wage) to provide some contrast. γ_k and τ_t are province and year level fixed effects respectively. The results of these regressions are captured in Table 3.10.

Table 3.10 summarizes the results of these different specifications. Coefficients are to be interpreted as the percent change in rates for every additional firm. That is, each New HWF increases the rate at which non-employed workers rejoin the labour force by a little over 1%. These results tell some potentially interesting stories.

First, consistent with the descriptive fact that a New Firm's biggest impact on its local labour market is in its first year, estimates that pool all firms in a year are washed out relative to estimates of just the number of firms that commenced opera-

Table 3.10: Regression Estimates

Dependent Variable	First Year	All
Hires from Non-Employment (New HWFs)	0.0131	0.0045
	(0.0060)	(0.0075)
First Time Hires (New HWFs)	0.0107	0.0085
	(0.0075)	(0.0097)
Hires from Non-Employment (New Firms)	0.0042	0.0006
	(0.0033)	(0.0022)
First Time Hires (New Firms)	0.0057	0.0024
	(0.0038)	(0.0028)
Hires from Non-Employment (All Firms)	·	0.0000
	·	(0.0000)
First Time Hires (All Firms)	·	0.0000
	·	(0.0000)
N		

tions in that year. Second, New HWFs seem to have much more of an effect on both reemployment and first-time employment rates than all New Firms. While their net employment is lower than New Firms, it is likely that they poach their remaining work force from firms with healthier prospects. That is, New HWFs may poach talent from "better" firms which in turn are more likely to fill their vacancies with additional hires. In contrast, New HWFs may acquire talent from firms that are already shrinking, and consequently do not replace lost workers. In other words, the "spillover" employment effects of New HWFs may be larger than the same effects of New Firms in general. This is consistent with my findings in Chapter 4 that the spillover wage effects of New HWFs are larger compared to the spillover wage effects of all New Firms. Third and finally, running Equation 3.1 on the stock of all firms produces no change to either of our rates of interest at all, which give some comfort that this exercise is not all driven by statistical noise.

3.5 Conclusion

This Chapter puts together descriptive information on the kinds of workers that are hired at new firms. It is interesting to note that new firms, and especially new high wage firms, seem to generate local employment in the regions they start in. Descriptively, we find that local workers benefit from job creation including those from nonemployment. This suggests that large new firms may actually be net-surprlus generating. Additionally, they may form an important channel for job growth in downturns. This can be rationalised by a simple model where all things held constant, if new firms demand just as many workers as incumbent firms, new ones may be more likely to hire "creatively" from parts of the labour pool that are selected against.

Unfortunately, this study is limited in its inability to find "causal" links. the Italian data as it currently stands provides little information on firms themselves. Nonetheless, this work provides some basic information that points to useful areas. Future work in this area will require cleaner identification, possibly through the use of Bartik style shocks or through micro-strategies such as that employed in Greenstone et al. (2010).

Chapter 4

Wages Spillovers From New Firms

4.1 Introduction

Chapter 3 discusses the impact of New Firms on the employment margin and finds small but economically significant increases in entry from non-employment and into the labour force for the first time. This Chapter focuses on their impact on wages. Greenstone et al. (2010) find small increases in the wage of the labour market due to exogenous plant openings. Here, I revisit that question and attempt to trace to whom exactly wage gains are going, if anyone.

In this Chapter, I maintain my focus on the Veneto region of Italy. Using a matched employer-employee dataset, I construct a yearly panel of workers and firms from 1985 to 2001. I start first with workers who gain employment in New Firms and New HWFs themselves. I borrow the framework used to study plant closings by Jacobson, LaLonde, and Sullivan (1993) to study plant openings in Veneto. Event study estimates suggest persistent wage gains of about 2% to those workers that move to New Firms and about 6% to those that move to New HWFs.

Standard models would predict that a large new firm demanding labour at a higher price would exert competitive pressure on existing firms, and bid up the price of labour. If that were true, we should see an overall increase in the wage level in a province after the arrival of a New Firm. As in Chapter 3, I run cross-sectional regressions of the form given in Equation 3.1 to detect any wage effects, but find almost none.

While it is possible that I am underpowered to pick up an effect, there are several competing explanations for why there may not be a province level wage effect.

New Firms are selected to be large and are often larger than incumbent firms. The Employer Size-Wage Effect is well known (Brown and Medoff 1989). It could be that workers selected to work at New Firms are more productive than those at old incumbents or that workers at New Firms need to be compensated more for working at larger firms, which they have preferences against. Another explanation might simply be that the labour supply curve faced by the firm is upward sloping, as suggested by Manning (2013).

I dig further into the possibility of wage spillovers and consider the effect of New Firms on those workers who were colleagues with someone who joined a New Firm. Social networks have long been to help find jobs (Granovetter 1074). In particular, Saygin, Weber, and Weynandt (2014) find that connections with coworkers are an economically important social network. In their context, the population of interest are workers who exited the labour force in a mass layoff. They find that those with job connections to someone that was reemployed were more likely to be reemployed themselves.

There are two main channels through which social networks can help with job matching. On the labour supply side, Calvó-Armengol and Jackson (2004) suggest a model where employed workers pass job information through their networks until it reaches unemployed workers. This can easily be extended to my setting, where those who are still on the job are looking for connections to "better" jobs. On the labour demand side, there are models of referral based hiring where current workers suggest others who may be worth hiring by the firm (Dustmann, Glitz, Schönberg, and Brücker (2016)).

I find that between 15 to 20% of workers who are hired at New Firms have an ex-colleague that works there. A key concern in this literature is that networks are endogenously formed(Imbens and Goldsmith-Pinkham 2013). While there is a growing literature on trying to control for this endogeneity (Auerbach 2017), I follow the simpler approach used in Saygin et al. (2014). Unless one believes that coworker networks are formed with the intention to use them for future job opportunities, using social networks formed through past coworker links should be acceptable.

Ex-coworker wages fall sharply after their colleagues leave, except in the case that they are hired by the firm that their colleague left to. I attribute to the fall in wages for ex-coworkers that do not get employed by the New Firm to the fact that workers not hired by the New Firm are somehow negatively selected, as in

Table 4.1: How Many New Firms Does Someone Work For?

Firms	New Firms		New HWFs	
	Number	Percent	Number	Percent
0	2,245,824	96.72	2,999,928	99.05
1	69,629	3.00	21,447	0.92
2+	6,622	0.29	700	0.03
Total	2,322,075	100.00	2,322,075	100.00

Gibbons and Katz (1991). This is compounded by the possibility that New Firms are likely poaching workers from those firms that are already in decline. The falling wage for stayers adds credence to the hypothesis that New Firms do not exert much competitive pressure in terms of wages on incumbent firms. Similar to Saygin et al. (2014), I find strong evidence for the referral channel of social networks, as the only workers whose wages recover or show improvement are those that are later hired by the poaching Firm.

As the setting and data used in this chapter are identical to Chapter 3, I omit the "Data and Background" and "New Firms and New Workers" sections. Section 4.2 looks at the wage effects of New Firms on workers that join them. Section 4.3 tries to find spillover effects of New Firms on provincial wages, focusing first on the entire province and then specifically on past coworkers. Section 4.4 concludes.

4.2 New Firms and Wages

I begin my analysis of the wage impacts of New Firms on the workers that work for them. I define a "New Worker" as anyone who joins a New Firm. Most workers in Veneto never work for a New Firm (see Table 4.1). A little under 1% work at a New Firm once while a negligible fraction work for one or more New Firms.

Table 4.2 shows the industrial composition of New Workers in their immediately previous jobs. Both New Firms and New HWFs hire predominantly from the Production sector, but a large fraction join from Service sector jobs as well as the Handicrafts sector.

To isolate the effect on wage of joining the New Firm, I estimate models of the

Table 4.2: What Industries Do New Workers Come From?

Firms	New Firms		New HWFs	
	Number	Percent	Number	Percent
Production	63,921	78.49	18,100	80.70
Handicrafts	6,761	8.30	1,848	8.24
Agriculture	299	0.37	45	0.20
Banking and Insurance	312	0.38	136	0.61
Services	10,149	12.46	2,301	10.26
Total	81,442	100.00	22,430	100.00

form:

$$Y_{it} = \alpha_i + \gamma_t + X_{it}^{'}\phi + \sum_{k=\underline{C}}^{\overline{C}} \beta_k D_{it}^k + u_{it} \tag{4.1}$$

where Y_{it} is worker i's daily log wage in year t. I define the date at which worker i is hired at a New Firm as e_i and $D_{it}^k = I[t = e_i + k]$. Additionally $\underline{C}$ and $\overline{C}$ are constants chosen to bin up endpoints so that $D_{it}^{\underline{C}} = I[t \leq e_i + \underline{C}]$ and $D_{it}^{\overline{C}} = I[t \geq e_i + \overline{C}]$. Finally, X_{it} are worker-time specific controls including top level industry codes, experience and province. I impose an additional constraint that a worker cannot have worked in more than one New Firm, to keep the event study clean. This is relatively innocuous as this drops less than 0.5% of all workers (see Table 4.1).

The average wage gain for a New Worker is about 6% (see Table 3.3). The event study of wages of these workers around the year they join a New Firm finds a slight wage gain for joining of around 2% (see Figure 4.1). The event study controls for worker experience, industry and province which help absorb a large component of the raw 6% change in wage. Estimating Equation 4.1 for only those workers that joined New HWFs reveals much larger effects, which is consistent with Table 3.3. Figure 4.2 shows a 6% increase in wage for those workers that join new HWFs.

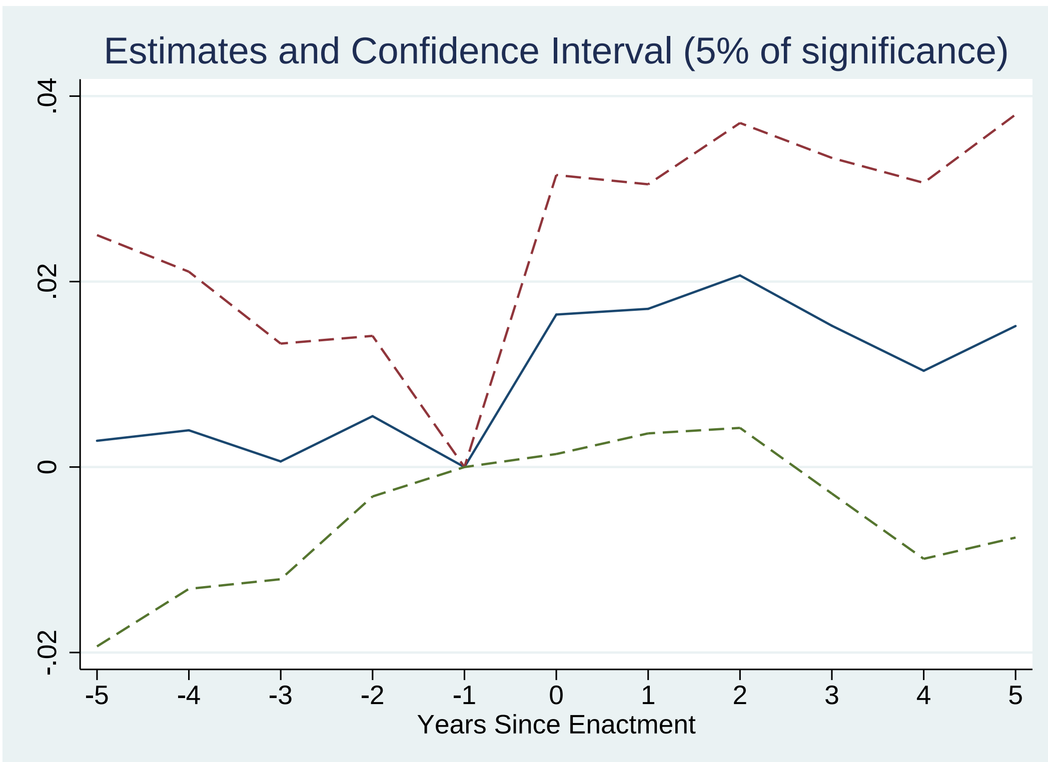

Figure 4.1: Event Study: Impact of Joining New Firm on Wage

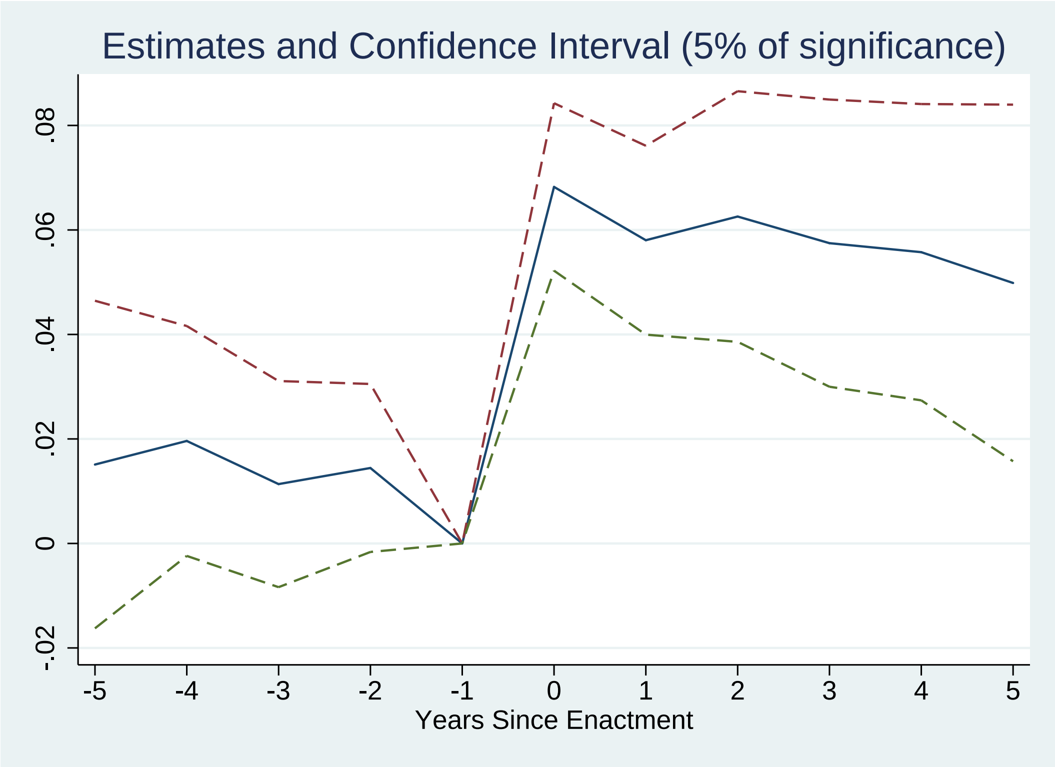

Figure 4.2: Event Study: Impact of Joining New HWF on Wage

Table 4.3: Number of Years in Which At Least One Worker Leaves for a New Firm

Events	Frequency	Percent	Cumulative Percent
1	2,731	38.50	38.50
2	1,409	19.86	58.36
3	856	12.07	70.43
4	528	7.44	77.87
5	391	5.51	83.38
6	279	3.93	87.31
7	209	2.95	90.26
8	154	2.17	92.43
9	120	1.69	94.12
10+	417	5.88	100.00
Total	7,094	100.00	.

4.3 Spillover Effects

It is alluded to in Greenstone et al. (2010) that new firms bid up the wage of other incumbent firms in the labour market. However, running a version of Equation 3.1 from Chapter 3 finds almost no effect of establishing a New Firm (or a New HWF) on the average wage in a province. As suggested by the authors, the right economic linkages may be through worker flows and not through geographical proximity.

Consequently, I start with the subset of workers who join New Firms and identify the set of firms they come from ("Sending Firms"). Most Sending Firms experience only one loss event to New Firms but a significant fraction experience two or more. See Table 4.3 for a detailed breakdown.

For these Sending Firms, I identify all the workers who would have experienced the "loss" of their colleague ("Ex-coworkers"). Due to the mechanics described in Table 4.3, Ex-coworkers can experience multiple events where they lose a colleague to a New Firm. Most Ex-coworkers experience only one loss event, but over 50% experience multiple. I focus on those Ex-coworkers who experience only one event so as to have a clean event study. Table 4.4 provides a detailed breakdown of events experienced by Ex-coworkers.

Firms lose anywhere from one to several hundred workers to a New Firm. Not

Table 4.4: Events Experienced Per Coworker

Events	Frequency	Percent	Cumulative Percent
1	177,402	32.94	32.94
2	88,411	16.42	49.35
3	58,635	10.89	60.24
4	44,856	8.33	68.57
5	34,473	6.40	74.97
6	22,645	4.20	79.18
7	17,030	3.16	82.34
8	15,675	2.91	85.25
9	13,708	2.55	87.79
10+	65,745	12.21	100.00
Total	538,580	100.00	.

surprisingly, the 25th, 50th and 75th percentiles of this loss distribution are all one. The mean number of workers lost is 2.29 and the 99th percentile is 16. Consequently, the distribution of losses has a long but skinny right tail. Due to this skewed distribution, an event study on Ex-coworker wages will tend to overemphasise the events in Sending Firms that lose many workers. The firms are likely in some decline already and are probably negatively selected in some way.

Of the 268,000 workers identified as Ex-coworkers, only a small fraction of less than 5% become New Workers themselves. An even smaller number of only 1.5% of the total number of Ex-coworkers go on to become New Workers at New HWFs. The fraction of The exact numbers are laid out in Table 4.5. As a fraction of the total number of workers employed at New Firms and New HWFs, this number is quite significant. 15 to 20% of all workers at New Firms or New HWFs joined their firm after a previous colleague joined.

I run the same event study specified in Equation 4.1 now focusing on wages of ex-coworkers. The event is now the year that a coworker leaves for a New Firm. Results are captured in Figure 4.3. As can be seen, the effect on coworker wages is quite stark. Upon a colleague leaving to a New Firm, coworker wages decline quite rapidly. This is likely due to the fact that the Sending Firms being negatively selected outweighs any positive wage spillover.

Table 4.5: Ex-coworkers Who Go On To Become New Workers

Variable	Number	Percent of Ex-coworkers	Percent of all New Workers
Ex-coworkers	268,303	100.00	.
Eventual New Workers	12,538	4.67	16.44
Eventual New Workers at New HWFs	4,188	1.56	18.91

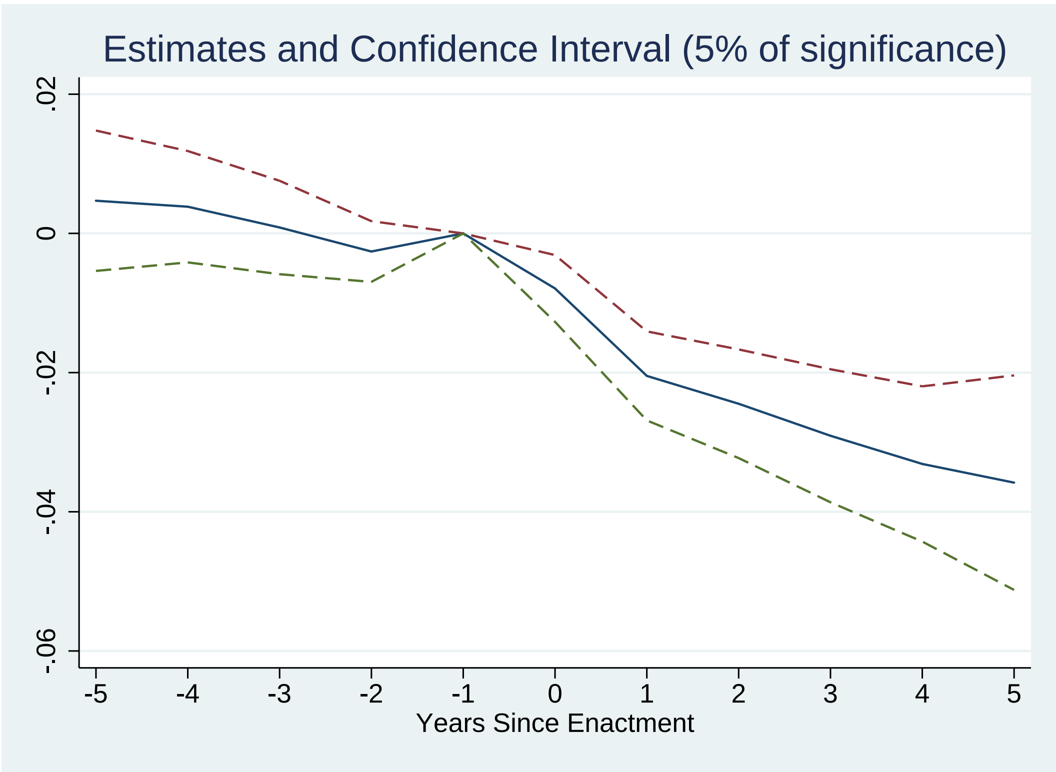

Figure 4.3: Event Study: Ex-Coworker Wages

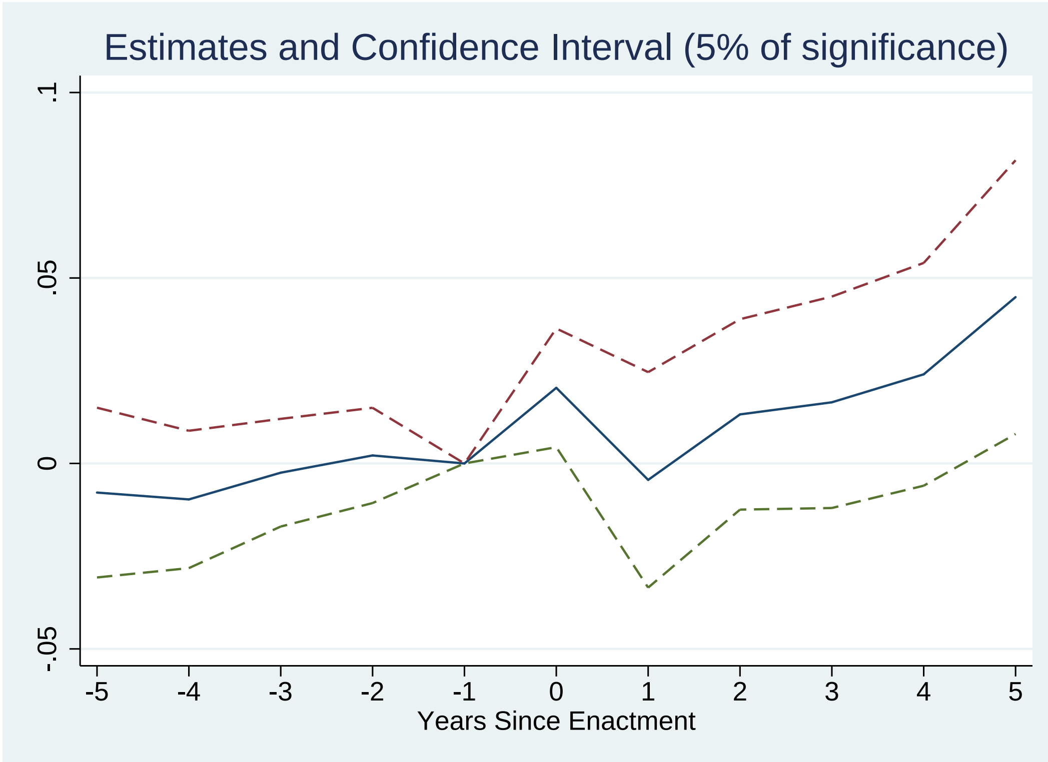

Figure 4.4: Event Study: Ex-Coworker Wages For Only Those Subsequently Employed at New Firms

To investigate this further, I break up this set of Ex-coworkers into those that eventually find employment at a New Firm themselves and those that do not. I run the event study specified by Equation 4.1 on both these groups. The results are captured in Figures 4.4 and 4.5. It is clear the majority of the decline in wages indicated by Figure 4.3 is due to Ex-coworkers that never go on to work at New Firms. Such workers suffer the fate of being negatively selected in that they were not poached by New Firms in an earlier round, and they continue to work in firms that lose workers to New Firms. Rather than the New Firm bidding up the wages at incumbent firms, it seems that they poach on firms on the decline implying no wage spillovers to ex-coworkers. The lucky few Ex-coworkers that do find employment in New Firms seem to stanch the wage loss and maybe even witness wage gains similar to earlier movers.

The same set of questions can be asked of colleagues of workers that join New HWFs. That is captured in Figure 4.6. Again, I break up this set of Ex-coworkers into those that eventually find employment at a New HWF themselves and those

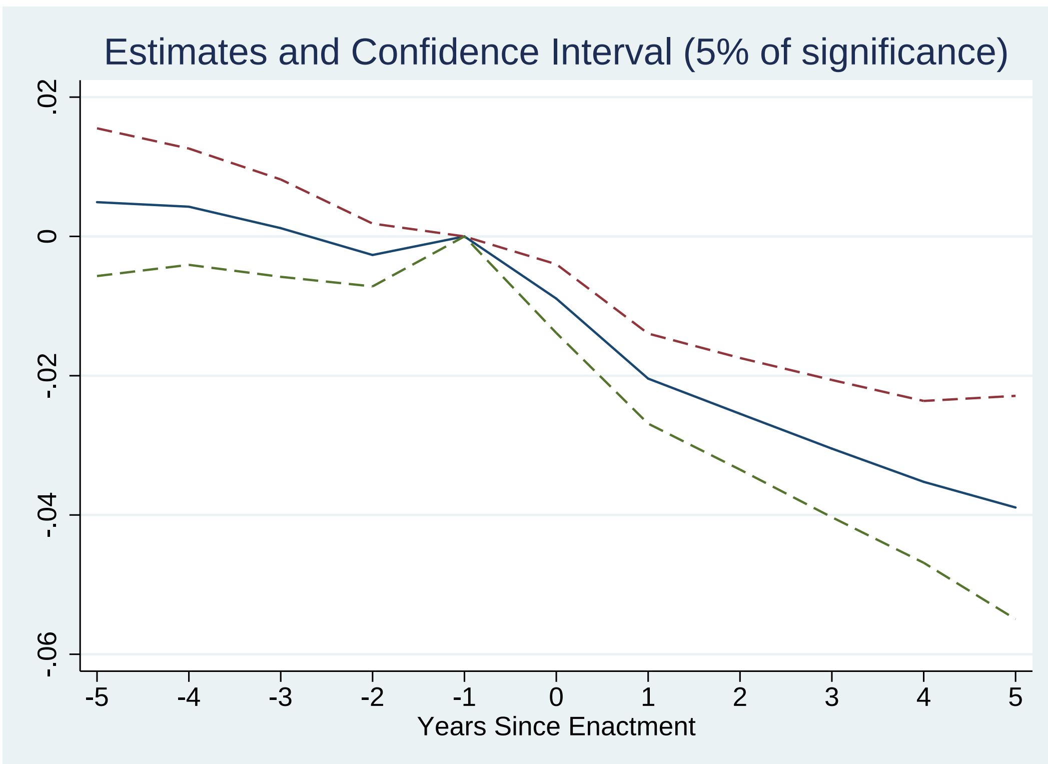

Figure 4.5: Event Study: Ex-Coworker Wages For Only Those Not Subsequently Employed at New Firms

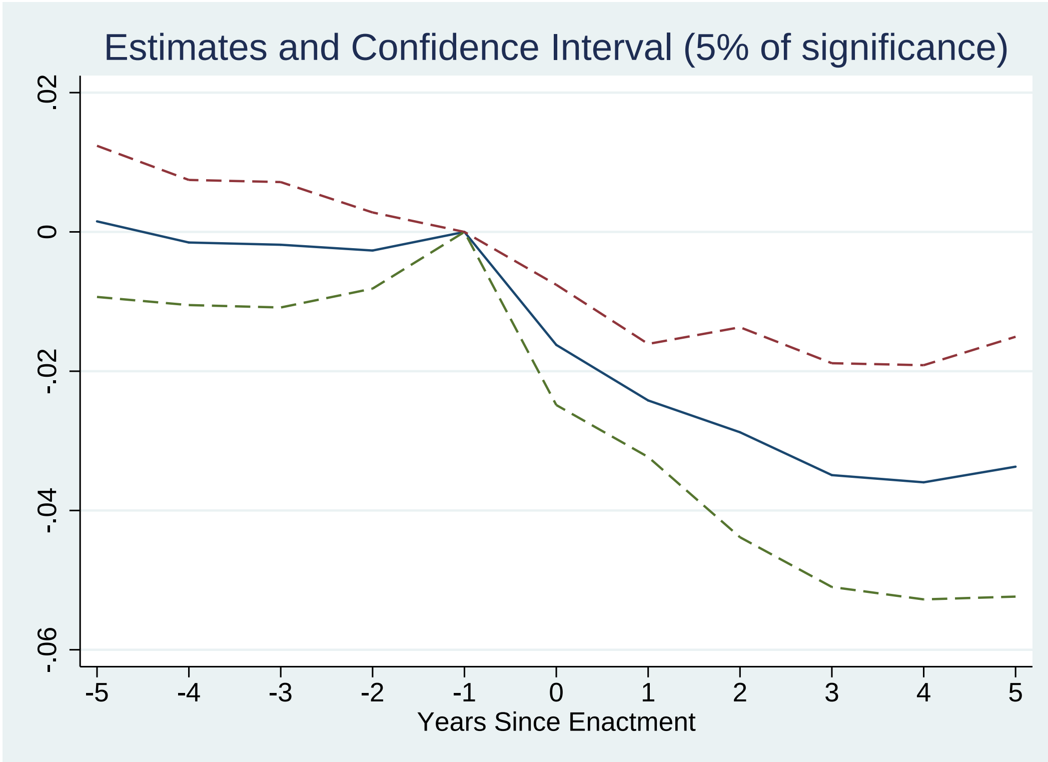

Figure 4.6: Event Study: Ex-Coworker Wages (New HWF Coworkers Only)

that do not. I run the event study specified by Equation 4.1 on both these groups. The results are captured in Figures 4.7 and 4.8. Given the very small number of Ex-coworkers who go on to work at New HWFs, the event study is underpowered and no clear trends emerge.

4.4 Conclusion

Chapters 3 and 4 jointly provide information on the impact that new firms may have on their local labour markets. This Chapter specifically focuses on the wage spillovers generated by these firms. I find some evidence that the spillovers are not large enough to affect the entire labour market. This suggests that simple models of the labour market that assume healthy competition may be incomplete. Further research in the area can flesh out the theoretical cases where one might expect that large labour demand shocks do not affect the entire market.

On further inspection I find that some workers do benefit from hiring at New Firms, namely those who have relationships with the people actually hired at those

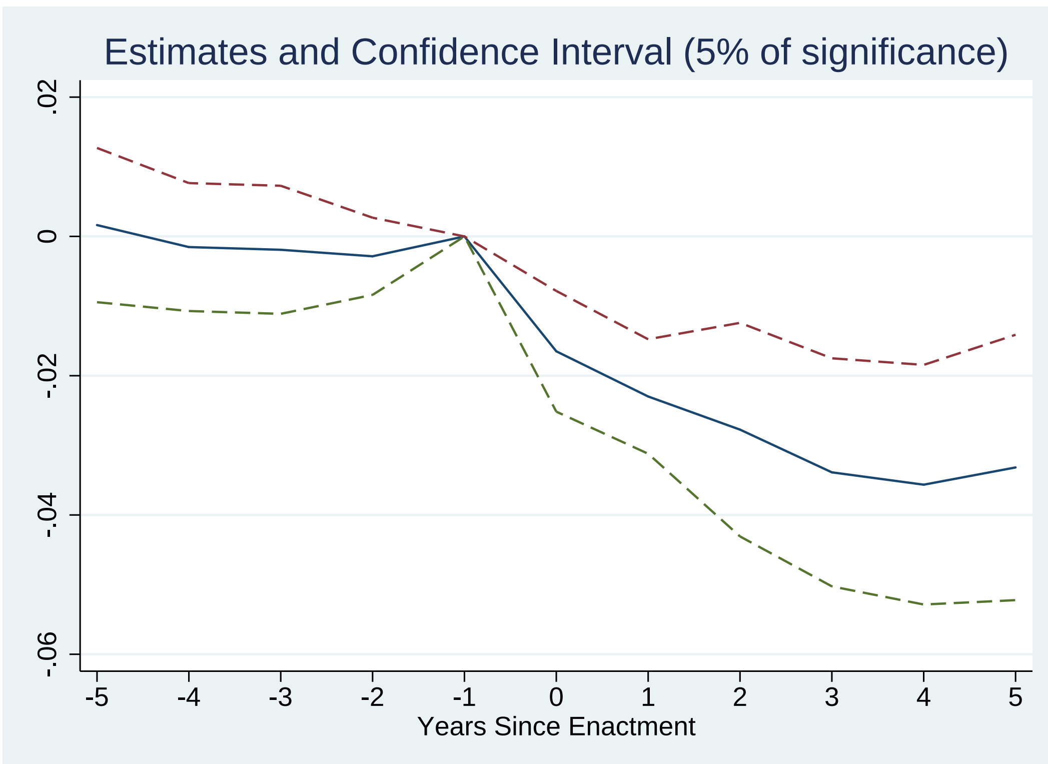

Figure 4.7: Event Study: Ex-Coworker Wages (New HWF Coworkers Only) For Only Those Not Subsequently Employed at New HWFs

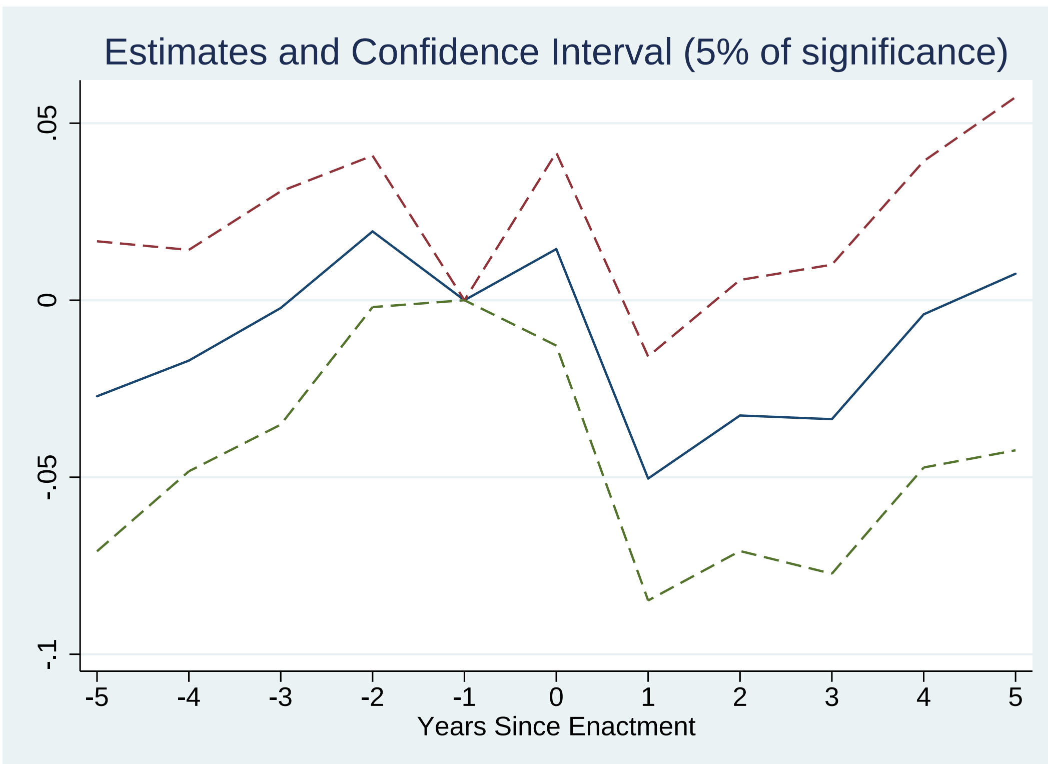

Figure 4.8: Event Study: Ex-Coworker Wages (New HWF Coworkers Only) For Only Those Subsequently Employed at New HWFs

firms. However, past colleagues seem important only insofar as they provide information and referrals to jobs in their new workplaces. It is interesting that the workers that remain at incumbent firms seem to suffer in terms of wage growth. Future work should focus more on understanding the reasons for poor wage growth among incumbents. Both strands of research, as in the work in Chapter 3 will benefit tremendously by cleaner methods to identify causally useful variation.

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Appendices

Appendix A

Veneto Build

The Veneto Workers History (VWH) dataset contains three files – a worker file, a firm file, and a worker history file. Using these, I construct one yearly employer-employee matched panel file through the following steps:

1. Build PY backbone.
   (a) Start with the worker history file.
   (b) Keep only data from 1985 onwards.
   (c) For each worker, keep only one job per year. This job is selected to be the one that paid her the most in that year.
2. Clean firm file.
   (a) Clean up firm start / stop dates.
3. Merge worker and file files to PY backbone
4. Prepare the PY file for estimation
   (a) Keep only workers between the ages of 18 and 64.
   (b) Drop apprentices and people with wages below EUR 5.
   (c) Drop workers in years where they worked zero days.
   (d) Drop workers who experience very large wage changes (above 100% in one year).
   (e) Drop workers in the public sector.

(f) Drop workers with more than 10 jobs in a year.

(g) Drop workers with no reported gender.

Below are numbers showing how the various subsets affect the sample's overall size.

Appendix B

Veneto New Firms

Hethey-Maier and Schmieder (2013) show that a common feature of administrative datasets such as the VWH data is the introduction of new firm identifiers that are not necessarily new establishments. Sometimes, this is due to legal reasons. When the entity reporting information on the establishment changes, that can trigger a change in the firm identifier. This can happen in the case of change of ownership of an establishment. Similarly, some changes in the tax status of an establishment can trigger changes to its identifier. Finally, establishments often split into two or more establishments. In such cases, it is important to identify spin offs. Conversely, two or more establishments may merge into an establishments. Hethey-Maier and Schmieder (2013) suggest using worker flows between firm identifiers to recognise these cases and correct for them. Figure B.1 reproduces Table 1 from Hethey-Maier and Schmieder (2013).

Once the basic PY file is built, I apply a version of the corrections suggested by Hethey-Maier and Schmieder (2013) to ensure that firm identifiers genuinely identify New Firms. First, I only correct for firm entries and not exits. Second, Hethey-Maier and Schmieder (2013) observe the entire universe of workers in their dataset. I only observe workers in Veneto, as well as their full work histories including when they are not in Veneto. Consequently, it is not always possible to correctly estimate predecessor employment. The full firm flow correction algorithm, as well as additional cleaning is provided below:

1. Start with the PY build

2. For each New Firm:

Table 1: Classifying Entering and Exiting Establishments by Clustered Worker Flows

Panel A: Entries

		Predecessor exits MCI / Predecessor Employment			Predecessor continues MCI / Predecessor Employment			No predecessor MCI=0
	MCI / Inflows	<30%	30-80%	>80%	<30%	30-80%	>80%	
≤3 empl.	-	New Estab (small)	New Estab (small)	New Estab (small)	New Estab (small)	New Estab (small)	New Estab (small)	New Estab (small)
>3 empl.	<30%	New Estab (med & big)	New Estab (med & big)	New Estab (med & big)	New Estab (med & big)	New Estab (med & big)	New Estab (med & big)	New Estab (med & big)
	30-80%	New Estab (fuzzy)	New Estab (fuzzy)	Unclear	New Estab (fuzzy)	New Estab (fuzzy)	Unclear	
	>80%	Spin-off pushed	Spin-off pushed	ID Change	Spin-off pulled	Spin-off pulled	Unclear	

Panel B: Exits

		Successor is entrant MCO / Successor Employment			Successor is existing estab. MCO / Successor Employment			No successor MCO=0
	MCO / Outflows	<30%	30-80%	>80%	<30%	30-80%	>80%	
≤3 empl.	-	Small Death	Small Death	Small Death	Small Death	Small Death	Small Death	Small Death
>3 empl.	<30%	Atomized Death	Atomized Death	Spin-off pushed	Atomized Death	Atomized Death	Atomized Death	Atomized Death
	30-80%	Fuzzy Death	Fuzzy Death	Spin-off pushed	Fuzzy Death	Fuzzy Death	Fuzzy Death	
	>80%	Unclear	Unclear	ID Change	Take-Over / Restruct.	Take-Over / Restruct.	Unclear	

Notes: MCI stands for Maximum Clustered Inflow: the size of the largest cluster of inflowing current workers. Inflows stands for all the total number of workers that arrived since the previous year at an EID, which for a new EID is the same as total current employment. MCO stands for Maximum Clustered Outflows: the size of the largest cluster of outflowing current workers. Outflows are all workers that leave the EID until the next year.

Figure B.1: Table 1 from Hethey-Maier and Schmieder (2013)

(a) Identify the Predcessor, ie the firm that contributed the most to the employment of the New Firm in its first year

(b) Compute the Maximal Cluster Inflow (MCI), ie the total number of workers joining the New Firm

(c) Compute the Inflow Fraction, ie the ratio of the MCI to the New Firm's total employment

(d) If the Inflow Fraction is larger than .8 and the New Firm's size is larger than 3, the New Firm inherits the Predecessors establishment identifer

3. Update all firm covariates to reflect changes to establishment identifiers