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# ARRESTS WITHOUT CONVICTION: HOW OFTEN THEY OCCUR AND WHY

Final Report Appendix Volume

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#### ACQUISITIONS

Floyd Feeney Forrest Dill Adrianne Weir

Report to the National Institute of Justice U.S. Department of Justice

Center on Administration of Criminal Justice University of California, Davis

November 1982

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#### San Diego Jacksonville Percent Percent Number Convicted Number Convicted (100) (100) 1 1 6 4 100 3 (100) 10 (50) 1 \_ 2 \_\_\_\_\_30 191 \_\_\_\_\_ 181 200 50 200 34 1 1 (100) (100) l 3 4 (100) 3 -1 3 (100) ---50 191 <u>190</u> 44 34 200 50 200

L

- i

this case

this case

case

# To Other Charges in Case No relationship Sample event dropped in part due to plea to nonsample event charges in Guilty plea to sample event due to dismissal of nonsample event charges in Other effect No other charges in this Total To Concurrent Cases No relationship Sample event dropped in part due to plea in other case Guilty plea to sample event in part due to dismissal in No concurrent cases Total

other case

Other effect

#### Table C-10-1

#### Relationship to Other Charges and Cases--Robbery (In percent of persons arrested)

#### Relationship to Other Charges and Cases--Burglary (In percent of persons arrested)

	Jacksonville		San	San Diego		
		Percent		Percent		
To Other Charges	Number	Convicted	Number	Convicted		
No relationship	7	100	5	60		
Sample event dropped in part due to plea to non- sample event charges in this case	1	(100)	7			
Guilty plea to sample event due to dismissal of non- sample event charges in this case	3	(100)	5	100		
Other effect	_	<b>-</b> ·	1	(100)		
No other charges in this case	189	68	200			
Total	200	70	219	53		
To Concurrent Cases						
No relationship	9	100	8	88		
Sample event dropped in part due to plea in other case	l	-	11	-		
Sample event dropped in part due to conviction in concurrent case	2	-	-	67		
Guilty plea to sample event in part due to dismissal in		(100)	. :	100		
other case	4	(100)		100		
Guilty plea due in part to reduction in other case	 <del></del>	_	1	(100)		
Other effect	3	(67)	3	(100)		
Unclear	-		2	(50)		
No concurrent cases	181	69	186	52		
Total	200	70	219	53		

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#### To Other Charge

No relationshi

Sample event dr part due to ple sample event ch this case

Guilty plea to due to dismissa sample event ch this case

Other effect

No other charge case

Unclear

#### Total

To Concurrent C

No relationship

Sample event dro part due to plea case

Guilty plea to in part due to in other case

Other effect

No concurrent ca

Total

(

C-2

### Table C-10-3

# Relationship to Other Charges and Cases <u>Felony Assault--Stranger-to-Stranger</u> (In percent of persons arrested)

	Jacks	sonville	San	Diego
res in Case	Number	Percent	NI	Percent
		CONVICTED	Number	Convicted
.p	2	(100)	2	(100)
ropped in ea to non-				
harges in	_			
		-	-	-
sample event al of non- harges in				
	-	-	1	(100)
	· <b></b>	_	<b></b>	-
es in this				
	26	54	34	50
	· · · ·	100	-	-
	29	59	37	54
Cases				
)	_	-	1	(100)
copped in a in other				
	-	<u> </u>	-	-
sample event dismissal				•
•		-	-	· · ·
		-	<b></b>	-
ases	29		36	_53
	29	59	37	54

.

Convictions by Robbery TypeMcClintock Codes* (In percent of persons arrested)					
Robbery of persons in charge of valuables as part of their employment	Jacks	sonville Percent Convicted	San Number	Diego Percent Convicted	
In transit	3	-	10	40	
Carried during every-day employment	1	(100)	3	(67)	
In shops, banks during working hours	83	70	44	36	2
On business premises following illegal entry	3	(67)	2. 	 –	
Robbery in open or public places following sudden attack					
Male victims in open	51	31	64	25	
Female victims in open	9	22	14	71	
Male victims in public but enclosed areas (i.e., public restroom, hotel hallway) Robbery on private premises	2		12	42	A second of
By offenders who knock and forcibly enter	9	56	8	63	
By housebreaker subsequently disturbed by member of household	1	(100)	_		
By offenders who enter house without permission	1	-	· - ·	· · -	
Robbery after short association					
Of victim decoyed by prostitute	9	22	5	20	
Of prostitute by client	. –	<del>_</del>	1	- '	
Of victim in street or in car following preliminary association	5	40	14	57	
Of victim in vicinity of bar after drinking with	2				
offender	3	(33)	<b>_</b>	· _	150
After going home together	-	-	5	· · · · · ·	

C-4

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CONTINUED ON NEXT PAGE

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Robbery after pre-association (e.g. friends, lovers, s

Other

No actual overt at or conspiracy or only

Unclear

Total

\*Codes are adapte

# Table C-ll-l (continued)

# Convictions by Robbery Type--McClintock Codes\* (In percent of persons arrested)

	Jack	sonville	San Diego		
evious	Number	Percent Convicted	Number	Percent Convicted	
workmates)	7	43	4	· _	
	1	-	l		
ttempt accessory					
	2	-	8		
	10	60	5	_	
	200	50	200	34	
ed from McCli	intock and G	ibson, Robbery	in London	(1961), p. 43.	

C-5

Sec

Convictions by Assault Type--McClintock Codes

Felony Assau	ltStran	ger-to-Stranger					
(In percent of persons arrested)							
	Jack	San	Diego				
		Percent		Percent			
	Number	Convicted	Number	Convicted			
Disputes in street	4	(100)	11	55			
Disputes on highway	1	-	1	(100)			
Disputes in bar or cafe	10	60	7	57			
Sexual offenses	<b>-</b> '	-		-			
Miscellaneous attacks	13	46	17	47			
Civilians interviewing							
apprehend criminals	<u> </u>	(100)		(100)			
Total	29	59	37	54			

6:0

Handgun Rifle or shotgun

Carried and Seen

Knife

Bottle

L

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Club or blunt instrument

Liquid

Other weapon

Implied

Handgun

### No weapon Seen or Implied

Unclear

Total

# Table C-11-3

# Weapon Carried or Implied by Defendant--Robbery (In percent of persons arrested)

	Jacks	sonville	San Diego			
	Number	Convicted	Number	Convicted		
	54	60	25	44		
	5	80	4	(75)		
	13	85	22	64		
	2	-	-	-		
	2	(100)	6	17		
	l	(100)	-	_		
	1	-	2	-		
	5	100	6	83		
<u>l</u>	99	40	83	36		
	7	43	_16	19		
	200	50	200	34		

5A

#### Table C-ll-4

# Force Applied in Robbery By Defendant (In percent of persons arrested)

	Jacksonville		San	Diego Percent
	Number	Convicted	Number	Convicted
Locked victim up, forced victim to lie on floor	14	86	9	67
Threw something at victim and missed	2	(50)	1	(-)
Tied up or gagged victim	24	42	13	23
Pushed victim around or knocked victim down	16	44	35	49
Kicked victim or beat victim with fists	8	25	10	30
Clubbed or hit victim with something	<b>-</b> 1	-	6	67
Cut or knifed victim	1		1	
Shot and hit victim	. –	-	-	-
Hit victim with auto	5	80	5	40
Unclear	11	27	20	10
No force used	108	55	100	29
Total	200	50	200	34

(§ - §)

69

5

Locked victim victim to oie Threw something and missed Pushed victim a knocked victim Kicked victim o victim with fis Clubbed or hit with something Cut or knifed vic Shot and hit vic Other Unclear No force used Total

T

**(**)

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C-8

# Table C-11-5

# Most Serious Type Force Used By Any Participant in Case as a Whole--Robbery (In percent of persons arrested)

	Jacksonville		San Diego		
· · ·	Number	Convicted	Number	Percent Convicted	
up, forced on floor	18	67	12	42	
ng at victim	2	- -	1	_	
around, down	39	28	23	17	
or beat sts	24	29	54	37	
victim	14	43	18	33	
Victim			11	36	
.ctim	-		1	(100)	
	7	57	7	14	
	.—		3	-	
	88	60	62	42	
	200	50	200	34	

(							í	
	Jacks Number	sonville Percent Convicted	San Number	Diego Percent Convicted			and the second model of the second	
No loss	16	19	25	32			a high air air airean 18 an	
\$1-20	25	48	34	18			an unital la faire de la contraction de	
\$21-50	22	59	25	36			a superior a second a second second	
\$51-100	33	42	36	47			angelangen gebreiten.	
\$101-200	24	50	30	20			nin nationale nationa	
\$201-500	25	80	19	47			al and a second s	
\$501-1,000	8	75	15	40			Add - Lands of the South of Contract of Co	
\$1,001 or more	11	55	3	(67)			тарум <sup>1</sup> (2) до 19 м <sup>1</sup> м <sup>2</sup> м 1 м 1 м	
No information	3		13	31		and a second	n (a.v., gan g Millionen ().	<b>I</b>
Total	200	50	200	34	~ ~		provenska vranegoversk	
						1	1	

Dollar Loss--Robbery (In percent of persons arrested)

 $\langle \gamma \rangle$ 

- in

Offenders

l offender

2 offenders

3 offenders

Unclear

Victims

l victim

2 victims

Unclear

Overall

3 or more victims

C-10

#### Table C-11-7

# Number of Offenders and Victims--Robbery ( (In percent of persons arrested)

Jack	sonville	San Diego			
Number	Percent Convicted	Number	Percent Convicted		
61	51	63	32		
63	56	78	45		
34	56	35	23		
35	34	21	19		
7	-	. 3	_		
162	47	164	30		
13	54	28	61		
17	59	1	, <del>-</del>		
8		3			
200	50	200	34		

62

4 or more offenders

# Type of Premise--Burglary (In percent of persons arrested)

	Jacks Number	onville Percent Convicted	San Number	Diego Percent Convicted		and and a second and		
Single family dwelling	62	66	76	61			and the second	Sleep
Duplex	· <u>1</u>	(100)	15	40		An and a second seco		Restroom
Apartment	31	58	37	32				Consent o
Hotel	· · -	-	11	36				Claim of
Other residential	8	88	4	(75)			Grand A start you have	Other
Bar-restaurant	8	88	15	67				Total
Warehouse or industrial	17	71	5	00				
Retail store	16	80	11	73		an and a second s		
Pharmacy	5	60	7	43	(° ~ 3)	an Ar wand de Yang yang yang yang yang yang yang yang y	And a second secon	
Other commercial	45	69	20	55				
Church	l	(100)	9	44				
Schools-libraries	3	(100)	4	(50)		4) II. (19. )		
Other public buildings	3	(67)	• 3	(100)				
Unclear			2					
Overall	200	70	219	53		and second s		

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# Table C-11-9

# Non-Theft Reasons Claimed for Entering Premises--Burglary

Jack	sonville	San Diego				
Number	Convicted	Number	Percent Convicted			
1	(100)	5	20			
3	(67)	2	(50)			
2	(50)	5	20			
2	-	2	(50)			
_16		_13	46			
24	75	27	37			

 $\mathbf{A}$ 

.

# Property Recovered--Burglary (In percent of persons arrested)

	Jacksonville		San	Diego			and a familitation of the
	Number	Convicted	Number	Convicted			
All recovered	32	69	34	59			an in mEndon bring the second
Some recovered	33	76	38	50			
None recovered	18	33	27	26		-	وكالما مراجع والمحمد والمحمد
Money taken from defendant(s) at arrest, unclear if was part of stolen property	1		- · ·	_			ي منهم الجامع من من من من من من الخاصة الخاصة الجامع من
Getaway not complete, property never actually lost	32	84	33	82			والمعارية والمعاركة والمعارية والمعارية والمعارية والمعارية والمعارية والمعارية والمعارية والمعارية والمعارية و
Other	_	-	2	(50)			and a stand stand stands of the stand
Nothing taken	76	75	81	49	A sur		a a succession of the second se
Unclear if property recovered	8	_25	3	(33)	<b>~</b>		and a second
Total	200	70	219	53		- · · · · · · · · · · · · · · · · · · ·	

No loss \$1-20 \$21-50 \$51-100 \$101-200 \$201-500 \$501-1,000 \$1,001 or more No information Total

()

3

39 20

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C-14

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# Table C-11-11

# Dollar Loss--Burglary (In percent of persons arrested)

Jack	sonville	San Diego			
Number	Percent Convicted	Number	Percent Convicted		
76	75	82	49		
3	67	10	50		
9	33	11	64		
7	71	7	14		
9	67	12	58		
24	67	17	53		
10	70	22	68		
24	75	28	43		
38	66	30	63		
200	70	219	53		

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### Anyone in Building or Confronted?--Burglary. (In percent of persons arrested)

	Jacks	sonville	San Diego		
	Percent			Percent	
In Building	Number	Convicted	Number	Convicted	
Yes, someone was in					
building	35	60	40	53	
No one was in building	165	72	168	52	
Unclear	-	· –	11	55	
Confronted					
Someone was confronted by defendant	24	54	22	50	
Someone was confronted by defendant and other suspect	4	(50)	1	(100)	
Someone was confronted by other suspect only	-	- -	2		
No one was confronted	172	72	190	54	
Unclear			3	(33)	
Overall	200	70	219	53	

Offenders

( )

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 $\sim$ 

l offender

2 offenders

3 offenders

4 or more offenders

Unwitnessed burglary

Unclear

Total

#### Table C-11-13

### Number of Offenders--Burglary (In percent of persons arrested)

Jacks	onville	San Diego				
	Percent		Percent			
Number	Convicted	Number	Convicted			
72	68	84	52			
71	75	53	74			
20	85	16	56			
19	42	9	56			
9	67	45	36			
9	67		_17			
200	70	219	53			

C-17

# Number of Offenders and Victims Felony Assault--Stranger-to-Stranger (In percent of persons arrested)

	Jacks	sonville	San	Diego
Offenders	Number	Percent Convicted	Number	Percent Convicted
l offender	24	68	21	52
2 offenders	2	(50)	5	60
3 offenders	3	-	10	60
Unclear	-	-	l	-
Victims				
l victim	23	61	28	61
2 victims	2	(100)	6	50
4 or more victims	4	(25)		
Overall	29	59	37	54

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Outside
Street-sidewalk
.Parking lot
Highway
Park
Other open space
Inside building
Store
Bar, club, etc.
Hotel, motel
Other commercial buil
Victim's house
Defendant's house
Bus
Cab
Other
Total

# Table C-11-15

# Where Assault Occurred Felony Assault--Stranger-to-Stranger (In percent of persons arrested)

	Jack	sonville	San	1 Diego		
	Number	Percent Convicted	Number	Percent Convicted		
	7	57	14	50		
	2	(100)	3	_		
	3	(33)	l	(100)		
	<b></b> .	-	2	(50)		
	1	(100)	_	-		
	1	(100)	1	(100)		
	10	60	9	56		
	_	-	2	100		
lding	2	-	-	. <b>-</b> .		
	·	-	2	(50)		
	2	(50)	-	<b></b>		
		-	1	(100)		
	_ `	-	1	(100)		
	<u> </u>	(100)	<u> </u>			
	29	59	37	54		

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# Provocation Felony Assault--Stranger-to-Stranger (In percent of persons arrested)

	Jacks	onville Percent	San	Diego Percent	
(Pressoution)	Number	Convicted	Number	Convicted	
Yes, physical	3	(33)	1	(50)	
Yes, threat or other verbal	-	-	2	100	
Victim stole from defendant	1	(100)	-		
No	21	52	30	57	
Unclear	4	100	3	-	
Provocation (Defense)					
Yes, physical	6	50	8	88	Ø
Yes, threat or other verbal	ı l	(100)	6	67	4
No	8	63	11	36	
Defendant did not say	11	46	12	42	
Unclear	3	(100)		-	
Overall	29	59	37	54	

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	Jack	sonville Percent	San	Diego Percent
Who First Verbally	Number	Convicted	Number	Convict
Combative?	_			
Defendant	8	63	20	55
Someone with defendant	-	. –	2	(100)
Victim	-	-	4	(75)
Someone with victim	-	-	1	(100)
No one	11	55	7	43
Unclear	10	60	3	
Who First Physically Combative?				
Defendant	20	65	30	53
Someone with defendant	3	-	3	(100)
Victim	2	(50)		-
No one	1	(100)	-	<b>-</b> .
Unclear	3	(67)	4	(25)
Number of Episodes				
1	24	63	23	48
2	5	40	10	90
3	-	-	1	,—
Unclear	_		3	-
Disagreement as to Prose- cutor's Version of Events				
No	18	72	16	44
Defendant	5	20	13	62
Witnesses	1	(100)	<b>-</b>	-
Unclear	5	40	8	63
Overall	29	59	37	54

### Table C-11-17

# Dynamics of Dispute Felony Assault--Stranger-to-Stranger (In percent of persons arrested)

### Table C-ll-18

Presence of Weapons and Use of Force Felony Assault--Stranger-to-Stranger (In percent of persons arrested)

	Jacksonville		San	Diego
Weapons Present	Number	Convicted	Number	Percent Convicted
Handgun	14	50	2	(100)
Rifle or shotgun	2	(100)	_	••••
Knife	6	67	14	36
Bottle	3	(67)	2	(100)
Club or blunt instrument	2	34	8	75
Auto	1	(100)	6	67
Other weapon	·	-	2	-
None			4	(25)
Total	29	59	37	54
Force Applied Pushed victim around, knocked victim down	_	- -	· · · ·	
Kicked or beat victim with fists		· <b>_</b>	. 4	(50)
Clubbed or hit victim with weapon	7	57	8	88
Stabbed or cut victim	4	(50)	11	36
Shot and hit victim	4	(25)	1	(100)
Hit victim with auto	-	-	4	(50)
Other	10	60	1	-
Unclear what force used	-	-		- -
No force used	4	(100)	<u> </u>	(50)
Total	29	59	37	54

C-22

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# Hospitalized

At least one vict treated and releas no one hospitalize

Minor injuries on

Not injured

Overall

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### Table C-11-19

	Injur	ry to	Victin	n(s)	
Felony	Assault	:St	ranger-	-to-St	ranger
(In	percent	of p	ersons	arres	ted)

	Jacks	sonville Percent	San	Diego Percent		
	Number	Convicted	Number	Convicted		
	7	29	10	70		
cim ased but						
zed	6	50	11	55		
ly	1	(100)	7	57		
	15	_73	9	33		
	29,	59	37	54		

	Ta	able C-14-1										
	Identificat:	ion AttemptsRobb	pery						Table C-	-14-1 (continued)		
	Jacks	sonville	Sar	Diego					Identificati	ion AttemptsRobb	erv	
Within One Hour	Number of Identification Attempts	Number of Identification Attempts Ending In Conviction	Number of Identification Attempts	Number of Identification Attempts Ending In Conviction	 1		non de la companya d		Jacks Number of	sonville Number of Identification	San Number of	Diego Number of Identification
Positive ID made	70	43	75	35	-			Over 6 Hours	Identification Attempts	Attempts Ending In Conviction	Identification Attempts	Attempts Ending In Conviction
V-W knew D	8	2	1	-	•			Positive ID made	e 5	1	4	, ,
Tentative ID made	e 2	1	a da <b>-</b> 1977	-				V-W knew D	1	- -	• •	- -
D found <u>not</u> the robber	l	· _ ·	3	. · ·				Tried but no ID made	2	1	1.	
Tried but no ID	_							Other	2	1	_	. <b>_</b>
made	7	5	3	<b>–</b> *				Total	10	3	5	1
Other	3	2					-					
Total	91	53	84	35				<u>Total Field</u> Identifications				
In 1-2 Hours								Positive ID made	95	<del>-</del>	91	
Positive ID made	16	9	12	6				V-W knew D	12	-	1	_
V-W knew D	1	-	-	-	~			Tentative ID	4	- · · ·	-	_
Tentative ID made	e 2	-	-		11			Tried but no ID				
Tried but no ID made	2	1	2	. –				made	15	-	8	_
Other	2	1	-	-				D found not the robber	1	_	. 3	
Total	23	11	1.4	6		ŧ .		Other	11	:	1	-
								Total	138		104	
In 2-3 Hours									100		104	<b>–</b>
Positive ID made	4	2	-	-			len frankjer	Photo Lineups				
V-W knew D	1	<b>—</b>	-	_				Positive ID made	40	30	24	18
Tried but no ID		_	_					V-W knew D	1		4	2
made	2	L	Ţ	_				Tentative ID	2	2	6	5
Other	2	<u></u>					and an and a second	D found <u>not</u> the				
Total	9	4	1	<del>-</del> .				robber	4	2	-	
In 3-6 Hours								V-W make conflic ID's	ting l	1	4	3
Positive ID made	4	2		-				Tried but no ID	16	7	31	13
V-W knew D	1			: <u>.</u>		<b>T</b>		Other	2	1	<u> </u>	1
Tried but no ID made	2	1	1		(T)			Total	66	43	70	42
Other	2	1	<u></u>				1				CONTINUED ON	NEXT PAGE
Total	9	4	1									
		C-24	CONTINUED ON N	IEXT PAGE								

# Table C-14-1 (continued)

# Identification Attempts--Robbery

	Jacks	sonville Number of Identification	San Number of	Diego Number of Identification	re - Bury - arts Bretter				Num Ident	Jacksonville ber of Multiple ification Attempt	Numi s Ident:	San Diego Der of Multiple ification Attempt
Photo Book	Identification Attempts	Attempts Ending In Conviction	Identification Attempts	Attempts Ending In Conviction	<ul> <li>Longity, Bally, and State of the State of th</li></ul>	n ger være i som en	, E	Field ID and photo lineup		9		_
Positive ID made	e 5	2	_	-		n de Sente d		Field ID and other		2		9
V-W knew D	1	-	-	-				•		- · · ·		
Tried but no ID	3	1	1.	· <b>_</b>			-	Photo lineup and		7		15
Asked and refuse	ed l	-	_	-			-	arte armeur		<b>~</b>		
Other	2	1	-					Photo lineup and other		3		1
Total	12	4	l	 	-	and and a second		Other multiple attempt	ts	_2		
Live Lineup					-	Lanaman C. Status		Total		17		25
Positive ID made	e 4	2	13	11		다. (11) (11) (11) (11) (11) (11) (11) (11		IOCUL		17		20
V-W knew D	1	-	-	-		0151 (1140) 						
Tried but no ID	3	1	17	7		19-10-1		Re	eason for Se	cond or Third Att	empt	
Other	2	_1			A.7							
Total	10	4	30	18	4.2				Number of Attempts	Positive Identifications	Number of Attempts	Positive Identifications
V-W Spots D After	• • • • • • • • • • • • • • • • • • •							Second or third				
6 Hours				_				attempt to get ID	9	4	19	15
Positive ID mad	le 8	3	5	· · · · · _				Testing a positive ID	6	4	6	5
V-W knew D	1		-						-			
Tried but no ID	) 2	1		-				Defense initiated	2	-	. 🛋	-
Other	2	<u> </u>				стан 1996 - 199						
Total	13	5	6	-			er contra de marte					
Other One-on-One						r nohi ter Wang, da ya						
Positive ID mad	le 8	4	1	1								
V-W knew D	2	· · · ·		-		And the second se						
Tried but no II	2	1	1	-		- Charleston						
Other	2	_1				and a second sec						
Total	14	6	2	· <b>1</b> ·		T	er a					
Total						nd Taba a y la de la desarra de la desar						
					\$					C-27		

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C-26

# Table C-14-2

# Multiple Identification Attempts--Robbery .

J Numb Identi	acksonville er of Multi fication At	e iple tempts	San Diego Number of Multiple Identification Attemy	ots
	9		<del>_</del> . * *	
	2		9	
	1		15	
	3		1	
	2			
	17		25	

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# In-Court Identifications--Robbery

	Jack Number of Identification Attempts	sonville Number of Attempts Ending In Conviction	San Number of Identification Attempts	Diego Number of Attempts Ending In Conviction
Positive ID mad	e 3	1	43	42
V-W knew D	l	-	3	2
Tried but no ID made	2	1	13	6
Other	2	<u> </u>		
Total	8	3	59	44

#### Whether There Had Been a Prior Out-of-Court ID?

	Ja	ecksonville*	S	**	
	Number of Attempts	Positive Identifications	Number of Attempts	Positive Identifications	4
In-court only	NA	NA	15	11	
Had made previous identification	NA	NA	41	32	

\*No information was available on in-court identifications for Jacksonville.

#### Questioned

Refused to answer

Denied knowledge or participation

Admitted being at scene but denied participation

Confessed

Admitted possession of stolen property

Other

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Total questioned by patrol officer

Not questioned by patrol officer

Arrest not made by patrol officer

Unclear

Total

#### Table C-15-1

# Questioning of Defendant by Patrol Officer ( (In percent of persons arrested)

	Jacksonville Burglaries Percent		Sa Bur	n Diego glaries	San Diego Robberies		
	Number	Convicted	Number	Convicted	Number	Percent Convicted	
<b>c</b>	4	(100)	31	48	22	41	
	18	56	23	57	16	19	
•							
	9	88	8	38	17	41	
	34	82	26	65	5	40	
on Y	-	-	4	-	_	-	
	_11	36	_32	_41	15	7	
	(86)	(63)	(124)	(49)	(75)	(29)	
	8	75	57	54	87	30	
	60	62	35	66	23	78	
	_56	75	2		_15	7	
	200	70	219	53	200	34	

	Question	ing of Defe	endant by	Detective			
	(In per	cent of per	sons arr	ested)	-		
	Jacksonville San Diego Burglaries Burglaries Percent Percent Number Convicted Number Convicted				San Diego Robberies Percent		
Questioned	Number	Convicted	Number	Convicted	Number	Convicted	
Refused to answer	5	80	13	54	9	89	
Denied knowledge or participation	9	33	10	40	<sup>-</sup> 27	52	
Admitted being at scene but denied							
participation	7	29	7	57	19	42	
Confessed	51	82	27	78	22	77	
Admitted possession of stolen property		-	6	50	-	_	
Other	8	38	8	50	3	(33)	
Total questioned by detective	(80)	(93)	(71)	(61)	(80)	(60)	
Not questioned by detective	3	(67)	31	81	5	60	
Defendant confessed to patrol officer	33	79	16	69	7	29	
Unclear	84	68	100	_36	108	13	
Total	200	70	219	53	200	34	

 $\delta = \frac{1}{2}$ 

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Conflicting-Implausible Statements

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Two or more nonincriminating statements which conflict

Highly implausible confession

How Confession Recorded

Written and signed

Taped

In officer's written report

How Incriminating Statement Recorded

Written and signed

Taped

Written but not signed

In officer's written report

Other

Unclear

No statement made

Total cases

C-30

#### Table C-15-3

# Plausibility and Recording of Confessions and Statements (In percent of persons arrested)

Jacksonville Burglary Percent		San Bur	Diego glary Percent	San Diego Robbery Percent		
Number	Convicted	Number	Convicted	Number	Convicted	
4	(25)	18	44	6	50	
1	(100)	1	-		· <u>-</u>	
31	84	2	(100)	l	(100)	
1	(100)	2	. –	1	(100)	
47	85	46	76	24	75	
3	(33)	-	-	-	. –	
- -	_	2	(50)	4	(50)	
-		:	-	2	<u> </u>	
38	55	59	39	51	31	
-		2	(50)	1		
5	60	1	<b>-</b> <sup>1</sup>	-	-	
_75	63	105	48	115	25	
200	70	219	53	200	34	

# <u>Questioning of Defendant by Patrol Officer</u>. Felony Assault--Stranger-to-Stranger (In percent of persons arrested)

Questian	Jacksonville Percent		San Diego Percent		
Questioned	Number	Convicted	Number	Convicted	
Refused to answer	. –	-	4	(50)	
Denied knowledge or participation	5	20	l	_	
Admitted being at scene but denied					
participation	1	(100)	6	83	
Confessed	8	63	13	62	
Admitted possession of stolen property	-	-	_		
Other	3	(33)	3	(33)	
Total questioned by patrol officer	(17)	(47)	27	59	
Not questioned by patrol officer	3	(1.00)	5	20	
Arrest not made by patrol officer	l	(100)	. · ·		
Unclear	8	63	5	_60	
Total	29	59	37	54	

#### Questioned

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Admitted being scene but denie participation

#### Confessed

Total questioned by detective

Not questioned detective

Defendant confest to patrol office

Unclear

Total

C-32

# Table C-15-5

# Questioning of Defendant by Detective Felony Assault--Stranger-to-Stranger (In percent of persons arrested)

	Jack	sonville	San	Diego
	Number	Percent Convicted	Number	Percent Convicted
at				
	-	-	l	(100)
	_2	(50)		
2d	(2)	(50)	l	(100)
by	27	59	36	53
ssed er	8	63	13	62
	19	_58	_20	50
	29	59	37	54

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# Co-Participant Statements (In percent of persons arrested)

	Jack	sonville Percent	San	n Diego Percent			n denne dat in German	
Robbery	Number	Convicted	Number	Convicted		-	(A more with the second se	
Incriminating statement	-	-	40	58				Usability
Exculpatory statement	-	-	2	(50)			appendiate and the second s	<u>osastatey</u>
Other statement	_	-	6	17				No apparent problem
Unclear	-	- '	8	· _			ender en	Inadmissible
No co-participant statement	-	-	64	27		al de la companya de		Other
No co-participant			80	21		and the second se		Exculpatory only
Total	-		200	34		CONCIMIENT IN 19 10	a chu	No co-participant
Burglary						an a	British de la constante de la c	or no co-participan statement
Incriminating statement	49	84	42	64				Unclear
Exculpatory statement	4	-		-		o I Januar		_
Other statement	<b>–</b>	-	-	-		vite en anti	a) and the second s	Total
Unclear	8	75	2	-	<b>د ب</b>	n a na sua sua sua sua sua sua sua sua sua su		Willingness to
No co-participant statement	51	67	41	61		Change with the second s	ay a sa ang a sa ang a sa ang ang ang ang ang ang ang ang ang an	Testify
No co-participant	88	_66	133	47		and the second	re chi si	Did testify (at
Total	200	70	219	53				disposition or tria
Felony Assault (Stranger-to-stranger)			•			An one of the second	C	Willing but never called
Incriminating statement	-	-	5	80			Compared Section 1	Indicated as willin
Exculpatory statement	2	(50)	-	-		-	rich and a second se	but later refused
No co-participant statement	7	14	4	(25)			in the second	Other
No co-participant	20	_75_	28	_54			approved a second s	No mention of
All cases	29	59	37	54				Proprem
								Unclear

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No co-participant

Total

(7)

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# Table C-16-2

# Usability of Co-Participant Statements (In percent of persons arrested)

	Jacks	Burgl onville	ary San	Diego	Rob San	bery Diego
	Number	Percent Convicted	Number	Percent Convicted	Number	Convicted
າຣ	56	77	41	66	46	50
			2	-	-	_
	2	-	l	· _ ·	1	(100)
	-	_	-	-	1	(100)
nt-						
	129	67	174	51	144	29
	8	75	1		8	
	200	70	219	53	200	34
J, al)	3	(100)	_	-	_	-
	-	_	l	(100)	2	(100)
ng	_	-	-		l	-
	-	-	<b>-</b> .	-	2	(50)
	106	72	77	65	88	41
	3	(100)	5	_	7	29
	88	65	136	47	100	26
	200	70	219	53	200	34

# Use of Car as a Link (In percent of persons arrested)

		Burgl	ary	Rob			
	Jacksonville San Diego			San			
Defendant's		Percent		Percent		Percent	
Car Linked	Number	Convicted	Number	Convicted	Number	Convicted	
By license number	7	43	6	50	20	55	
By description plus partial number	l	(100)	-	-	2	(50)	
Similar to description	3	(100)	3	(100)	6	17	
Other	_	-	3	(33)	2	(100)	
Co-defendant's Car Linked							
By license number	6	50	3	<b>-</b> . *	8	50	P
By description plus partial number	-	_	· _		3	(33)	4
Unusual description	-	-	· _	-	1	(100)	
Similar to description	-	-	1	(100)	1	-,	
Other	1	-	1	-	1		
Other Situations							
Not linked	15	61	20	45	9	22	
No car seen or defendant arrested near scene and no							
need to trace	163	74	181	55	137	28	
Unclear	4	(50)	1		2		
Total	200	70	219	53	200	34	

Defendant's car

Defendant had acces to car linked

Co-defendant's car

Other

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Unclear

C-36

e Carana da Santa da Carana da

# Table C-16-4

# Whose Car Linked? (In percent of persons arrested)

		Burglary Robbery					
	Jacks	onville	San	Diego	San	Diego	
	N7	Percent		Percent	-	Percent	
	Number	Convicted	Number	Convicted	Number	Convicted	
	9	55	8	75	25	60	
ss							
	3	(33)	1	-	7	43	
	7	43	5	40	16	44	
	-	. –	2	-	4	(50)	
	l	(100)	1	-	2		

(FI)

Suspect	's Car	SeenWhet	her Linko ns arres	ed or Not ` ted)			
(11	Jack	Burg sonville Percent Convicted	lary San Number	Diego Percent Convicted	Rol San Number	bbery Diego Percent Convicted	-
-	NUIIDEL	CONVECTO					
Yes, full license number obtained	15	47	20	50	30	53	
Yes, partial number obtained	1	(100)	1	-	5	40	
only; license number not obtained	13	46	15	47	20	55	
vos other	_	-	<del>89</del> .	-	3	-	
Unclear	1	(100)	-	-	-	-	
Car apparently not seen	51	65	92	37	104	30	
Arrested at scene							
so car trace not	119	77	91	70	36	19	
needca	200	70	219	53	200	34	-
TOTAL		19	36	47	58	50	
Car seen	29	40		47	16	48	1
Car linked	18	44	17	41	40		, , , , , , , , , , , , , , , , , , ,

# Robbery

6-3

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Defendant's Wea Impounded

.Fired in crim confirms as s

Weapon used i similar weapo

Weapon <u>not</u> us but weapon im

Other

Co-Defendant's Impounded

> Fired in crim confirms as s

Weapon used i similar weapo

Weapon <u>not</u> us but weapon im

#### Unclear

### Burglary

Defendant's w impounded at

Defendant's w impounded oth at scene

Co-defendant impounded at

C-38

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#### Table C-16-6

### Impounding of Weapon (In percent of persons arrested)

	Jacks	sonville	San	San Diego		
	Number	Convicted	Number	Convicted		
apon						
me, ballistics same	· - ·	-	- 1			
in crime, on impounded	-	· _	43	23		
sed in crime npounded	-	-	7			
	- <u>-</u>	-	l	. –		
Weapon						
me, ballistics same	—	<b>_</b>	; <b>-</b>	-		
in crime, on impounded		-	18	39		
sed in crime npounded	-	-	58	35		
	-		3	(33)		
weapon scene	4	(100)	6	50		
weapon her than	3	(100)	Ę	20		
's weapon	<b>.</b>	(100)	2			
scene	1	-	1	-		

	Re (In perc	ecovery of E ent of pers	property sons arre	sted)	•			
From Dofondant	Burgl Jacksonville Percent		.ary San	ary San Diego Percent		bery Diego Percent		
		- CONVICLED	IN UNIDEL		MULLOEL	CONVICTED		
Clearly linkable	24	79	40	70	23	61		
Probably linkable	42	74	23	70	11	73		
Similar property	1	·(100)	3	-	1	-		
Similar denominations	-	_	-		19	53		
Other	-	· –	1	-	4	· _		
From Co-Defendant								
Clearly linkable	6	83	7	29	11	36		
Probably linkable	5	60	1	-	5	60		
Similar denominations	-	-	-		3	-		
							~	
From Third Party or Place								
Clearly linkable	7	57	15	87	4	(50)		
Probably linkable		-	9	22	2	(33)		
Similar property	9	100	· _	-		-		
Money taken from defendant or co-defendant at arrest, but amounts	· · ·				5	20		
NOC TINVEDIC	<b>ب</b>		-	-		20		
None recovered	21	29	25	24	77	18		
No loss	76	75	86	50	26	31		
Unclear	6	67	2	50	-			
Similar denom- inations recovered	1	-	1		· ·	-		

Serial number, engraved ID

Identifying marks

Victim identified

Description matched

Other

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Unclear how linked

Not linked

No property stolen or none recovered

#### Table C-16-8

# How Property Linked? (In percent of persons arrested)

Burglary				Robbery		
Jacks	onville	San	Diego	San Diego		
	Percent		Percent		Percent	
Number	Convicted	Number	Convicted	Number	Convicted	
34	77	43	65	31	48	
-	-	4	(50)	2	(100)	
36	81	39	64	13	69	
24	71	12	42	28	39	
3	(33)	4	(75)	15	47	
4	(75)	2	· · _ ·	3	-	
3	-	3		1	-	
96	68	112	44	107	21	

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# Impounding of Burglary Tools (In percent of persons arrested)

		Jack	Jacksonville		Diego
	•	Number	Percent Convicted	Number	Percent Convicted
Yes	•	38	74	52	69
No	•	162	69	167	
Total		200	70	219	53

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Arrested near scene

> Distinctive clothing

Non-distinct clothing

#### Arrested later

Distinctive worn at arre

Other distin clothing mat description impounded

Non-distinct clothing wor arrest

Other non-di tive clothin matching des tion impound

Other

Impounded bu unclear why

Suspect not se no clothing de available

No clothing impounded

#### Table C-16-10

# Impounding of Defendant's Clothing for Descriptive Purposes (In percent of persons arrested)

		Burgl	ary		Rob	bery
	Jacks	onville	San	Diego	San	Diego
		Percent		Percent		Percent
	Number	Convicted	Number	Convicted	Number	Convicted
	. 1	(100)	6	17	16	44
ive	l	(100)	l	· _'	7	43
<u>.</u>						
clothir st	ng _	-	-	-	7	14
ctive ching						
	-	-	_	. –	2	(100)
ive						
n at	· _	-	1		2	(100)
lstinc-						
scrip- led	-				-	-
	1	-	2	(100)	6	33
it	_	-	_	-	2	· _ ·
een or escript:	ion					
	-	. –	-	-	2	
	197	70	186	53	156	34

# Fingerprints (In percent of persons arrested)

Burglary

San Diego

Number Convicted Number Convicted Number Convicted

5

Percent

80

Jacksonville

4

Good prints

Partial prints

matching defendant's

matching

matching

defendant's

Good prints

co-defendant's

Percent

(100)

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Robbery

San Diego

2

Percent

(100)

# Other Scientific Evidence

(~~)

1

1 ~

Yes, incrimina

Unclear

Apparently not

Total

# Other Physical Evidence

Yes, incriminat to defendant

Yes, incriminat: to co-defendant

Apparently not

Total

Other Evidence
Yes
No
Unclear
Total

~				
-	-	4	4	

1 (100) 1 (100) 5 20 2 (50) Good prints which

match neither defendant nor							
co-defendant	4	(50)	3	(67)	2	(50)	
Prints taken, no match	30	67	15	67	13	38	
Prints sought but not obtained	20	95	25	36	9	33	
Fingerprints applicable but							
not sought	10	80	76	65	4	(25)	
Other	1	(100)	4	(25)	5	60	
Unknown	9	66	: <b></b>	-	6	33	
Not appropriate	114	67	88	40	159	31	
At scene arrest	2	(50)	2	(50)	-	_	

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# Table C-16-12

# Other Evidence (In percent of persons arrested)

		Burg	lary					
C ·	Jacks	Jacksonville		San Diego		Robbery San Diogra		
	Number	Convicted	Number	Percent _Convicted	Number	Percent		
ating	2	(100)	10	80	1	(100)		
	-	-	: —	_	21	(100)		
:	198	69	209	_51	197	33		
•	200	70	219	53	200	34		
						•••		
ing								
2	38	74	63	73	22	50		
ing	1	(100)	1		-			
	160	68	155	45	177	-		
	200	70	219	53	200	32		
						74		
	2	(50)	1.0					
	100	(30)	18	67	25	32		
	198	70	201	51	174	34		
-		<u> </u>			1			
	200	70 2	219	53	200	34		

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# Victim-Witness Availability Problems

	Jackson	ville	San D	iego	,
	Number of Victims and Witnesses	Number of V-W Problems Ending in	Number of Victims and Witnesses	Number of V-W Problems Ending in	
Unavailable for:	With Problem	Conviction*	With Problem	Conviction*	
In Robbery Cases					
Police follow-up	2	. I	3	-	-
Photo line-up			1	- · · · ·	
Prosecution generally	12	<del>_</del>	28	6	
Either police follow-up or prosecution	7	_	18	_	
Preliminary hearing	-	_	9	5	
Trial	2	·	14	14	ŕ
Either preliminary hearing or trial	-		1	1	
Either live lineup or trial			3	3	
Total	23	1	78	29	
In Burglary Cases					:
Police follow-up	2	2	3		
Prosecution generally	-	<del>.</del>	l		
Either police follow-up or prosecution	6	1	4	2	
Preliminary hearing	_	_	7	6	
Trial	<u> </u>		2	2	rs (1)Mentrum (4)
Total	8	3	17	10	

\*As the base for this figure is the victim or witness rather than the sample event or the case, the ratio of convictions to problems is not the same as the percentage of sample events ending in conviction. Action Taken

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Letter sent

Investigator tr to find

Investigator tri to find and lett sent

Investigator tri to find and othe

Transportation a

Investigator tri to find and tran tation arranged

Transcript or der used

Plea accepted

Other

Nothing

Unclear

Total

Success of Efforts

Succeeded in makin V-W available

Failed to make ava

Nothing done

Unclear

Total

\*As the base for this figure is the victim or witness rather than the sample event or the case, the ratio of convictions to problems is not the same as the percentage of sample events ending in conviction.

# Table C-17-2

# What Was Done About Unavailability of Victim-Witness--Robbery

	Jackson Number of Victims and Witnesses With Problem	Number of Number of V-W Problems Ending in Conviction*	San M Number of Victims and Witnesses With Problem	Diego Number of V-W Problems Ending in
	3	_		conviction*
ied			<del>-</del> .	-
-	9	1.	7 7	
ied ter				3
	2	•		
ied		-	1.	-
er	-	<b></b>	<b>1</b> .	
arranged	1	· _	1	<b></b>
.ed				7
spor-	_			• .
position		<b>-</b>	3	3
		-		
	1	ан сайтан са Тама сайтан с	8	8
	1	. <b></b>	5.	5
	4	_	5	l
	1	_	36	2
	23		<u>    1                                </u>	
		1	79	29
5				
ng				
	3	-	21	
ilable	15	l	10	17
	5		36	9
			1	2
	23	1	<u> </u>	
nis figur	e is the vict	im an		28

C-47

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# Victim-Witness Unwilling to Assist--Robbery

	Jacksonville		San Diego		
	Number of Victime and	Number of	Number of	Number of	
V-W Uprilling	Witnesses	v-w Problems Fnding in	Witnoscos	V-W Problems	
to Assist With:	With Problem	Conviction*	With Problem	Conviction*	
				CONVICTION	
Preliminary hearing	·	-	4	, <del>-</del> , , , , ,	
Follow-up generally		-	1	<b>-</b> .	
Prosecution generally	31	2	17	1	
Police follow-up and					
prosecution	-	<b>_</b>	5	<b>_</b> ·	
Other	2	<u> </u>	-		
Total	33	3	27	1	
How Know Unwilling?					_
V-W failed to show up	7	-	4	· -	4
Declined verbally	11	1	17	l	
Declined in writing	14	1	2	• – • •	
Other	1	-	5	1	
Unclear			1	-	
Total	33	2	29	1	

\*As the base for this figure is the victim or witness rather than the sample event or the case, the ratio of convictions to problems is not the same as the percentage of sample events ending in conviction.

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Persuaded to as Subpoenaed

Action Taken

Other

Nothing done

Unclear

Total

Success of Effor

Became willing

Continued unwill

Nothing done

Unclear

Total

C-48

# Table C-17-4

# Efforts to Deal with Victim-Witness Unwillingness to Assist--Robbery

	Jacksor Number of	ville	San I	Diego
	Victims and Witnesses With Problem	Number of V-W Problems Ending in Conviction*	Number of Victims and Witnesses With Problem	Number of V-W Problems Ending in
ssist	-	-	2	COnviction*
	2	-	1	-
	2	1	1	-
	27	2	22	
			3	
	33	3	29	1
ts				
	1	-	4	1
ing	4	<b>-</b>	1 .	- -
	27	2	24	1
	1			~
	33	2	29	2
this fi			•	-

\*As the base for this figure is the victim or witness rather than the sample event or the case, the ratio of convictions or problems is not the same as the percentage of sample events ending in conviction.

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Entry

# Bearing of Availability or Unwillingness on Dispositions

						1						
Robbery	Jackson Number of Victims and Witnesses With Problem	nville Number of V-W Problems Ending in Conviction*	San I Number of Victims and Witnesses With Problem	Diego Number of V-W Problems Ending in Conviction*	- - - - - - - - - - - - - - - - - - -		<u>Role</u> Vi	ctim	Jacksor Number of Victims and Witnesses With Problem 83	ville Number of V-W Problems Ending in Conviction*	San D Number of Victims and Witnesses With Problem	Diego Number of V-W Problems Ending in Conviction*
Case weakened and plea taken	_	<u> </u>	13	13	1		Ci	vilian witness	10	1	120	10
Case dismissed	13	_	9				Tune	Fuidence Provide	ad the second seco			
DA reject	28	l	10			ute call where	<u> 1996</u>	DVIdence FIOVIde	<u>.</u>			
Police release	_		29	_		e generation and a second s	Sa	w suspect, can su	ipply 14	3	16	2
Unclear	-	_	7	:					<b></b>	5	10	<b>ک</b>
None	6	2	24	15			Ma	iđe ID	50	16	82	39
Total	47	3	92	28		an fair an	Ga	ve license number	-	· _	4	1
D							Ot	her	6	1	l	-
Burglary					the second		Un	clear	5	_		
Case weakened and plea taken	<del>.</del>	ананананананананананананананананананан	2	2			) Know	s Defendant				
Charges reduced	3	3	<b>-</b> .		1	والمراجع والمراجع	Bv	name	22	7	8	2
Case dismissed	-		1	. –		najiji te na			_		· _	
DA reject	17	-	3	-			Ву	sight	2	· <u></u>	1	1
Police release	<u> </u>	_	5	_			Un	clear	2	<b>—</b> •	13	1
Unclear	<b>-</b> .		l				Tota	1 Problem V-Ws	93	23	136	46
None	2	2	10	7	in Physers, Les						•	
Total	22	5	25	10			*As samp	the base for this le event or the c	figure is the ase, the ratio	victim or witr of convictions	ess rather that to problems i	n the s not the

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\*As the base for this figure is the victim or witness rather than the sample event or the case, the ratio of convictions to problems is not the same as the percentage of sample events ending in conviction.

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### Table C-17-6

Role of Victims and Witnesses--Robbery .

sample event or the case, the ratio of convictions to problems is not the same as the percentage or sample events ending in conviction.

by	<u>Formal App</u> Problem Victi	MS or Witness	,		, , ,
		withesse			
Robbery	Jacksor Number of Victims and Witnesses With Problem	Number of Number of V-W Problems Ending in Conviction*	San ) Number of Victims and Witnesses With Problem	Diego Number of V-W Problems Ending in Convictiont	
Trial	1	-	1	1	
Deposition	5	1	<u>_</u>	_	
Preliminary hearing	-	, · · <del>-</del>	26	24	
Preliminary hearing and trial	. <b>–</b> 11	-	6		
Preliminary hearing and grand jury	- -	· ·	1	0	
Other	_1		_	1	
Total	7	1	34	32	
Burglary					
Deposition	2	2	-		
Preliminary hearing	-	-	6		
Preliminary hearing and trial	-	-	· · · ·	-	
Unclear	6	6	. <b>-</b>	1	
Total	8	8	8	8	

\*As the base for this figure is the victim or witness rather than the sample event or the case, the ratio of convictions to problems is not the same as the percentage of sample events ending in conviction. ()

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Availability pro

.Unwillingness pr

Availability and unwillingness

Credibility prob

Availability and credibility

Unwillingness an credibility

Availability, un and credibility

Total cases with

Total cases

Availability--to

Unwillingness--t

Credibility--tot

#### Table C-17-8

### Victim-Witness Problems--Robbery (In percent of persons arrested)

	Jacks	onville	San	Diego
		Percent		Percent
	Number	Convicted	Number	Convicted
oblems	- -	- -	29	28
roblems	9	11	7	-
1		- -	7	_
olems	22	45	27	44
1	4	(100)	21	48
nd	15	5	6	<u>-</u>
willingness				
	<u> </u>		4	50
n problems	47	21	101	32
	200	50	200	34
stal	6	-	61	33
otal	26	10	24	8
al	44	33	58	41

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# What Was Done About Unavailability of Victim-Witness--Burglary

Action Taken	Jackson Number of Victims and Witnesses With Problem	ville Number of V-W Problems Ending in Conviction*	San D Number of Victims and Witnesses With Problem	niego Number of V-W Problems Ending in Conviction*	- A GATTAN AMAZATTAN		V-W Unwilling to Assist With: Prosecution gene
Letter sent		- -	1	-			Police follow-up
Investigator tried to find	7	3	4	4			Other
Transportation arranged	-	-	2	2			Not clear
Other	l	-	_	. <b>-</b>			Total
Nothing	- 	<b></b> 2	9	4		:	How Know Unwilli
Unclear			<u> </u>	جسين بني			
Total	8	3	17	10			V-W failed to sh
						V and the second	Declined verball
Success of Efforts			• •	•			Declined in writ
Succeeded in making V-W available	2	2	5	4			Unclear
Failed to make availa	ble 6	1	2	2			Total
Nothing done	-		9	4			*As the base for sample event or
Unclear				•••••			same as the perc
Total	8	3	17	10			

\*As the base for this figure is the victim or witness rather than the sample event or the case, the ratio of convictions to problems is not the same as the percentage of sample events ending in conviction.

#### Table C-17-10

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#### Victim-Witness Unwilling to Assist--Burglary

	Jackson	ville	San D	iero
lling st With:	Number of Victims and Witnesses With Problem	Number of V-W Problems Ending in Conviction*	Number of Victims and Witnesses With Problem	Number of V-W Problems Ending in Conviction*
ion generally	10	2	4	_
follow-up and tion	5	-	2	-
	-	·	1	-
r	3	<u> </u>	<u> </u>	
	18	2	8	_
Unwilling?				
ed to shut up	2	1	3	_
l verbally	7	1 · · · 1 ·	4	-
l in writing	6			. –
	3	<b></b>	<u>    1</u>	
	18	2	8	-

base for this figure is the victim or witness rather than the event or the case, the ratio of convictions to problems is not the the percentage of sample events ending in conviction.

# Efforts to Deal With Victim-Witness Unwillingness to Assist-~Burglary

	Jackson Number of Victims and Witnesses With Problem	ville Number of V-W Problems Ending in Conviction*	San D Number of Victims and Witnesses With Problem	iego Number of V-W Problem Ending in Conviction*
Action Taken		_	3	1
Other	-		10	1
Nothing done	7		12	-
Unclear	1			-
Total	8	-	19	2
Success of Efforts			2	1
Continued unwilling	-	-	c	-
Nothing done	7	- ·	11	-
	1		4	
Unclear		-	19	2
Total	8			-

\*As the base for this figure is the victim or witness rather than the sample event or the case, the ratio of convictions to problems is not the same as the percentage of sample events ending in conviction.

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#### Jacksonville San Diego Number of Number of Number of Number of Victims and V-W Problems Victims and V-W Problems Witnesses Ending in Witnesses Ending in Role With Problem Conviction\* With Problem Conviction\* Victim 32 10 24 11 · Civilian witness 11 12 4 7 Police officer 2 2 1 2 Type Evidence Provided Saw suspect, can supply name or address 11 12 8 6 7 Made ID 2 3 Gave information regarding stolen property 1 Police officer who made investigation at scene of burglary 18 7 14 6 7 Other loss, etc. Λ Δ 1 Unclear 2 Knows Defendant 25 By name 3 2 6 By sight 4 1 1 1 Unclear 8 7 3 Total Problem V-Ws 45 16 38 19

\*As the base for this figure is the victim or witness rather than the sample event or the case, the ratio of convictions to problems is not the same as the percentage of sample events ending in conviction.

#### Table C-17-12

#### Role of Problem Victims and Witnesses--Burglary

# Victim-Witness Problems--Burglary (In percent of persons arrested)

	Jacksonville Percent		San D	iego Percent
	Number	Convicted	Number	Convicted
vailability problems	2	(100)	9	56
Unwillingness problems	11	18	6	· -
Availability and	1	(100)	· · · -	_
Credibility	6	17	6	50
Availability and credibility	4	<b>_</b>	1	(100)
Unwillingness and credibility	l	-	2	-
Availability, unwilling- ness and credibility	_1			
motal cases with problem	s 26	23	24	38
Total cases	200	69	219	53
Availabilitytotal	8	38	10	60
Unwillingnesstotal	14	21	8.	· –
Credibilitytotal	12	13	9	44

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d, Burglary and theft are combined for Baltimore; there is no weapons offense comparable to those in Chicago and Detroit.

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### Table C-19-1

# Characteristics of Defendants And Cases in Baltimore, Chicago, And Detroit

	Baltimore <sup>a</sup>	Chicago <sup>b</sup>	Detroit <sup>a</sup>
Percentage black	81.5	69.9	82.7
Percentage over 21 years old	66.4	70.2	63.0
Percentage with previous			
record	58.1	missing	66.0
Offenses	-		
Murder	3.8	1.7	3.6
Rape	5.1	1.1	1.4
Armed Robbery	16.2	8.5	10.3
Assault	7.3	5.8	7.6
Heroin dealer	11.8	22.0	1.6
Heroin use	2.2	6.7	7.8
Burglary	28.4	8.5	12.5
Theft	n.a. <sup>d</sup>	6.5	4.8
Robbe-y	10.9	4.1	2.9
Weapons	n.a. <sup>d</sup>	4.0	17.4
Other	14.2	15.0	30.1
Evidence			
Photo identification	8.7	missing	4.6
Lineup identification	1.2	missing	13.0
Eyewirnesses	13.0	10.0	4.7
Confession	9.7	2.8	26.0
Physical Evidence	68.1	78.3	86.0

a. Weighted file sample: N = 1577.

b. Weighted preliminary hearing observation sample: N = 1,015.

e. Weighted observation sample: N = 1,640.

Source: J. Eisenstein and H. Jacob, Felony Justice 206, 242 (1977).
# Table C-19-2

# Factors Involved in Preliminary Hearing Decision and Decision to Convict (In percent of explanation due to factor indicated)

Preliminary Hearing Decision	Baltimore	Chicago	Detroit
Identity of courtroom	82	79	60
Prior record	5	0	12
Pretrial release	1	1	20
Pace	0.2	5	0.3
Fuidence	l	0	1
Type of counsel	7	0	0
Original offense	4	16	7
Total amount of variance explained	80	55	17
Decision to Commit or Acquit at Trial			
Identity of courtroom	16	18	40
Strength of evidence	0	5	34
Defendant characteristics	55	12	7
Original offense	29	65	19
Total amount of variance explained	12	15	17

Source: J. Eisenstein and H. Jacob, Felony Justice 206, 242 (1977).

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		General Felony Preliminary	Drug Court <u>Hearing</u>	Drug Court Revised Dismissals	Guilty Plea	Decision to go to 	Decision to go to Trial Revised*
	Case seriousness	7	2	2	-	0	0
	Strength of state's case	9	1	1	_	0	0
	Prior arrest record	2	3	2	3	· 🗕	-
	Presence of defense counsel	7	2	2	-	_	·
	Legal motions	1	14	<u> </u>	_	8	8
	Resisted arrest	3	2	-	-	-	· _
	Type offense	2	2	-	-	_	-
	Interaction of serious ness and strength		-	3	-	3	3
	Other pending complain	ts -	_	14	-	. <del>-</del> ·	-
٢	Interaction of counsel and counsel responsive ness	- ;- -	<u> </u>	2		- -	 
	Confined or not	-	-	2	-	-	· _
	Months spent in confin ment	e- -	_	3		а <u>—</u>	-
	SES	-	-	-	l	-	-
	Indictments	—	-	. · <b>-</b> ·	. 14	-	
	Judge's responsiveness	-	-	-	1	-	-
	Prosecutor's respon- siveness	_		-	3		
	D's request for plea bargaining session	_		<u> </u>	19	—	-
	Session	-	-	-	_	-	3
	Defense counsel responsiveness	-	· -	· <u>-</u>	- -		4
	Total percent of variance explained	30	25	30	40	12	21
	*Note: These columns p	redict only	the decis	ion to go to	trial,	not the di	sposition.
	Source: P. Nardulli, Criminal Just	The Courtroc	m Elite: , 171, 18	An Organizat 8, 194, 197	ional Pe (1978).	rspective	on

# Table C-19-3

# Preliminary Hearing and Trial Court Decisions (In percent of explanation due to factor indicated)

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# Table C-19-4

# Regression Results on the Probability of Conviction in Robbery Cases (Superior Court, Washington, D.C., 1974)

Independent Variables	Estimated B	Significance Level
1 witness	.2591	<.001
2 witnesses	.3074	<.001
3 witnesses	.3159	<.001
4 witnesses	.3469	<.001
5 or more witnesses	.4546	<.001
Codefendants in case	0435	<.06
Case seriousness		
(Sellin-Wolfgang Index)	0040	<.05
Defendant abuses alcohol	1743	<.05
Stranger-to-stranger	.0725	<.05
Time from offense to arrest		
30 minutes or less	0852	<.01
Property or evidence recovered	. 1453	<.001
Defendant is employed	0600	<.05
Defendant uses opiates	.0733	<.08

Note: N = 1.790

Intercept = .0820

Multiple  $R^2 = .108$ 

Includes only cases that were closed at the time of the analysis.

Source: K. Williams & J. Lucianovic, Robbery and Burglary 60 (March 1979) (Institute of Law and Social Research).

# Indepen

1 witness 2 witnesses 3 witnesses 4 or more witnes: Property or evide Firearm used dur Defendant's age Stranger-to-strang Time from offense 30 minutes or

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Note: N = 1,320Intercept = .3040 Multiple  $R^2 = .092$ Includes only cases that were closed at the time of analysis and that were originally brought as either a first- or second-degree burglary.

Source: K. Williams & J. Lucianovic, Robbery and Burglary 60 (March 1979) (Institute of Law and Social Research).

# Table C-19-5

# Regression Results on the Probability of Conviction in Burglary Cases

ndent Variables	Estimated B	Significance Level
	.2455	<.001
	.2269	<.001
	.2338	<.001
ises	.2467	<.001
ence recovered	.1150	<.001
ring offense	.2666	<.001
	0059	<.001
ger	. 1013	<.01
se to arrest		
more	0581	<.05

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End

# Table C-19-6

# The Decision to Prosecute Fully (Y1) Standardized Regression Coefficients and Related Statistics for Statistically Significant (p .05) Variables<sup>a</sup>

	Variable		r	Metric Coefficient (Standard Error)	Standardized Coefficient
x	Eyewitness Identification		.101	.105 (.023)	.223
×2	Confession of Defendant and/or Accomplice(s)		.037	.06 <b>7</b> (.030)	.032
X <sub>4</sub>	Recovery of Stolen Property		.073	.149 (.036)	.161
Xé	Amount of Nonexpert Testimony		.026	.058 (.023)	.108
x11	Victim-Defendant Relationship		022	063 (.023)	185
X <sub>12</sub>	Victim Prosecution Preference		.184	.325 (.059)	.321
X <sub>13</sub>	Prior Victim-Defendant Conflict		.046	.009 (.002)	.098
X <sub>17</sub>	Defendant Age		082	005 (.002)	090
X <sub>21</sub>	Counsel		093	083 (.032)	038
×23	Racial Composition a. Black defendant-white victim vs. black intra-racial		.069	.158 (.041)	.092
	b. White intraracial vs. black intraracial		010	.147 (.040)	.149
×24	Prosecution Charge		.132	.022 (.004)	.418
		R <sup>2</sup> N	.120 418		

aThe metric and standardized coefficients are taken from the weighted least squares solution, while the zeroof the correlations and R<sup>2</sup> are taken from the ordinary least squares solution.

Source: Myers and Hagan, Private and Public Trouble: Prosecutors and the Allocation of Court Resources, 26 Social Problems 439, 447 (1979).

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Vacable X<sub>1</sub> Exercises Identifica X3 - Weapon Decovered

X5 Account of Expert To

Y Victan Sex

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X10 Victim Colpt: ymen

X14 Alleged Victor Mar

X<sub>19</sub> Defendant Proc Pro

X<sub>22</sub> Amount of Bood

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# Table C-19-7

ze nan na na nan an nan na na na na na na	ſ	Metric Coefficient (Standard Error)	Stard odized Coefficient
bon -	.163	.164 { U10}	. 91
	065	.221 ( 067)	.167
stimuny	.064	.105 (درب )	,101
	.051	.135 (.057)	.30 <b>6</b>
	.108	(304 (3001)	.97
Status	007	.187 (.014)	(.37
confact	0.33		
ord	.236	0'52	.140
	.193	39 x 10 <sup>5</sup> (.17 x 10 <sup>5</sup> )	.104
R <sup>2</sup>	137 370		. •

The Decision to Try the Case (Y<sub>1</sub>). Standardized Regression Coefficients and  $C_{1}$  and  $C_{2}$  and

• The metric and star-tar for die off-dialists are taken from the weighted least squares solution, while the zero-order correlations and R<sup>2</sup> are taken from the ordinary least squares solution. See *unita*, locar rise 4.

Source: Myers and Hagan, Private and Public Trouble: Prosecutors and the Allocation of Court Resources, 26 Social Problems 439, 449 (1979).

# Table C-22-1

# Arrest Relationship to Sample Event Felony Assault--Stranger-to-Stranger (In percent of persons arrested)

( 111	percenc	OT	persons	arresteu

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	Jacksonville		San I	Diego
	Percent			Percent
	Number	Convicted	Number	Convicted
On scene or near on scene arrest for sample event within one hour	20	60	30	53
Later arrest for sample event of specific offender known by name or address	7	57	7	57
Unclear	2	(50)		
Total	29	59	37	54

Robbery Sample ever

Arrest for:

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Sample ever

Other offer

Other offen

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Total

Burglary

Sample event

Sample event

Other offense

Other offense

Other

Total

# Table C-22-2

# Use of Warrants--Robbery (In percent of persons arrested)

	Jacksonville Percent		Sar	Diego
	Number	Convicted	Number	Percent Convicted
<b>N</b> h				
ent non-warrant	187	48	173	34
nt warrant	5	80	7	100
nse non-warrant	-		18	100
nse warrant	· _	. <b>-</b>	T	4.4
	5	_60	-	-
	200	50	200	
			200	34
non-warrant	194	62	198	E A
warrant	5	60	8	. 54
e non-warrant	1	(100)	8	88
e warrant	-		2	-
	-		2	(50)
	200		2	(50)
	200	70	219	53

C-67

# Appendix AA

# THE "SOCIOLOGICAL HYPOTHESIS" REVISITED: EXTRA-LEGAL DEFENDANT CHARACTERISTICS AND THE CONVICTION RATE

The influence of social inequality on decision-making is a central issue in the study of criminal justice. Critics frequently argue that defendants who are poor, unemployed, residentially unstable, or minority-group members have a disproportionate likelihood of arrest, prosecution, conviction, and harsh sentencing. Researchers have sought to document and explain unequal justice by demonstrating that defendants' "extra-legal" attributes are causal factors in official decision-making.

According to Hagan, research of this kind takes a "sociological viewpoint":

The dependent variables given prominence by this approach include the race, sex, age, and socio-economic status of the defendant. Although such variables are <u>presumably</u> irrelevant to...[official decisions], sociologically-oriented\_1 studies have attempted to detect their extra-legal influence.<sup>1</sup>

Hagan calls the rival hypothesis a "legalistic" viewpoint. This alternative outlook focuses on "factors emphasized in official-normative descriptions" of criminal justice. In the case of sentencing (the subject of Hagan's discussion), such factors include "the defendant's prior conviction record and the nature and number of the charges presently brought against him."<sup>2</sup> Though these statements reflect an impoverished conception of sociological analysis, they also raise an important question. The ideal of even-handed administration and the threat of economic and racial discrimination are major concerns for students of American criminal justice.



This chapter examines a number of empirical studies bearing on the "sociological hypothesis" as an explanation of conviction and nonconviction dispositions in criminal cases. Little support for the hypothesis can be found in existing research on arrest practices and sentencing decisions. A recent literature review on the determinants of police behavior concludes that the defendant's sex is the only "extra-legal" characteristic consistently associated with the probability of arrest; findings concerning the effects of race, age, and socio-economic status on arrest decisions are contradictory and subject to divergent interpretations.<sup>3</sup> Hagan's original conclusion concerning the negligible predictive power of "extra-legal" factors with respect to sentencing decisions has been re-affirmed in a later review.<sup>4</sup> As shown below, available empirical research on prosecution and adjudication justifies a similar conclusion: there is no evidence that "extra-legal" defendant characteristics are significant determinants of conviction-nonconviction dispositions.

The following analysis employs the same criteria Hagan used to evaluate sentencing studies:

AA-2

- (1) Research should distinguish between statistical and substantive significance. To establish that two variables occur together with a frequency greater than chance (statistical significance) is not the same as to determine the strength or size of the relationship between those variables (substantive significance).
- (2) Statistical significance should not be confused with causal significance, which can be established only after testing the original relationship against simultaneous control variables which point to possible alternative explanations of the relationship. Controls for "legal" factors are essential for research on the role of "extralegal" factors in criminal justice.

Ī	Percent of Ma Stamfor	les Convic d, Connect	ted by Rac icut, 1957	<u>e and Age</u> , 7-1961	
Age	W Total Charges	hite Percent Convicted	Ne Total Charges	gro Percent <u>Convicted</u>	Between-Race Two Tail Test
16-24	882	57.5	428	77.3	P<.00006
25-34	517	55.7	482	67.0	₽<.00022
35-44	423	58.2	344	65.7	p =.0216
45-54	381	62.7	124	70.9	p ≈.0802
55-plus	258	74.8	43	79.1	p =.5222
All ages	2,461	59.9	1,421	70.5	₽<.00006

AA-4

Source: Forslund, 1969, p. 142.

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# Table AA-1

(3) It is important to control for offense categories because criminal offenses vary in terms of the kinds of persons who enter the criminal justice system as defendants, the "seriousness" of the accusations against them, and the legal issues their cases entail. These factors may interact in complex ways to produce differences in case outcomes for different offenses.

None of the five studies examined below is without problems, but it is possible to distinguish them according to relative degrees of methodological adequacy. The review shows that as adequacy of research improves, empirical support for the "sociological hypothesis" dwindles.

## A. Methodological Blunders and False Conclusions

Two studies illustrate how unwarranted methodological assumptions can lead to invalid inferences about the effects of "extra-legal" factors on the conviction rate. In the first, Forslund<sup>5</sup> attempted to determine whether black defendants had a higher probability of conviction than white defendants by analyzing dispositions for all cases involving males arrested on criminal charges (excluding traffic violations) in Stamford, Connecticut between 1957 and 1961. After controlling for age and occupation (Tables AA-1 and AA-2), he concluded that racial bias produced a higher rate of conviction for black defendants. Several problems, however, cast doubt on this conclusion.

First, Forslund employed a questionable base for measuring the rate of conviction: the units of count are not defendants (which he sampled), but charges. His data indicate a ten percent difference between conviction rates for charges against black and white defendants. But that figure is an accurate measure of the

To Occupation Cha White collar Blue collar 1.0 Laborer 7 All occupations 2,25

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# Table AA-2

# Percent of Males Convicted by Race and Occupation, Stamford, Connecticut, 1957-1961

W	hite	Ne	gro	
tal rges	Percent Convicted	Total Charges	Percent Convicted	Between-Race Two Tail Test
21	52.7	93	71.0	p<.00046
54	56.2	627	69.7	₽<.00006
75	71.3	681	72.4	p =.3336
50	60.8	1,401	71.1	p<.00006

Source: Forslund, 1969, p. 142.

# Table AA-1

# Percent of Males Convicted by Race and Age, Stamford, Connecticut, 1957-1961

	W1 Total	Percent	Ne Total Charges	gro Percent Convicted	Between-Race Two Tail Test
Age	Charges	Convicted	Charges	CONVICTOR	
16-24	882	57.5	428	77.3	p<.00006
25-34	517	55.7	482	67.0	p<.00022
35-44	423	58.2	344	65.7	p =.0216
45-54	381	62.7	124	70.9	p =.0802
55-plus	258	74.8	43	79.1	p =.5222
All ages	2,461	59.9	1,421	70.5	₽<.00006

Source: Forslund, 1969, p. 142.

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conviction-rate differential between black and white defendants only if all defendants had the same number of charges. Since the number of charges is known to affect the probability of conviction.<sup>6</sup> even a slight difference in average numbers of charges against the two groups of defendants could change the amount of apparent racial discrimiination. For example, suppose that (a) each charge against white defendants in Table AA-1 represents one defendant. Also suppose that (b) one third of the black defendants have two charges each and the rest of the black defendants one charge each and (c) half of the multiply-charged black defendants are convicted of both charges and the other half are convicted of one charge. Under these assumptions, the defendant-based conviction rate among multiply-charged blacks would be 100 percent. Among singly-charged black defendants, the conviction rate would be 53.4 percent--more than six percent lower than among white defendants (all of whom are assumed to have one charge each). The overall conviction rate for blacks would be 68.9 percent. What is the true difference between rates of conviction for black and white defendants? Forslund's failure to use a defendant-based measure of conviction makes it impossible to know.

Related to this problem is a second and equally serious weakness. Although Forslund drew his data from cases of arrested defendants, his analysis focused on charges, <u>i.e</u>. on events that were not independent of each other. Some of the events analyzed--namely, charges against multiply-charged defendants--were more likely to be included in the "sample." That procedure violates the assumption of statistical independence that must be met when using the <u>t</u>-test and other parametric statistics to measure between-group differences.<sup>7</sup> The consequence is that Forslund's findings are weighted toward cases involving multiple charges.

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This defect invalidates his effort to explain the conviction-rate difference as a result of greater prosecutorial leniency toward white defendants.<sup>8</sup> Forslund found that a larger percentage of charges against white defendants (31 percent) were nolled than charges against black defendants (22 percent) and that black defendants were convicted of only slightly more (3.5 percent) charges not nolled than white defendants. Therefore, he concluded, "the greater tendency to nolle charges against white than against Negro males accounts in large part for the differences in conviction rates between the two racial groups."9 That conclusion is correct in terms of the way Forslund defined the "conviction rate." Without knowing number of charges, however, one cannot conclude that white defendants had a lower probability of conviction because they enjoyed an advantage over black defendants with respect to prosecutorial decisions to nolle charges. If whites began with substantially more charges per case than blacks--reversing the hypothetical situation posed above--the findings could indicate either no difference in conviction rates between black and white defendants or a difference favorable to black defendants. In general, then, Forslund's findings are biased because (a) his units of sampling were individuals, not charge and (b) statistical tests of the sort he used require independent

AA-6

random sampling, an assumption he violated when using charges as units of analysis. The study has other weaknesses. It does not control for type or level (misdemeanor-felony) of offense or for evidential strength. Although Forslund noted the possible importance of prior record as a determinant of conviction, he did not have data on defendants' prior records. The value of this study is almost entirely negative: it shows how research on criminal court decision-making should not be done.

A second study employing a defective research design to test the "sociological hypothesis" is Chiricos, Jackson and Walds.<sup>10</sup> Although their study does not deal with nonconviction dispositions, it deserves attention as another example of empirical research yielding highly questionable conclusions about the influence of "extra-legal" factors on criminal court decision-making. The authors examined relationships between a number of defendant and case-related characteristics and judicial decisions under a Florida statute that allows judges to withhold formal adjudication of guilt in felony cases that result in conviction.

Despite a finding or verdict of "guilty" the accused in Florida is not "convicted" until formally adjudicated by the court, a step which follows the verdict. Thus, at the time of sentencing, the court may place the accused on probation and concurrently withhold adjudication of guilt. The accused in this instance is not a "convict," even though he has been found guilty and placed on probation.

The advantage of this procedure for the defendant without a prior felony conviction is that it eliminates some of the stigmatizing consequences of having been convicted of a felony.

This study is methodologically superior to Forslund's in several respects. The investigators used a defendant-based unit of count, "legal" variables as test factors, and controls for type of offense. They concluded, as did Forslund, that race of the defendant was a significant determinant of decisions, noting that "regardless of which social or legal characteristic is introduced as a control on the original relationship between race and adjudication status, blacks are invariably adjudicated quilty more often than whites."<sup>12</sup> They also claimed that decision-making was affected by age and level of education (younger and better-educated defendants having higher probabilities of receiving the privileged disposition of "adjudication withheld"). "Surprisingly," the authors revealed, their analysis failed to show that the defendant's sex and level of occupational skill had any impact on decision-making. But they viewed the overall results as providing strong support for the argument that "extra-legal" factors play a large role in court decisions. To evaluate these conclusions, consider the following three methodological problems. First, data for the study were taken from records on "2,419 consecutive cases received by the Florida Probation and Parole Commission in an eight month period from July 1, 1979, to February 28, 1970."13 In other words, the cases studied came from courts throughout the state of Florida. The investigators aggregated these cases into one sample, however, ignoring almost completely the possibility that inter-jurisdictional differences in decision-making might affect their results. Only one court-related variable was included:

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whether the case had been decided in a general jurisdiction court or a limited-jurisdiction criminal court. The factor is moderately associated with decisions to withhold adjudication, but it does not adequately control for possible differences among local courts in the tendency to withhold adjudication. Therefore, the relationships between "extra-legal" factors and judicial decisions mask the confounding effects of any inter-jurisdictional differences that may have existed. For example, the majority of cases involving black defendants may have come from courts in which judges were likely to adjudicate guilt regardless of race and the majority of cases involving white defendants may have come from courts in which judges were likely to withhold adjudication regardless of race.

Another methodological issue concerns the way the investigators measured type of offense. Rather than controlling for individual offenses, they grouped all offenses into one of three categories--"personal," property," and "other." This may have biased the results of their analysis, since the frequency of decisions to withhold adjudication varied considerably among offenses within the categories, as shown in Table AA-3. In fact the results were not affected by this crude approach to measurement because the authors dropped the offense variable from the analysis after showing that the percentage of decisions to withhold adjudication differed in each of the three categories. Thus, an even more serious problem than the measurement of offense categories is that the study lacks control for type of offense.

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# Table AA-3

Percent c	of Cases Resulting In Decisions	`
<u><u>T</u></u>	o Withhold Adjudication	
		(N)
Personal Offenses	56.7	( 351)
Aggravated assault	58.9	( 241)
Homicide	41.7	( 36)
Robbery	44.7	( 38)
Other	69.4	( 36)
Property Offenses	69.4	(1080)
Burglary	70.0	( 504)
Larceny	72.0	( 250)
Auto theft	56.9	( 51)
Forgery	61.8	( 68)
Other	70.5	(207)

Other Offenses 71.4 (906)

(No further information given)

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Source: Chiricos, Jackson and Waldo, 1972, pp. 501, 564.



Third, the investigators employed a questionable technique for analyzing the effects of "extra-legal" factors on decisions to withhold adjudication. To measure the strength of zero-order relationships between independent variables and the dependent variable, they presented contingency coefficients, chi-square values, and probability statistics. Then they displayed a set of contingency tables with percentages of defendants adjudicated guilty in categories made up of various combinations of "favorable" and "unfavorable" characteristics. The investigators included three "extra-legal" variables (race, age, and education) and four "legal" variables (prior record, representation by private or court-appointed attorney, plea of not guilty or nolo contendere, and trial in general jurisdiction court or limited jurisdiction criminal court) in constructing the tables. Unaccountably, the variable of offense does not appear in the tables. The results show that by adding "favorable" (e.g., no prior felony conviction, white, age 20 or less, private attorney) or "unfavorable" (e.g., two or more prior felony convictions, black, age 21 years or more, court-appointed attorney) characteristics to particular categories, the percentage of defendants in those categories receiving the privileged disposition of adjudication withheld increases or decreases. An illustration is shown in Table AA-4.

A more appropriate strategy would have been to examine the effects of "extra-legal" factors net of the effects of "legal" factors, using some standard multivariate technique such as partial correlation or regression for the purpose.

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Abbreviati N.F.C. : P.F.C. = Ed. 10th Ed. < 10 Age < 2

Age 21

# Table AA-4

# PERCENTAGE OF PROBATIONERS ADJUDICATED GUILTY BY PRIOR FELONY CONVICTIONS AND SEVERAL COMBINATIONS OF SOCIAL CHARACTERISTICS

Favorable Characteristics			Unfavorable Characteristics
(2027) N.F.C. (1426) White $+$ N.F.C. id $\ge$ 10th $+$ White $+$ N.F.C. ge $<$ 21 $+$	26.9 23.3 19.8	59.4 68.4 72.3	P.F.C. (387) P.F.C. + Black (117) P.F.C. + Black + Ed < 10th (65) P.F.C. + Black +
$d \ge 10th + white + N.F.C.$	14.8	76.4	$Ed < 10th + Age \ge 21 (55)$
Viations are as follows: C. = No Prior Felony Convic C. = One or More Prior Felon 10th $+ = 10$ Years or More F < 10th = 9 Years or Less For < 21 = Age 20 or Less 21 + = Age 21 or More	tions y Conv ormal I mal Ed	ictions Education ucation	1

Source: Chiricos, Jackson and Waldo, 1972, p. 566, Table 3.

Instead, the investigators seem to have chosen a technique that both maximizes the apparent effects of "extra-legal" factors and prevents analysis of the extent to which these effects may be due to the impact of "legal" factors (court jurisdiction, offense category, type of plea, prior record) on decision-making. The results of selected first-order controls for prior record are reported at various places in the discussion. Most of those findings indicate that the original zero-order relationships between "extra-legal" factors and case outcomes persist when the single "legal" variable of prior record is introduced as a control. However, the authors downplayed an important inconsistency in their results: an original 12.8 percent difference in percentages of black and white defendants adjudicated guilty dropped to a statistically insignificant difference of 3.5 percent among defendants with two or more prior felony convictions. They also termed an "interesting contradiction"<sup>14</sup> the finding that a substantially higher proportion of defendants with 12 or more years of education and at least two prior felony convictions (84.6 percent) were adjudicated guilty than defendants with 10-11 years of education and two or more felony convictions (50 percent) and defendants with 0-9 years of education and two or more prior felony convictions (72.8 percent). These findings underline the need for analytic procedures more sensitive to interactions among various factors that may affect criminal court decision-making.

# B. Methodological Advances and Anomalous Results

Results from the two studies examined next disconfirm predictions based on explicit theoretical arguments concerning

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the effects of "extra-legal" defendant characteristics on case dispositions. These studies are more adequate methodologically than the investigations by Forslund and Chiricos et al.: both employ controls for type of offense and one tests carefully for causal significance. But neither study considers evidential strength as a possible determinant of prosecution and adjudication decisions. By ignoring a key "legal" variable, the authors of each study conclude that the "sociological" hypothesis may still be a valid explanation of criminal court decision-making. Burke and Turk<sup>15</sup> applied log-linear analysis to data on post-arrest decisions in a 20 percent random sample of adults arrested in Indianapolis in 1964 (N=3,941). With this technique they were able to measure the effects of age, race, occupational status, and prior record (in this case, prior incarceration) on six categories of their dependent variable: (1) non-prosecution, (2) dismissal and acquittal, (3) judgment withheld, (4) suspended sentence and probation, (5) fine and (6) prison. "The results of the analysis," the authors observe, "suggest that assertions of legal system bias against the socially disadvantaged require better evidence than has previously been offered in their support."<sup>16</sup> Let us consider the findings which led them to this qualified endorsement of the "sociological hypothesis."

The principal results are summarized in Table AA-5 which reports effect parameters for each of the independent variables on the first two categories of the dependent variable. (Effect parameters measure differences between expected and

AA-14

# Table AA-5

# Effect Parameters For Case Dispositions By Age, Race, Occupational Status, and Record of Prior Incarceration

	Effect Para	neters, Coffense	Effect Parameters, Controls for Offense		
Independent Variable	No Prosecution	Dismissal, Acquittal	No Prosecution	Dismissal, Acquittal	
Age					
Under 25	.407**	188	.154	097	
25-34	.229*	018	.142	087	
35-49	164	.108	.068	033	
Over 49	473**	.099	364**	.216*	
Race					
White	069	167**	.008	.008	
Nonwhite	.069	.167**	008	008	
Occupational Status					
(Linear effect)	015	.260**	032	.144	
(Quadratic effect)	032	.052	028	.076	
Prior Incarceration					
Yes	.002	319**	010	205**	
No	002	.319**	.010	.205**	
** p ≤ .01 * p ≤ .05					
Source: Burke and Tur	<b>,</b> 1975.				

actual numbers of cases in cells of contingency tables, the form of data for which log-linear analysis is used.) Figures shown in the two columns on the left represent effect parameters without controlling for type of offense. The columns on the right contain effect parameters after the effects of offense have been removed. The findings shown in Table AA-5 prove the importance of controlling for type of offense. Before the effects of offense category are removed statistically, all three "extra-legal" factors appear to affect dispositions. With respect to age, the data indicate disproportionate probabilities for youthful defendants (under 34) to have their cases rejected by prosecutors and for older defendants (over 49) to have their cases prosecuted. Race of the defendant also appears to influence the likelihood of dismissal or acquittal in court, although the effect shown indicates that nonwhites enjoy an advantage over whites. Occupational status appears related to the probability of dismissal or acquittal in the direction predicted by the "sociological hypothesis" (i.e., a positive association between occupational status and the likelihood of dismissal or acquittal). But when offense category is controlled, both race and occupational status are stripped of their effects on dispositions. The only "extra-legal" influence remaining is age, and here the results indicate contradictory effects. Cases against older defendants are disproportionately likely to be disposed by dismissal or acquittal.

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These results did not persuade the authors to reject the "sociological hyppothesis." Instead, they proposed an interpretation emphasizing the "complexity" of relationships between "extra-legal" factors that obscure or distort their true effects on decision-making. Commenting on the disappearance of the effect of occupational status when offense is controlled, they observe:

... a possibily confounding factor is the highly significant relationship between prior incarceration and occupational status (tables not shown here), since the greater chance that lower-class males have been previously incarcerated implies that some part of their risk arises from a factor already shown to generate a higher probability of ... [conviction] irrespective of offense. In addition, the strong relationship between race and occupational status (tables not shown here) introduces still another complication for any simple behavioral interpretation in terms of class culture differences.

The authors advance a similar argument concerning the elimination of the effect of race after controlling for offense:

It would, however, be premature to conclude that race has nothing to do with disposition, for the significance. regardless of offense, of the race by occupational status association and of the three-factor interaction of age by race by prior incarceration [findings reported but not shown] suggests that the race effect may be masked by its complex relations with other factors, and that there may indeed be some discrimination operating in ways not readily described by the simplifying rhetoric charac  $\overline{18}$ teristic of most debates over racism in law enforcement.

This line of interpretation culminates in the following proposal:

The unsurprising finding that race and occupational status are significantly related emphasizes the need not only to test for discriminatory legal processing but also to test for the relative validity of explanations, for whatever bias may be found, emphasizing racism, gn the one hand, or class discrimination, on the other.

Clearly Burke and Turk are clinging tenaciously to the view that decision-making is affected by "extra-legal" factors of each on case dispositions.

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AA-18

despite findings which contradict that view. Several considerations weaken their argument. First, their reasoning concerning "interactions" (i.e., correlations) among the independent variables is speculative and murky in the extreme. The "unsurprising" relationship between race and occupational status, for example, hardly explains why controls for offense eliminate the zero-order effects of these two variables. If official decisions reflect both racism and class discrimination, as Burke and Turk imply, then nonwhite defendants are at a double disadvantage: all are nonwhite and most are lower-status. The effect of race should therefore persist after controlling for offense. In general, when two independent variables are correlated, the problem for research is to partial out the relative effects of each on the dependent variables. Otherwise their zero-order effects will be exaggerated. Burke and Turk seem to employ just the opposite logic by which the correlation between race and occupational status somehow reduces the individual effects

A second difficulty arises with respect to the variable of prior incarceration. Never explaining the meaning of this term (does it refer to jail, prison, or both?), they initially introduce prior incarceration as "the measure representing prior arrest in this analysis."<sup>20</sup> Later, however, they characterize prior incarceration as an aspect of "social disadvantage"<sup>21</sup> and treat it throughout their analysis as simply another "extralegal" variable like age, race, and occupational status. The

finding that the effect of prior incarceration persists after controlling for offense is, in fact, the strongest support for their argument concerning "social disadvantage" and decisionmaking. As discussed below, however, a prior criminal record (including prior incarceration) cannot be seen as a purely "extra-legal" factor.

Perhaps the most serious defect is the investigators' failure to examine the effects of "legal" factors on prosecution and conviction decisions. Instead of considering offense category as a possible determinant of decision-making, they removed the effects of offense category in order to study the effects of "extra-legal" factors. (How this was accomplished is not indicated.) Moreover, they do not consider the possible effects of evidential strength or number of charges. Thus, it seems clear that Burke and Turk have contributed little in the way of substantive findings to the debate over the relative importance of "legal" vs. "extra-legal" factors in criminal court decisionmaking. Rather, it is the methodological contribution they make-controls for offense category--which distinguishes their work.

A study by Bernstein, Kelly and Doyle<sup>22</sup> represents another step in the development of an improved methodology for evaluating the "sociological hypothesis." Using regression analysis, they examined the way decision-making in a high-volume urban criminal court was affected by a large number of "legal" and "extra-legal" variables. This technique allowed them to control for offense, pay attention to substantive significance, and test for causality. However, other methodological problems limit the validity of their results and cast doubt on the implications they draw

from their findings. The principal findings are presented in Table AA-6. This table includes results only for the first two categories of the investigators' dependent variable -- "dismissal" and "adjournment in contemplation of dismissal." (The third category -- "sentence"--is not relevant to this discussion.) Unlike dismissal of charges, adjournment in contemplation of dismissal (ACD) is a disposition in which the defendant is "...adjudicated guilty but not formally convicted unless...rearrested and charged with a new offense in the six-month period following the original ACD disposition."<sup>23</sup> Thus, ACD is similar to the "judgment withheld" procedure that Chiricos et al. studied in their analysis of Florida cases: both dispositions make it possible for defendants to be found guilty without acquiring records of convictions. The authors include in the original table only those variables whose net regression effects were statistically significant at the level of .10 or better. Race and time employed were significantly related to sentence severity; they are presented here to underline their lack of effects on dismissal and ACD. Age, another "extra-legal" variable, failed to show any relation with any category of the dependent variable and is not included in the original table. A general conclusion from these results is that "extra-legal" factors do not account for variations in either dismissal or ACD outcomes. But it is difficult to summarize the results any more precisely than this. Only one "legal" variable -- number of arrest charges--has negative effects on both decisions.

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# Table AA-6

p ,	Dismissals b	В	p	ACDs b	B
p	b	В	p	b	В
	·				
.05	.035(.018)	.047			
			.13	.141(.072)	.155
.01	.036(.021)	.044			
			06	089(.030)	.107
08	022(.010)	042	08	085(.046)	.065
			14	006(.004)	069
	.05 .01 08	.05 .035(.018) .01 .036(.021) 08022(.010)	.05 .035(.018) .047 .01 .036(.021) .044 08022(.010)042	.05 .035(.018) .047 .13 .01 .036(.021) .044 06 08022(.010)04208 14	.05 $.035(.018)$ $.047.13$ $.141(.072).01$ $.036(.021)$ $.04406$ $089(.030)08$ $022(.010)$ $042$ $08$ $085(.046)14$ $006(.004)$

Standard errors in parentheses.

\*Four categories: unemployed 6+ months; unemployed less than 6 months; employed less than 6 months; employed 6+ months.

\*\*Weighted index of prior convictions: prior felony convictions were given 3 points, prior misdemeanor convictions 2 points and prior violations 1 point. The index is the sum of the scores for each defendant.

Source: Bernstein, Kelly and Doyle, 1977, p. 750, Table 2.

The remaining "legal" variables affect either one decision or the other. Thus, defendants charged with burglary and assault had better chances of receiving dismissals. Defendants charged with resisting arrest or drug offenses and defendants with heavier records of past convictions had poorer chances of receiving ACD dispositions. The investigators attempt to explain these inconsistent and seemingly patternless results by invoking four different lines of interpretation. The positive relationship between burglary charges and dismissal outcomes is seen as reflecting "evidentiary concerns." The fact that we find defendants charged with burglary more likely to have their cases dismissed may be a function of burglary cases being difficult to prosecute successfully. Many burglaries are committed at times and in places where ( ); eyewitnesses are not present. The absence of witness naturally reduces the strength of the evidence. To explain why defendants charged with assault have better chances of receiving dismissals, the authors introduce a version of the "sociological hypothesis" according to which officials place "lesser value on interpersonal violence when it occurs among minority groups." While we lack individual data on victims, our court observations revealed that almost all of the assault cases prosecuted were assaults between persons of the lower classes who predominate in the catchment area served by this court...we suggest that interpersonal violence evokes a lesser response when both the defendant and the victim are socially disadvantaged because there is less concern for disadvantaged victims. According to the authors, the finding that resisting arrest charges reduced the probability of ACD dispositions can be

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understood in relation to "the organizational imperative to maintain good relationships among criminal justice personnel. To support this interpretation, the authors cite works by other researchers which demonstrate "the importance of the police to the criminal justice system and the need to sanction those who counter police authority."<sup>27</sup> The authors do not offer any explanation for the finding that defendants charged with drug offenses have better chances of receiving ACD dispositions, even though the relative size of this relationship is greater than the resisting arrest-ACD relationship.

Finally, the relationship between prior record and ACD disposition is also seen as supporting the "sociological hypothesis." The authors propose that this finding "suggests differentiation on the basis of accumulated disadvantagea status."

That is, those with heavy prior records, having previously been adjudicated guilty, have already accrued a disadvantaged label. Whether differentiation on the basis of this prior disadvantaged status is discriminatory depends on whether the status of "prior convicted offender" was ascribed or achieved. To the extent that one's conviction for a prior crime was not entirely a function of the alleged offense, the negative effect of a prigg record can be interpreted as partly discriminatory.

While these explanations reveal considerable ingenuity, they can be neither proved nor disproved with data from the study. They amount to post hoc speculations advanced to account for findings manifestly at odds with the "sociological hypothesis"--or, as the authors term it, the "societal reaction" hypothesis. As they observe in concluding remarks, the results of the study indicate that "the role of the deviants' social attri-

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butes in explaining variation in societal reactions seem very much overstated" by proponents of the hypothesis.<sup>29</sup>

Rather than abandoning the argument, Bernstein et al. recommend "considerable shifting" of its "theoretical focus":

Specifically, we interpret our findings to suggest that greater attention be paid to (1) organizational imperatives of the deviance-controlling agency, (2) the expectations and values of those participating in the decisions and (3) the role of accumulated disadvantaged statuses acquired in prior deviance processing stages.

What is interesting about this proposal is that two of the three elements retain emphasis on "extra-legal" attributes as causal factors in decision-making. One reason why the "expectations and values" of decision-makers might be expected to matter is that defendants vary with respect to "extra-legal" attributes. Another is that cases against defendants vary with respect to the seriousness of offenses charged.<sup>31</sup> Indeed, the single example given--official attitudes toward assaultive crimes by minority group members -- seems to combine both of these reasons. The authors may have had other considerations in mind, but it is unclear exactly how far we move beyond the "sociological hypothesis" by focusing on officials' "expectations and values." That question can also be asked about the third element in the proposed reformulation: "accumulated disadvantaged statuses." The authors give one illustration for this concept: the finding (noted above) that defendants with relatively heavy prior records were less likely to receive ACD dispositions. As their strongly-qualified statement indicates, however, the extent to which the acquisition of prior records depends on

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"ascribed" (i.e.; "extra-legal") characteristics of defendants is an unresolved issue.<sup>32</sup> An additional guestion which the authors ignore is why prior record does not affect the probability of dismissal.

It therefore appears that the reformulation offered may refer only to "organizational imperatives" of official agencies. The authors do not specify the theoretical meaning of this concept. Instead, as with the other two elements, the problem of definition is solved with a single example, i.e., the need for maintaining "good relationships among criminal justice personnel"<sup>33</sup> that the authors see reflected in the lower likelihood of ACD dispositions for defendants charged with resisting arrest.

The conclusions advanced by Bernstein et al. are models of ambiguity and equivocation. Their argument is the same as that advanced by Burke and Turk: "extra-legal" factors may not have direct effects on decision-making, but the possibility of indirect effects cannot be ruled out. Therefore, the "sociological hypothesis" still appears valid, and researchers should continue to search for empirical support.

Unfortunately, the authors draw this implication from results that may themselves be invalid. Their "sample" consisted of:

all males arraigned in a city in New York State, from December, 1974 to March, 1975, whose most severe arrest charge was a felony charge, whose cases were not disposed of at first court presentation and whose cases were finally disposed of in criminal court within the four-month period of coservation, by a judgment other than an acquittal, by j dges who disposed of more than one percent of the cases (N=1,213).34

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The proportion of cases excluded, and hence the possibility of substantively biased findings, may have been sizeable. This is indicated in a footnote reference to defendants disposed

at first court appearance.

Seventeen percent of persons arrested for felonies are finally disposed of at their first court presentation. i.e., within 24 hours after the arrest. Since the disposition process is so truncated, we analyzed data for this group separately. Our findings indicate that the factors that affect...disposition decisions for these defendants are quite different for those not so rapidly disposed. But the authors do not consider whether these and other exclusions

may have affected the form and composition of the regression model they develop to explain their findings.

There are other reasons to question their results. The authors make no reference to defendants with other cases pending against them at the time of arrest. The presence of such defendants in the sample would bias the results of the analysis since the probabilities of both dismissal and ACD dispositions were almost surely affected by pending cases.<sup>36</sup> A second weakness is the investigators' failure to control for evidential strength. They maintain that the effects of burglary charges and number of charges on dismissals (see Table AA-6) are "interpretable as reflective of evidentiary concerns," but the logic of this

claim is unclear. In fact, the authors do not include a single item of evidence among their independent variables. Third, the authors report results for only five felony offenses (burglary, robbery, drugs, larceny or theft, and assault), which probably means that defendants charged with other offenses were dropped from the regression analysis. Thus, their effort to compare

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the relative importance of "extra-legal" and "legal" factors as determinants of decision making falls short of even minimal adequacy.

# C. Methodological Adequacy and Significant Results

The final study offers the most reliable findings yet available concerning the validity of the "sociological hypothesis" as an explanation of criminal court decision-making. Myers and Hagan<sup>37</sup> used regression analysis to study decisions to prosecute in a systematic random sample of defendants (N=980) charged with felony offenses in Marion County (Indianapolis), Indiana, whose cases were disposed between January 1974 and June 1976. Cases involving "victimless" crimes (gambling, prostitution, and drugs) were excluded from the sample. The authors also examined decisions to try defendants rather than accept guilty pleas, but these findings do not bear on nonconviction dispositions and are disregarded in the present discussion.

In addition to standard "extra-legal" defendant characteristics, the investigators included a number of variables relating to evidential matters and victim attributes. The twenty-four independent variables fall into five groups. Names of the variables and coding categories are as shown in Table AA-7.

Two considerations are important in interpreting the findings shown in Table AA-8. First, the "case" unit in this study is not an arrested defendant but a defendant against whom an information or indictment has been filed.<sup>38</sup> The sample contains only cases which were neither (a) rejected by prosecutors nor (b) prosecuted as misdemeanors. This limits the utility of

#### Notation Variable

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Evidentiary Strength  $X_1$  Eyewitness Identification(s)

X<sub>2</sub> Confession of Defendant and/or Accomplices

X<sub>3</sub> Weapon Recovered

X<sub>4</sub> Stolen Property Recovered

X5 Amount of Expert Testimony

X<sub>6</sub> Amount of Nonexpert Testimony

X7 Number of Witnesses

Victim Credibility and Culpability X<sub>B</sub> Victim Sex

X<sub>9</sub> Victim Age

X<sub>10</sub> Victim Employment Status

X<sub>11</sub> Victim-Defendant Relationship

X<sub>12</sub> Victim Prosecution Preference

X<sub>13</sub> Prior Victim-Defendent conflict<sup>a</sup>

X14 Alleged Victim Misconduct

X<sub>15</sub> Victim Prior Record

Defendant Credibility and Dangerousness X<sub>16</sub> Defendant Sex

<sup>a</sup>The measure for prior victim-defendant conflict is the weighted sum of responses (No = 0;  $Y_{CS}$  = 1) to the following: victim struck the first blow (3); victim argued with the defendant immediately prior to the offense (2); prior arguments with the defendant (1); prior harrassment or abuse by victim (1). The alpha reliability coefficient (Cronbach, 1951) of the resulting measure was .67. <sup>b</sup>Race composition is orthogonally coded as two vectors. In the first, black defendant-white victim events are assigned the value of 1; the remainder are assigned 0. In the second, white defendant-white victim events are assigned the value of 1; the remainder, 0. <sup>c</sup>Prosecution charge refers to the rank of the most serious charge filed by the presecutor; seriousness is "assed on the mean statutory penalty.

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## Table AA-7

## Variables Used by Myers and Hagan

Scale	Notation Variable	Scale
Interval	X <sub>17</sub> Defendant Age	Interval
Interval	X <sub>18</sub> Defendant Employment Status	0 Unemployed 1 Employed 2 Self-employed
0 No 1 Yes 0 No 1 Yes	X <sub>19</sub> Defendant Prior Record	0 None 1 Arrest, no conviction 2 Conviction, no incar- ceration
Interval	X20 Pretrial Belease Status	3 Incarceration(s)
Interval		1 Out on bond 2 Never arrested
Interval	X <sub>21</sub> Counsel	0 No counsel 1 Court-appointed 2 Privately retained
0 Female	X <sub>22</sub> Amount of Bond	Interval
1 Male Interval	Xacial Composition X <sub>23</sub> Racial Composition <sup>b</sup>	White defendant-white victim
1 Unemployed 2 Employed 3 Self-employed		Black defendant-white victim Black defendant-black victim
1 Family 2 Friend or acquaintance 3 Stranger	Legal Seriousness X <sub>24</sub> Prosecution Charge <sup>c</sup>	Interval
0 Unwilling to prosecute 1 Willing to prosecute		
Interval		
0 None 1 Non-criminal and non- sexual 2 Sexual 3 Potentially criminal		
0 None 1 Arrest, no conviction 2 Conviction, no incar- ceration 3 Incarregration(s)		
0 Female 1 Male	•	

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Table AA-8

The Decision to Prosecute Fully (Y1). Standardized Regression Coefficients and

	Variable		r	Metric Coefficient (Standard Error)	Standaroized Coefficient
×i	Eyewitness Identification		.101	.105 (.023)	.228
X2	Confession of Defendant and/or Accomplice(s)		.037	.067 (.030)	.032
X4	Recovery of Stolen Property		.073	.149 ( 036)	.161
X <sub>6</sub>	Amount of Nonexpert Testimony		.026	.038 (.023)	.108
x <sub>11</sub>	Victim-Defendant Relationship		022	063 (.023)	185
X <sub>12</sub>	Victim Prosecution Preference		.184	.325 (.059)	.321
x <sub>13</sub>	Prior Victim-Defendant Conflict		.046	.009 (.002)	.098
×17	Defendant Age		082	005 (.002)	090
X <sub>21</sub>	Counsei		093	~.083 (.032)	038
X <sub>23</sub>	Racial Composition a. Black defendant-white victim vs. black intra-racial		.069	.158 (.041)	.092
	b. White intraracial vs. black intraracial		010	.147 (.040)	.149
×24	Prosecution Charge		.132	.022 (.004)	.418
		R <sup>2</sup> N	.120 418		

<sup>a</sup>The metric and standardized coefficients are taken from the weighted least squares solution, while the zeroorder correlations and R<sup>2</sup> are taken from the ordinary least squares solution.

Source: Myers and Hagan, 1979, p. 447, Table 2.

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the results for evaluating the "sociological hypothesis" as an explanation of conviction-nonconviction dispositions. The authors do not indicate how the two prior selection decisions may have affected relationships in the sample of cases they studied. Second, the dependent variable -- "the decision to prosecute fully"--is a measure not of conviction but of prosecution. All cases not prosecuted were dismissed, but some prosecuted cases resulted in acquittals. This further limits the utility of the results for the question at hand.

With these notes of caution, let us consider the findings presented in Table AA-8. Regression coefficients for all independent variables shown are significant at probability levels of .05 or better. Correlation coefficients for only three of the variables--"eyewitness identification" (X1), "victim prosecution preference"  $(X_{12})$ , and "prosecution charge"  $(X_{24})$ --are significant at the .05 level. Each of these is a "legal" factor. Standardized regression coefficients indicate that the same three variables have considerably greater explanatory power than any others. By this test of substantive significance, the results strongly support the "legalistic" hypothesis that Hagan originally proposed as an alternative to the "sociological hypothesis."

The results also indicate that certain "extra-legal" variables make small contributions to the probability of "full" prosecution. Older defendants  $(X_{17})$ , defendants represented by court-appointed attorneys ( $X_{21}$ ), the defendants accused of offenses against white victims  $(X_{23})$  have somewhat greater chances of being prosecuted. Several other "extra-legal" defendant characteristics--sex  $(X_{16})$ , employment status  $(X_{18})$ , and prior record  $((X_{19})$ --do

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not affect the prosecution decision. Furthermore, the decision is unaffected by nearly all "extra-legal" victim characteristics--sex  $(X_{g})$ , age  $(X_{q})$ , employment status  $(X_{10})$  and prior record  $(X_{15})$ . Therefore, the results do not confirm the "sociological hypothesis." The influence of several "extra-legal" factors is modest and appears only after controlling for the effects of "legal" factors.

Myers and Hagan sum up their findings as follows:

Taken together, the findings suggest that evidence of the defendant's guilt, a credible victim (i.e., white, willing to prosecute), a potentially discreditable defendant (young, represented by court-appointed counsel), and a legally serious offense enhance the probability of full prosecution. Of these elements, the legal seriousness of the offense (i.e., prosecution charge) and the case's evidentiary strength are given substantially greater weight than other characteristics.

They also observe that their findings concerning the effect of "racial composition" has particular significance for evaluating research of this kind.

To discover the point at which effects become significant, we reanalyzed the data, adding each set of independent variables to a regression equation containing racial composition. The coefficients for racial composition increased in magnitude and reached statistical significance only after measures of evidence were controlled. Evidence, then, suppresses the effects of race. This feature of our results is crucial for it calls into question studies that find no effect for race but fail to control for evidentiary strength. An accurate estimate of the effects of race and other 'extra-legal' variables could very well hinge, then, on a consideration of the evidence.

Further research is needed to determine whether "extra-legal" factors actually have the kind of modest, indirect effects on criminal court decision-making to which Myers and Hagan refer. Unfortunately, the results of this study are not conclusive. It has other weaknesses in addition to the two limitations

mentioned above. Instead of measuring type of offense directly in dummy-variable analysis, the investigators measured seriousness of offense by taking the mean statutory penalty of the most serious charge filed. Also, the authors overlooked the matter of "pending" cases which has been shown to affect decisions in previous research.<sup>41</sup> Until more results are available. the most appropriate conclusion is that the "sociological hypothesis" remains unproven as an explanation of criminal court decisionmaking.

# D. Conclusions

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that "legal" factors are. stage of the total dispositional process, ignoring decisions

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The preceding review shows that empirical support for the view that "extra-legal" factors affect the probability of conviction is practically non-existent. Early studies seem to confirm the "sociological hypothesis" but they suffer from serious methodological deficiencies. When more adequate data, research designs, and analytical procedures are employed, the results of research indicate that "extra-legal" factors are not significantly related to the likelihood of conviction but

A number of problems are apparent in quantitative research on the determinants of criminal court decision-making. First, investigators typically concentrate on decisions at only one

at other stages. It is difficult to compare results from studies examining decision-making at different stages of the process. For example, Bernstein et al. studied cases not disposed at

the first court appearance, while Myers and Hagan looked at felony filings. Second, rarely do any two studies use the same independent variables or define them in the same way. Bernstein <u>et al</u>. measured the defendant's employment status according to length of time unemployed or employed, while Myers and Hagan measured it according to a three-fold classification (unemployed, employed, self-employed). Third, even when they are aware of the importance of controlling for offense categories, some investigators (e.g., Myers and Hagan) fail to employ adequate measures for type of offense.

It is possible that even the most sophisticated quantitative research may not bring greater accuracy in assessing the role of "extra-legal" factors in criminal court decision-making than we have already achieved. One reason is that defendants in criminal cases are often very similar with respect to certain theoretically important attributes such as income, education, and occupation. The absence of variation in these potentially significant causal factors is likely to result in findings indicating that they are not important determinants of case dispositions.<sup>42</sup>

Another inherent difficulty in quantitative research on criminal court decision-making is the assumption that "extra-legal" factors operate in a simple bi-variate fashion. Most researchers automatically assume that nonwhite defendants have greater chances of conviction than white defendants, relatively uneducated defendants greater chances than relatively educated defendants, in relation to other variables.

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poor defendants greater chances than wealthy defendants, and so forth. But this assumption is questionable. Results from the study by Myers and Hagan show that the defendant's race matters only in connection with the victim's race. The finding of an interaction effect involving the race of the defendant and the race of the victim points up the fallecy of attributing bi-variate effects to variables that may be significant only in relation to other variables.

The difficulty with the assumption of bi-variate effects is nicely expressed by Hindelang. His observation addresses bail decisions by individual judges, but it has general applicability to criminal court decision-making:

In setting bail and determining outcome, the court has simultaneous access to information. That is, information comes to the judge as a whole cloth and not in bits and pieces that eventually are stitched together. It would be surprising if the court considered bits of information in a case rather than viewing the case wholistically...

In other words, the significance of such "extra-legal" factors as race, income, and occupation probably depends on the total context in which these factors appear, rather than on any intrinsic importance they may have by themselves.

Another problem facing those who wish to do quantitative research on the determinants of criminal court decision-making concerns the distinction between "extra-legal" and "legal" factors. Few investigators have given this problem the attention it deserves. There is no difficulty in viewing race, socio-economic status (income, education, occupation), age, and sex as "extra-legal" characteristics. It is questionable, however,

whether prior record, pretrial release status, and legal representation (whether by private or court-appointed attorney) can be treated as "extra-legal" in the same sense. Victim participation in criminal court decision-making adds further complexity to the issue. For example, the victim's willingness to testify as a prosecution witness can be seen as a "legal" factor, but the victim's "disreputability" (as might be inferred by a prosecutor, for example, from a prior record or the circumstances of the offense) cannot be viewed as a matter of strictly "legal" relevance.

It seems clear that researchers need to transcend the simplistic "legal" vs. "extra-legal" dichotomy that has been employed in the past. A more adequate classification would recognize at least three different kinds of "legal" variables. One such category includes factors bearing on evidential strength: victim willingness to prosecute, number and kinds of witnesses, physical evidence, confessions, and testimony. Another category includes offense-related variables: amount and kind of harm done and relationship between defendant and victim. A third category includes case-processing variables: pretrial release and detention status, type of legal representation, police and prosecutorial screening decisions, pleabargaining practices, and sentencing decisions. But whether it is possible to design research permitting examination of all these factors remains unclear. In addition, the question of how to classify the variable of prior record is still unanswered.

Perhaps the greatest challenge to quantitative research on criminal court decision-making lies in the limited explanatory power achieved through previous investigations. Despite major improvements in methodology, researchers are unable to explain more than a small part of the total variation in outcomes. Unexplained variation may reflect the inadequacies of existing methodological approaches used to analyze the "dense causal web"<sup>44</sup> of criminal court decision-making. To a much greater extent, however, it may reflect the uncertainty, unpredictability, and indeterminacy of decision-making in criminal justice.<sup>45</sup> In unraveling the complexities suggested by this possibility, researchers may find it necessary to re-think the entire strategy of quantitative research.

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# Appendix AA--Notes

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# Appendix BB

CASE PROCESSING AND ATTRITION: THE EFFECTS OF PRETRIAL DETENTION AND DELAY ON CRIMINAL COURT DECISION-MAKING

In his classic 1927 study of <u>The Bail System in Chicago</u>, Arthur L. Beeley identified an issue that may be essential for understanding criminal court decision-making:

The problem of bail is obviously closely related to the matter of continuances. There would, in fact, be little or no administrative problem of the sort discussed here if the bulk of defendants were disposed upon first arraignment.

This observation suggests that pretrial release and detention policies <u>interact</u> with continuance policies--or, more generally, policies for managing court delay--as determinants of conviction and attrition rates. The following chapter argues that Beeley's original insight can help us reconcile contradictory empirical evidence concerning the effects of pretrial detention and court delay on dispositions of criminal cases.

Beeley recognized that the rate of attrition is closely related to the extent of pretrial detention. This can be seen in his discussion of "unnecessary arrests."<sup>2</sup> Quoting official statistics on case outcomes, he observed that the <u>majority</u> of all persons arrested and brought to court were eventually discharged without being convicted at an average rate of 60.5 percent for the period 1910-1921. His data also showed that summonses or police citations in lieu of arrests were used extremely sparingly to bring defendants to court (less than one percent of all criminal cases arraigned in the Chicago Municipal Court in 1925). Beeley concluded that too many persons, especially those



accused of minor crimes, were unnecessarily taken into custody, resulting in serious injustice. "If too poor to furnish security... [the defendant] is locked up pending arraignment, irrespective of the comparative insignificance of the charge and the fact that he is probably innocent -- at least that his chances of being discharged are as three to two."<sup>3</sup>

Students of criminal justice have shown more interest in pretrial detention and courtroom delay as possible determinants of attrition rates than in other aspects of case processing such as screening, pleabargaining, trial procedures, and sentencing. The argument that defendants held in pretrial custody have significantly greater chances of being convicted than defendants released before trial is currently one of the most often-repeated generalizations among scholars and researchers. The proposition that "excessive" delay leads to "unnecessary loss" of convictions has perhaps equal acceptance. Both hypotheses, however, appear to involve serious oversimplifications of decision-making in criminal courts.

The first part of the following discussion re-examines a number of empirical studies which seemed to show that pretrial detention makes a significant independent contribution to the attrition rate. This relationship is now part of the conventional wisdom in criminal justice research. Pretrial detention imposes numerous liabilities on defendants, as the Attorney General's Committee on Poverty and the Administration of Federal Criminal Justice<sup>4</sup> observed almost two decades ago. Defendants who cannot

obtain pretrial liberty are hampered from participating in the preparation of an adequate defense (gathering evidence, locating witnesses, and consulting freely with lawyers). These difficulties, compounded by punitive and degrading conditions typically associated with pretrial detention, may induce detained defendants to convict themselves by pleading guilty to charges they would contest if released before trial. As plausible as this argument may be, the empirical evidence in its favor has come from a number of widely-known studies that were badly flawed in design, analysis, or both. More recent studies examined in the second part of the chapter indicate that pretrial detention by itself is unrelated to the likelihood of conviction. Third, the chapter considers an alternative explanation which emphasizes the connection between detention and delay in explaining rates of nonconviction dispositions in criminal courts. Finally, the chapter discusses the implications of current research on case processing for the development of an empirically-based theory of decisions in criminal justice. A. The Effects of Pretrial Detention on Conviction and Attrition Perhaps surprisingly, the possibility that conviction might be disproportionately frequent among defendants detained before trial was not one of the "administrative problems" Beeley identified in his analysis of bail policy in Chicago. Rather, Beeley was concerned with what he saw as gross misapplication by court officials of four different alternatives (bail bonds, recognizance without sureties, cash deposits, and recognizance with sureties) then available for securing defendants' appearance in court.

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The administrative problem here is one of making discriminating use of these various forms of bail process. The interests of justice are frustrated when these alternative forms of conditional release are misapplied.

Beeley's was the first in a series of "bail reform" studies aimed at demonstrating that many more defendants "who are ordinarily committed to jail pending trial, might, without administrative difficulty, be conditionally released in the community."<sup>6</sup> Beeley drew a sample of cases from among unsentenced jail prisoners, dividing it into two groups--"dependables" and "undependables"--on the basis of classifications by an <u>ad hoc</u> "staff committee" of two social workers familiar with each of the cases and a psychiatrist unacquainted with the cases. These rather curious procedures, plus Beeley's reformist interest in demonstrating the desirability and feasibility of liberalizing pretrial release policy, kept him from investigating the possible impact of detention on case dispositions.

The Handicap of Jail Status. The possibility that detention increases the likelihood of conviction was first set forth as an explicit hypothesis in two studies of bail administration in Philadelphia and New York City directed by Caleb Foote.<sup>7</sup> These studies were also the first to <u>question</u> the assumed relationship between detention and conviction by suggesting that court officials' judgments as to guilt, evidential strength and convictability in individual cases might be responsible for both detention and conviction differentials. Foote and his collaborators knew the relationship could be spurious. "For example, if the magistrates are sufficiently sagacious

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to impose high bail predominantly upon those who are in fact guilty and are lenient with those who are in fact not guilty, then to that extent the higher proportion among jail defendants of those adjudged guilty will not reflect any handicap resulting from jail status."<sup>8</sup> But that suggestion remained an unexamined hunch for nearly two decades of research on the effects of pretrial detention, during which most investigators continued looking for and (predictably) finding evidence of the "handicap of jail status."

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The two studies by Foote and his associates are frequently cited in favor of the hypothesis that pretrial detention leads to a higher probability of conviction, but the support they provide is extremely weak. The New York City study (based on "a sample of 3,223 actions...composed of approximately one-half of the felony prosecutions for 1956 in each of the counties of New York, Bronx and Queens"<sup>9</sup>) reported only one result of interest: a 14.1 percent difference in <u>overall rates of grand</u> jury dismissals between bailed (23.9 percent) and jailed (9.8 percent) defendants. The results of the Philadelphia study were more detailed, but if anything they actually should have left the hypothesis in weaker shape (see Table BB-1). In four of the offense categories examined (burglary, auto theft, sex crimes, and parcetice offense).

auto theft, sex crimes, and narcotics offenses), the attrition rate for detained defendants <u>equaled or exceeded</u> the rate for released defendants. Yet the investigators did not search more deeply

# Table BB-1

## Percentages of Indicted Defendants Not Convicted

	Defe	endants on B	ail	Defendants in Jail			
Crime	Convicted	Not Convicted	Percent Not Convicted	Convicted	Not Convicted	Percent Not Convicted	
Violent crimes	50	101	67	86	29	25	
Burglary	19	3	14	52	14	21	
Assault and battery	62	79	56	27	7	21	
Auto theft	24	. 8	25	33	12	27	
Property crimes	47	46	49	75	6	7	
Sex crimes	44	1.6	27	11	4	27	
Narcotics offenses	29		3	_56	5	8	
All above offenses	275	254	48	340	77	18	
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into this seeming anomaly, their only comment being a gloss on the findings: "Although there are marked variations among different types of crimes, [Table BB-1] illustrates that a defendant who is out on bail is much more likely not to be convicted than is a defendant who comes to court from jail."<sup>10</sup> To their credit, Foote and his co-workers had an inkling that the relationship between pretrial detention and court delay might somehow matter for explaining their results. In the Philadelphia study, they noted that the average time from preliminary hearing to adjudication was one month for jail cases and nine months for bail cases. To interpret this difference, they called attention to the much higher rate of "nol prosses" (prosecutorial rejections) among bail case indictments (30.2 percent) than among jail case indictments (2.4 percent). "Some nol prosses," they proposed, "may simply mean that the failure to provide In other words, they argued that "deterioration" of evidence stemming from delay helped bailed defendants avoid conviction. The investigators were silent as to whether delay also improved the chances of jailed defendants, although their implicit if not explicit message emphasized the disadvantages of delay

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a speedy trial has resulted in the loss of valuable evidence."<sup>11</sup> for defendants avaiting trial in custody. This interpretation was modified slightly in the New York City study. There the same pattern of shorter delays in jail cases and longer delays in bail cases turned up, but with a difference, as shown in Table BB-2.

# Table BB-2

Time From Complaint To Dismissal By Grand Jury

Days	Prison <u>Number</u>	Cases Percent	Bail C Number	ases Percent	Prison-Ba Number	ail Cases Percent
0-49	138	85.2	92	64.8	32	46.4
50-99	17	10.5	33	23.2	20	29.0
100-149	7	4.3	13	9.2	6	8.7
150-199			4	2.8	7	10.1
200 and over					4	5.8
Totals	162	100	142	100	69	100

Source: Foote <u>et al.</u>, 1958, p. 728.

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These results showed "prison" defendants receiving dismissals sooner than "bail" or "prison-bail" defendants. The investigators speculated that prosecuting attorneys may have been giving "some preference to prison cases" in calendaring defendants' cases for presentation to the grand jury.<sup>12</sup> Yet they concluded that the policies prosecutors were using to handle jail cases amounted to a mixed blessing, since 14.8 percent of the jailed defendants spent between 50 and 149 days in jail before dismissal. "Probably this was because prosecutors occasionally hold back weak cases hoping to secure sufficient evidence to obtain an indictment."13 Although these results did not suggest to the investigators that delay might actually have been advantageous for those jailed defendants whose cases were dismissed in less than 50 days, they did complicate the original assumption that delay has uniformly negative effects on the likelihood of nonconviction dispositions for jailed defendants.

Bail Reform. Next to the Foote studies, the most widely known research on the relationship between pretrial detention and conviction was done during the early 1960's in New York City under the auspices of the Vera Foundation, later to become Vera Institute of Justice. The authors of the first Vera study proclaimed unconditionally that "a person not in jail at the time of adjudication stands a better chance of receiving a favorable disposition of his case."<sup>14</sup> To substantiate this claim they introduced the results shown in Table BB-3.

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# Case Dispositions by Jail Status and Charge

	At Liberty			De		
Charge	Percent Convicted	Percent Not Con- victed	Total Cases	Percent Convicted	Percent Not Con- victed	Total Cases
Assault	23	77	126	59	41	128
Grand larceny	43	57	96	72	28	156
Robbery	51	49	35	58	42	100
Dangerous weapons	43	57	23	57	43	21
Narcotics	52	48	33	38	62	42
Sex crimes	10	90	49	14	86	28
Others	30	70	47	78	22	23

Source: Ares, Rankin and Sturz, 1963, p. 84, Table 10.

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Another defect concerned the treatment of cases involving defendants charged in separate criminal actions. The authors explained that each such case was "merged" and treated as one case in the analysis.<sup>16</sup> This introduced another source of possible bias into the data, for it meant that cases in which defendants were convicted in only one of several actions were counted as convictions, regardless of dispositions in other actions.

The confidence to be placed in these figures as support for the argument depends upon how one assesses the consequences of certain limitations of the study. First, the authors made no attempt to control for evidential strength or prior record, to mention only two factors that could account for some of the differences shown. Second, they ignored the fact that nonconviction differentials between bail and jail cases were very small in certain offenses (-7 percent for defendants charged with robbery and -4 percent for defendants charged with sex crimes) and positive (+14 percent) for defendants charged with narcotics offenses. Third, the data represented about sixty percent of the total cases handled in the court studied because three classes of

cases were excluded: (a) those with defendants under twenty-one; (b) those which originated in the grand jury rather than in

the lower court; and (c) those which were not disposed at the time of data collection.<sup>15</sup> The decision to exclude two-fifths of the total court workload from the study may have had significant consequences for measured rates of pretrial detention and conviction

To the extent that defendants with actions pending against them were both more likely to be jailed before trial and more likely to be convicted in at least one case, the results do not confirm the argument that pretrial detention itself reduced the prospects of favorable outcomes at adjudication.<sup>17</sup>

The second Vera study<sup>18</sup> reached a more tentative conclusion about the relationship between detention and conviction. This study revealed a twenty percent difference between bailed and jailed defendants in likelihood of conviction, as shown in Table BB-4.

Rankin disclaimed any interest in pursuing the relationship between detention and conviction for two reasons. First, the results indicated that that relationship was less strong than the relationship between detention and sentencing severity--the central issue in the study. Second, she felt that "further research is needed before an effect of detention on conviction can confidently be stated to exist." <sup>19</sup> Nevertheless, she went on to display percentages of nonconvictions in all but one of the following tables "so that the reader may make comparisons if he wishes."<sup>20</sup> In those tables, Rankin introduced separate statistical controls for five different case- and defendant-related variables (prior record, bail amount, private or court-assigned counsel, "family integration," and "employment stability"). The results revealed persistent conviction-rate differentials between jailed and bailed defendants, reinforcing the impression that pretrial detention itself harmed defendants' chances at adjudication.

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# Table BB-4

ween	Detention	and	Unfavorable	Disposition
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	Bail <sup>a</sup> (%)	Jail <sup>b</sup> (%)
son <sup>C</sup>	17	64
: prison <sup>d</sup>	36	9
	47	27
ints	(374)	(358)

<sup>a</sup>Free some or all the time pending disposition.

<sup>b</sup>In jail all the time from arrest until disposition.

<sup>C</sup>Sentenced to one of the city, county, or state correctional institutions.

<sup>d</sup>Includes suspended sentence or choice of fine or prison.

e Includes acquittal, dismissal, discharge on own recognizance.

Source: Rankin, 1964, p. 642, Table 1.
But there are two other and perhaps more compelling reasons for questioning Rankin's findings on detention and conviction. First, an enormous number of cases were excluded from the analysis. Consider the following groups of cases excluded because they were ineligible for the fledgling Manhattan Bail Project:

defendants with a narcotics charge since 1955; defendants who admitted use of drugs; defendants with present or previous charges of forcible rape, homicide, sex offenses involving a minor, or a present charge of assault on a police officer; and defendants who were certain that they would have their own private attorney in court at the initial arraignment.<sup>21</sup>

Additional exclusions were made on other grounds:

Defendants released on their own recognizance pending trial were not included in this study because release on recognizance in itself may have an effect on disposition in addition to the effect of freedom pending trial. Defendants whose cases were adjudicated the same day they were arraigned were not included because there was no question of detention pending dispositions. Defendants for whom bail was never set were not included because they had no opportunity to post bail.<sup>22</sup>

Finally, one group of cases may or may not have been included

in the study:

Defendants with a certain combination of present charge and previous convictions [not further specified] were not bailable in...[the lower court], but were bailable in the higher court if a bail petition was made by the defendant's lawyer. Usually the petition was not made.

Second, Rankin presented data permitting analysis of conviction-rate differentials in relation to court delay as measured by the number of days from arraignment to disposition. Comparison of outcomes between jailed and "part time free" defendants revealed that the relationship between detention and conviction was <u>reversed</u> in cases subject to "long" delays of 30 days and over. Rankin introduced these data in order to correct for a "simplification" made in Table BB-4, which was to combine together "all bail defendants regardless of whether they made bail immediately or not."<sup>24</sup> Table BB-5 shows that rates of nonconviction were higher for defendants who posted bail after spending some time in detention than for jailed defendants <u>only</u> among cases disposed within the first 29 days after arraignment. Among cases disposed 30 or more days after arraignment, however, jailed defendants appear to have enjoyed some advantage over "part time free" defendants with respect to nonconviction dispositions. That finding might have prompted deeper inquiry into the relationship between detention, delay and conviction. Instead Rankin turned away from the issue to concentrate on her main interest, the relationship between detention decisions and sentencing outcomes.

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Unconstitutional Detention. In 1972 a study of the "handicap-of-jail-status" hypothesis was commissioned by the Legal Aid Society of New York City to be included as a brief in its suit challenging the administration of bail.<sup>25</sup> The suit argued that bail practices in that city violated the equal protection and due process clauses of the United States Constitution. It claimed that the results of the study proved beyond doubt that "the present bail system creates two classes of accused distinguished by their wealth or lack of it: those who are released and are relatively more likely to have a favorable outcome in their cases, and those who are detained in lieu

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### Table BB-5

# Relationship Between Detention and Unfavorable Disposition When Number of Days in Jail is Held Constant

Days in Jail

		Less Than		10 to 29 Days		30 Days And Over	
Disposition	None	Part- time Free (%)	Never Free (%)	Part- time Free (%)	Never Free (%)	Part- time Free (%)	Never Free (%)
Sentenced to prison	11	20	60	26	59	56	73
Convicted without prison Not convicted	38 51	34 46	10 30	39 35	9 32	38 6	9 18
Number of defendants <sup>a</sup>	(190)	(143)	(114)	(23)	(138)	(16)	(100)

<sup>a</sup>Information about number of days in jail was not available for 8 defendants; they are omitted from this table.

Source: Rankin, 1964, p. 643.

the likelihood of conviction.

The study has shown that one factor--whether the accused is released or detained pending trial--above all others determines both the outcome of his case and the likelihood of his receiving a prison sentence. By examining seriousness of charge, type of crime, weight of evidence, aggravated circumstances, prior criminal record, strength of family ties, employment status, and the amount of bail, the study demonstrates that neither independently nor in combination do any of these factors account for the disparity in outcome and in severity of sentence between those detained and those released. The inescapable conclusion is that the fact of detention itself causes those detained to be convicted far more often and, sentenced much more severely than those who are released.

But a different and much more balanced interpretation

of the results was prepared at the invitation of the editors of the Criminal Law Bulletin, where the study was published. In his methodological commentary, Hindelang<sup>28</sup> raised three objections to the data and analytic techniques used in the research. First, Hindelang noted that the cases actually studied--a select group comprising approximately 20 percent of the Society's files of closed cases on adult male defendants

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of bail and are therefore much more likely to be convicted and get a prison sentence."<sup>26</sup> Although the suit was unsuccessful, it served as an occasion to publicize yet another set of research results ostensibly demonstrating that pretrial detention increases

What were the findings of this study and why were its sponsors so confident they had irrefutable evidence that detention itself was the primary cause of conviction-rate differentials between the "ins" and the "outs," as the study referred to detained and released defendants? Kasanof and Single summarized their results and conclusions as follows:

accused of felonies -- might not be representative of all cases involving persons named as defendants in the action. But while couched in lawyerly terms, the criticism may have been more serious than Hindelang admitted, for the "sample" was even less representative of all criminal cases processed in Manhattan. Cases of female defendants, defendants with retained or assigned counsel other than Society lawyers, and defendants without counsel-were automatically eliminated from the study along with some 80 percent of the Legal Aid Society's cases.

Hindelang's second criticism went to the validity of certain measures used in the study. In particular, he questioned the measurement of evidential strength (whether or not the defendant gave a confession after arrest and whether or not evidence was found on the defendant) on both substantive and methodological gounds, arguing that (a) admissibility is the key issue with respect to both confessions and physical evidence and (b) dichotomization of the evidence variable overlooks the importance of "gradations in the damning nature of the evidence found on the accused."<sup>29</sup> He also objected to the use of the misdemeanor-felony distinction as a control variable for what the study referred to as "type of crime." More refined analysis by offense category would be necessary, Hindelang cautioned, before accepting this study's conclusion that "the type of crime in no way begins to offer an explanation for the different treatment"<sup>30</sup> of detained and released defendants at adjudication.

His third criticism concerned the analytic procedures used in the study. Hindelang called this "the question of simul-

Two or more control variables may have no effects individually but may show very strong effects jointly. Examining their effects independently is not sufficient. In setting bail and determining outcome, the court has simultaneous access to information. That is, information comes to the judge as a whole cloth and not in bits and pieces that eventually are stitched together. It would be surprising if the court considered bits of information in a case separately rather than viewing the case wholistically; in attempting to understand the decision-making process of the court, therefore, it would seem appropriate, to the extent possible, for the researchers to use tabular analyses to examine the effects of variables simultaneously.<sup>32</sup>

Because the study did not introduce simultaneous controls, the conclusions its authors drew about the irrelevance of their control variables for explaining the probability of conviction were in doubt.

Despite Hindelang's insightful understanding of the way court officials use information in making decisions, he neglected one variable that may be a key item of information in case processing. That variable is the amount of <u>delay</u> from initial appearance to disposition in individual cases. Had he thought about this matter, Hindelang might have come to an even more cautious view of the results than the one he submitted to the Criminal Law Bulletin. A Partisan Argument? Although resting on one of the flimsiest

foundations ever constructed in empirical research, the "handicap-of-jail-status" argument has held attraction for students of American criminal justice since the early works of Foote and the Vera Institute. For example, the President's Commission on Law Enforcement and the Administration of Justice referred

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# taneous control of two or more variables."<sup>31</sup>

to the Vera studies as providing evidence that detention heightens the probability of conviction.<sup>33</sup> In later research on the effects of pretrial detention, investigators have used more sophisticated techniques of regression and path analysis, thus satisfying one of the criteria ("simultaneous controls") Hindelang specified in his critique of the Legal Aid Society study. Problems of inadequate research design have continued to plague these efforts, however, leading to results hardly more acceptable than before.

A recent study by Swigert and Farrell<sup>34</sup> illustrates the difficulties inherent in research using sophisticated techniques to study the effects of detention but employing conventional assumptions about how those effects are produced. Although this study does not focus on pretrial detention, the results show how treating detention as a dichotomous variable and using no "simultaneous control" for court delay can produce results indicating that detention increases the probability of conviction. Swigert and Farrell analyzed case-processing decisions in a 50 percent random sample of all homicide cases decided over a nineteenyear period (1955-1973) in a large northeastern city (N=444). The authors included a measure of bail (coded as either "yes" or "no") among the variables they examined to explain differential treatment of the defendants, citing works by Foote and Ares, Rankin and Sturz as justification for predicting that pretrial detention might affect outcomes. Their results show that pretrial detention has an independent negative effect on the probability of avoiding conviction.

This finding is highly suspect. A serious problem of which the authors seem unaware is that <u>murder</u> is not a bailable offense. The authors do not tell us how many defendants in their sample obtained release on bail. They also overlook the fact that defendants accused of manslaughter ("negligent homicide") cannot be convicted of murder ("non-negligent homicide"). Instead, they begin their analysis by treating all cases as "homicide" cases without distinguishing between murder and manslaughter charges. They then use the murder-manslaughter distinction to measure their dependent variable, which they call "final disposition." (This would seem to vitiate entirely the authors' conclusions about the impact of defendants' social characteristics on decision-making in murder cases.) Finally, they neglect the variable of delay, which is typically quite high in murder cases because of special demands for time to conduct additional investigation, locate witnesses, and prepare prosecution and defense arguments. It is hardly surprising, then, that the Swigert-Farrell finding concerning the relationship between detention and ultimate disposition is almost completely at odds with an observational study of bail and defense preparation in murder cases. After observing and interviewing both public defenders and private attorneys in Chicago, Gilboy concluded: For a number of reasons an all out effort by an attorney to have bail set may be potentially harmful to the best

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to have bail set may be potentially harmful to the best possible defense at trial. Therefore, defense lawyers in capital cases will often decide not to seek bail at all (or they may choose to seek bail through methods that are likely to be less successful).

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The vitality of the hypothesis that detention increases the chances of conviction may be due to the fact. that it is a defense lawyer's argument. Pretrial detention, however necessary it may appear to police and prosecutors, imposes punishment on persons supposedly entitled to the presumption of innocence. That "jail status" handicaps not only the defendant but also the defense attorney is clear from an early discussion concerning the advantages of release:

A defendant free on bail or on his own recognizance can make good use of this liberty. He is available on a twenty-four hour basis to consult and participate fully with counsel in time-consuming preparations for trial. He alone may be able to locate and persuade defense witnesses to testify. He is often the key source of factual details on which to base pretrial motions and negotiations. He can assist in tracking down evidentiary leads.

The hypothesis originated with lawyers (Foote and Ares) and enjoyed its most lavish treatment from lawyers (the Legal Aid Society). As the authors of the Legal Aid Society study wrote:

This study was undertaken... in the hope of proving by hard data something which has been known by veteran criminal lawyers for a long time: The court's decision at arraignment to detain or release the accused is a crucial factor affecting the outcome of a case.

Now let us see how this hypothesis was undermined by non-lawyer researchers using more advanced techniques of analysis but expecting to find that pretrial detention itself is a determinant of the attrition rate.

B. The "Non-Effects" of Pretrial Detention on Attrition

The results of three recent studies<sup>38</sup> throw serious doubt on the claim that the probability of conviction is automatically increased for jailed defendants. The first two studies indicate

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that detention "sometimes" affects the likelihood of conviction; the third concludes that detention "never" affects the conviction rate. These results surprised the investigators. But because they failed to examine closely the factor of delay as a possible determinant of differential outcomes between jailed and/or released defendants, none could account satisfactorily for the apparent "non-effects" of detention.

Eisenstein and Jacob's Felony Justice<sup>39</sup> is one of the most ambitious cross-jurisdictional analyses of case processing ever done by social scientists. The study contains rich qualitative information on the functioning of "courtroom workgroups" and the influence of "workgroup organization" on felony dispositions in Baltimore, Chicago and Detroit. It also presents quantitative results which challenge many standard assumptions about criminal courts. One of these is the finding that "jailed defendants are not consistently convicted more often than those who win release before trial."<sup>40</sup> Careful inspection of methodological procedures used, however, makes this claim evaporate.

It is important at the outset to note the qualification--"consistently"--the authors attached to their conclusion. Their findings were difficult to summarize because under certain circumstances detention appeared to increase the likelihood of attrition. As they wrote: "Bail status sometimes affected case outcome."<sup>41</sup> The relationship, however, was not as strong or as consistent as they had expected. This result seems to have caught them by surprise, leading them into an interpretive

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One of their responses to this difficulty was to engage

in post hoc speculation about unanticipated findings.

For instance, in Chicago we found that for armed robbers and burglars, making bail was more important in determining their fate at the preliminary hearing than the strength of the evidence. However, the effect of bail was quite different for the two offenders. Bailed armed robbers were more likely to have their case dismissed or disposed of at the preliminary hearing than those who were jailed. Burglars released on bail were more likely to be indicted than their jailed counterparts. The difference may lie with the backgrounds of the armed robber and burglar. Although there may be little difference in the professionalism of the jailed and bailed armed robber, it is likely that the bailed burglar was a professional who had the resources to make bail, whereas his amateur counterpart was too poor to raise bond money and thus went to jail until disposition. But the professional burglar faced the more serious charges, which could not be diposed of at the preliminary hearing.

A second response was to ignore certain unexpected results. For example, one finding indicates that pretrial detention increased the probability that the defendant's case would be sent to the upper court for trial in Baltimore but not in Chicago or Detroit. 43 The only comments Eisenstein and Jacob make about this cross-city difference were to characterize the effect of pretrial detention as "almost invisible" and to emphasize that the "identity of the courtroom is the most important variable" in each of the cities.<sup>44</sup>

Third, Eisenstein and Jacob resorted to circular explanations for certain results. Their comments on Table BB-6 are illustrative.

The authors explain these results in the following way:

Canonical correlation squared

Percentage of explained variance accounted for

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Release

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### Table BB-6

## Chicago Courtroom Analysis of Decision to Dismiss Charges

	Southside	Northside	Narcotics <u>No. l</u>	Narcotics No. 2
1	.05	.12	.30	.42
ed r by:				
	79.0	43.2	18.1	54.6
· · ·	14.3	25.4	64.5	6.0
	0	27.2	3.8	29.3
	1.3	4.0	13.5	0

Source: Eisenstein and Jacob, 1977, p. 207, Table 8.4.

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One effect of courtroom workgroups is that they perceive defendants and cases differently and treat them differently. Table [BB-6] shows those differences for four Chicago preliminary hearing courtrooms which heard enough cases to permit this kind of analysis. The different norms of each courtroom emerge in striking fashion. In the north side courtroom, the offense charged was most important; defendant's race was a distant second. However, decisional patterns are not well captured by these variables for that courtroom, both as measured by the correlation coefficient and by the measure of improvement in our predictive ability. In the south side courtroom, offense charged was less important, and race and bail status were much more important: all the variables again explain little of the variance. In one narcotics courtroom, offense charged had little importance, but the race of the defendant has a large effect: whites were sent to trial more frequently than blacks. In the other narcotics courtroom, race had no significance, but the workgroup reacted to the offense charged and bail status. The only feature common to these four courtrooms was that weight of the evidence as we measured it did not explain much of the variance in the 45 decision to dismiss charges at the preliminary hearing.

A fourth response involved misinterpreting certain findings concerning the effect of pretrial detention on adjudication. Their data appear to indicate, for example, that jail status (here defined as "characteristics of defendant") accounts for 55 percent of the explained variation in the conviction rate in Baltimore, 12 percent in Chicago, and seven percent in Detroit.<sup>46</sup> Eisenstein and Jacob recognize the parallel between this finding and the one reported in Table BB-6 but later conclude that "[c]ontrary to popular rhetoric and some research, a defendant's...bail status...did not determine outcomes."47 This interpretation may badly distort the significance of the results discussed above, however, which (as we are told in a footnote<sup>48</sup>) were produced by excluding dismissals from analysis.

Another set of results indicating that pretrial detention "sometimes" affects case dispositions is reported in Bernstein,

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all males arraigned in a city in New York State, from December, 1974 to March, 1975, whose most severe arrest charge was a felony charge, whose cases were not disposed of at first court presentation and whose cases were finally disposed of in criminal court within the four-month period of observation, by a judgment other than an acquittal. by judges who disposed of more than one percent of the cases (N=1,213).

A number of questions arise immediately concerning the decision to exclude cases disposed at the first court date (17 percent of all persons arrested for felonies).<sup>51</sup> Are such cases and dispositions unimportant for understanding the "societal reaction to deviants." as the authors refer to criminal court decision-making? What consequences did this exclusion have on measured rates of nonconviction dispositions? How did it affect the conclusions they reach? The authors tell us only (in a footnote) that separate analyses of the excluded cases indicated that "the factors that affect the...dispositional decisions for these defendants are guite different from those affecting the same decisions for those not so rapidly disposed.<sup>52</sup>" A second possibly serious problem concerns the decision to exclude cases not disposed during the period of the study. Without knowing the number and kinds of cases that survived more than four months and the outcomes they received, it is impossible to state confidently that differences in attrition between jailed and released defendants found in the study represent real case-processing differences in the court studied.

The results of the Bernstein et al. study both contradict and confirm the "handicap-of-jail-status" hypothesis. They

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BB-26

# Kelly and Doyle.<sup>49</sup> The "sample" for this study consisted of:

show that the probability of "straight dismissal" <u>increased</u> if the defendant had been detained before trial. Defendants released before trial, however, had better chances of receiving another nonconviction disposition known as "adjournment in contemplation of dismissal."<sup>53</sup>

To account for these inconsistent results, the authors follow two contradictory lines of interpretation. With respect to the negative impact of detention on the probability of ACD, they merely re-phrase the "handicap-of-jail-status" hypothesis with references to "societal reaction" and "social typing."<sup>54</sup> But they offer a very different explanation for the finding that detention increases the likelihood of dismissal:

Our observations suggest that some court agents are using court processes as sanctions. That is, they assume that defendants who have been detained already have been sanctioned. To save the court further expenditures of time and money, the detention experience is treated as having provided the necessary "taste of jail" to deter future crime...The subsequent dismissal...obscures the fact that the defendant has been punished unnecessarily.

This interpretation views pretrial detention as a form of legally unauthorized but informally recognized punishment.

As we shall see below, the "informal punishment" hypothesis marks an emerging line of research on criminal court decision-making. For now, let us observe that Bernstein <u>et al</u>. did not connect their findings with the matter of delay. This is all the more remarkable given their criticism of previous research on the effects of detention for "failing to differentiate between those detained for longer versus shorter periods of time."<sup>56</sup> Unfortunately, the way in which they included delay in their measure of pretrial results almost uninterpretab release status" variable tre values--detained more than o than 30 days, release on bai nizance--that should have be in dummy-variable analysis.) Summarizing the results we may say that they appear in serious doubt without dis for Goldkamp<sup>57</sup> to address di that Foote had raised twenty appear to indicate unequivoc is wrong as it applies to con jailed and released defendam

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In the foregoing analysis, an attempt was made to explore the possible relationship between custody and adjudication by employing a trifurcated analytic model to help specify its applicability. When all defendants were considered in terms of dismissal or nondismissal of all charges (on Step One), the rate of dismissals appeared to be unaffected by pretrial custody status. When nondismissed defendants were examined on a diverted/nondiverted dimension (Step Two), a moderately strong bivariate relationship with pretrial custody was found. But when six control variables [charge seriousness, detainer/warrants, number of prior arrests, on conditional (probation, parole, furlough or work) release, open (or "pending") cases, and number of different offenses charged] were entered first in multiple regression, pretrial custody was stripped entirely of its ability to affect or explain variance in diversion decision outcomes. It was concluded from this that the relationship was spurious, explained by the common correlation of both custody and diversion to such variables as charge seriousness and prior arrests. Finally, a weak relationship of little consequence was found between pretrial custody and findings of guilt or innocence for defendants who had not been dismissed or diverted. It was concluded that pretrial custody had no noticeable effect on a defendant's prospective innocence or quilt.

BB-28

in their measure of pretrial detention makes their regression results almost uninterpretable. (Specifically, their "pretrial

release status" variable treats as ordinal categories four values--detained more than or equal to 30 days, detained less than 30 days, release on bail, and release on personal recognizance--that should have been handled as nominal categories in dummy-variable analysis.)

Summarizing the results of the two studies already discussed, we may say that they appear to place the "handicap" hypothesis in serious doubt without disconfirming it entirely. It remained for Goldkamp<sup>57</sup> to address directly the issue of spuriousness that Foote had raised twenty-five years earlier. His findings appear to indicate unequivocally that the "handicap" hypothesis is wrong as it applies to conviction-rate differences between jailed and released defendants:

Goldkamp's analysis is methodologically superior to all previous quantitative research on the effects of pretrial detention on prosecution and adjudication decisions. Other findings he reports<sup>59</sup> indicate that the "handicap" hypothesis may be correct as it applies to sentencing. Thus, among convicted defendants "the custody relationship re-surfaces in powerful form in the sentencing decision component where incarceration or nonincarceration is decided."<sup>60</sup> Obviously persuaded by his findings, Goldkamp concludes his study by urging further research designed to answer the following question: "Why might a prejudicial effect from pretrial detention be experienced at sentencing but not in the adjudication of defendants' cases?"<sup>b1</sup>

But is this the right question? Goldkamp's model includes no measure of delay. The next section suggests that failure to examine the joint effects of detention and delay is likely to produce invalid results concerning the effect of detention on the conviction rate.

### C. Detention, Delay and Attrition

Many observers think that delay increases the rate of attrition. According to an old adage familiar to detectives and prosecuting attorneys, cases get "worse" as they get "older." The belief that criminal complaints "deteriorate" with age is usually explained by alleged tendencies of witnesses to "disappear," complainants to "cool off" and "wear out," and evidence to "dry up" as cases "drag on." Students of criminal courts also believe that delay lowers the likelihood of conviction by increasing pressure on court officials to use screening

procedures and dismissals as expedient methods of "moving"

calendars. The President's Commission, for example, linked rising rates of attrition to delays occasioned by increasingly

heavy caseloads faced by urban criminal courts:

As the backlog of cases mounts, delay increases and the pressure to dispose of cases becomes overwhelming. Clearing the dockets comes to be an end in and of itself, and haste rather than intelligent deliberation is the norm of practice. Disposition by dismissal or by quilty plea is often characterized by hasty decisions with little attention given to penal and correctional consideration.

Empirical support for the hypothesis that delay affects the attrition rate is slim and at best inconclusive. The following discussion considers four studies dealing with this relationship. The results of the first three studies are completely contradictory: Banfield and Anderson<sup>63</sup> concluded that delay increases the attrition rate, Landes<sup>64</sup> that delay reduces it, and Levin<sup>65</sup> that delay has no effect on attrition. Defects of research design and analysis present in each study make it difficult to accept these findings as either confirming or disconfirming the hypothesis. The results of the fourth study by Nardulli<sup>66</sup> help to understand not only the contradictory nature of earlier findings but also the complex relationship between delay and attrition. As we shall see, the last study provides the first empirical evidence that delay increases the attrition rate through a joint or interaction effect with pretrial detention. The major assumption of the Banfield-Anderson study of continuances in Chicago is that costs are associated with court

delay. Excessive delay, they argued, wastes precious resources of time and money, creates judicial inefficiency, and imposes needless burdens on witnesses and attorneys. Delay also weakens prosecution and causes unnecessary "losses" of convictions. However, Banfield and Anderson recognized a distinction overlooked in most conventional discussions of the relationship between delay and attrition:

The use of the terminology "lost convictions" as a cost is in a sense misleading. In actuality, the costs of any disproportionate differences in conviction rates over time may be better expressed as "unjustifiable convictions."

In other words, the effects of delay are likely to be two-fold: (a) a lowered probability of conviction for some defendants and (b) an increased probability of conviction for other defendants.

This recognition enabled Banfield and Anderson to perceive in shadowy and uncertain terms a relationship that escaped subsequent investigators until Nardulli showed that delay and detention affect attrition jointly. Unfortunately, Banfield and Anderson were unable to demonstrate that what Nardulli later identified as one component of court delay--namely, confinement delay--may be a key factor in understanding the impact of delay on the attrition rate. This inability stemmed from certain weaknesses in their research discussed below.

The principal Banfield-Anderson results bearing on the matter at hand are presented in Table BB-7. The data reveal sharp increases in attrition with increasing delay (see "all cases" column in Table BB-7). They also indicate that the probability

Number of Court Appearances

9 or more Total

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BB-32

### Table BB-7

### Delay, Detention and Percent Not Convicted

Bailed Defendants		Jai Defen	Jailed Defendants		l-Jail Nation	All Cases		
(N)	(%)	(N)	(%)	(N)	(%)	(N)	(%)	
73	14	67	7	57	2	197	. 8	
63	29	54	17	26	27	143	24	
71	31	23	27	16	19	110	37	
207	24	144	14	99	21	450	20	

Source: Banfield and Anderson, 1968, p. 300 (Table 3) and p. 310 (Table 25).

of nonconviction dispositions increased over time among both bailed and jailed defendants, although the rise appears to have been somewhat less steep for jailed defendants. The latter finding led Banfield and Anderson to see that delay may be more advantageous for some defendants than for others. Although Banfield and Anderson seem not to have realized it. this insight points to a possible three-way relationship between delay. detention and attrition.

To understand their reasoning, it is necessary to consider how they thought the relationship between delay and attrition could be explained. Their explanation contained three elements. The first proposition was that delay itself is not the sole or even the most important determinant of attrition. Thus, they observed that attrition-rate differences were also associated with bail-jail status, type of counsel (retained vs. assigned), and race of defendant (black vs. white). Second, they argued the implausibility of explaining the relationship between delay and attrition by proving that "the short cases simply involve obviously guilty defendants while the lengthy cases involve 'hard' problems most likely to lead to a non-guilty disposition."68 Were it available they reasoned, such a "proof" would indicate that "case difficulty" is the cause of both delay and attrition. thereby rendering the original relationship spurious.<sup>69</sup>

But this seemed unlikely to Banfield and Anderson in view of what they saw as the "most startling statistics of this study...[which] indicate that the conviction rate for white defendants who have retained counsel declines at a faster pace

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had to be at work:

It is difficult to contend that lengthy non-white retained attorney cases involve less difficult problems than do lengthy white retained attorney cases. One's imagination must be expanded considerably to argue that 'borderline' arrests and prosecutions are more likely to occur among the white population than among the non-white.

These considerations left a third possibility for explaining the relationship: delay as "a necessary but not a sufficient condition"<sup>72</sup> for attrition. As already noted, Banfield and Anderson did not discern clearly the interaction between delay and detention as a possible determinant of the attrition rate--the explanation that Nardulli would later advance. Instead, they invoked the nebulous concept of "official attention" to explain their results:

The statistics do not tell us whether the conviction cost should be expressed in terms of loss of convictions of white defendants with retained counsel, or in terms of unjustifiable gain in conviction rates of other defendants. However, it was noted previously...that lawyers, and probably other judicial officials, seem to pay less attention to non-white than to white defendants. Making a further inference, it may be possible that to say either that continuances produce lost convictions when the case is "taken seriously" by the operators of the judicial system or that unnecessary convictions occur over time when the case is "taken

In the end, Banfield and Anderson explained the delay-attrition relationship as being largely a product of discriminatory treatment of low-income black defendants. Disproportionately subject to pretrial detention and representation by assigned public defender attorneys, these defendants bore the brunt of having their cases "taken lightly" and thus faced disproportionately high probabilities of conviction in "lengthy" cases.

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BB-34

than does the conviction rate for non-white defendants who have retained counsel."<sup>70</sup> Something other than "case difficulty"

The Banfield-Anderson study is flawed by serious problems of design and analysis. First, the investigators employed no control for type of offense, evidential strength, or prior record in their analyses of delay-attrition differentials. Second, they eliminated defendants with pending cases from their sample. Third, they drew all of their principal findings from analyses of cases decided in the upper court (Criminal Division) following grand jury indictment. Only six of the forty tables presented refer to cases disposed in the lower court (Municipal Department). These six tables give reason to believe that the relationship between delay and attrition is quite different for cases that survived the grand jury than for cases not presented to the grand jury. None of the six tables introduces the bail-jail distinction, thus precluding any comparison of lower-court cases and upper-court cases with respect to a potentially important determinant of attrition. Finally, the authors relied entirely on two-, three-, and four-variable contingency tables to analyze their data, making it impossible to detect possible interaction effects. (This could account both for the tentative quality of the explanation finally proposed and for the authors' failure to recognize the possibility that delay and detention interact in their effects on attrition.)

Despite these problems, the Banfield-Anderson study provides at least limited support for the claim that delay increases the rate of attrition. It also suggests that the effects of delay are roughly equal for detained and released defendants.

The next study by Landes<sup>74</sup> comes to exactly opposite conclusions about the relationship between delay and attrition. The level of statistical sophistication in this study is far above that of the Banfield-Anderson study. Landes' analysis includes many more variables and yields many more results than the previous investigation. Regrettably, methodological problems render his findings virtually worthless for explaining variations in the rate of attrition. The study is nevertheless instructive. Its shortcomings can help us see more clearly how delay may work in concert with detention as joint determinants of the attrition rate.

Landes reached two conclusions of interest. First, he argued that his results indicate a strong negative relationship between pretrial detention and attrition. This finding was consistent with his theoretical model of criminal court decision-making, which predicted that

if the defendant were not released on bail, the costs of his resource inputs would rise, leading to a reduction in these inputs and an increase in the probability of conviction. Therefore, a decline in the fraction of defendants making bail should result, in an increase in the fraction of defendants convicted.

Obviously, this argument re-states the traditional "handicap-of-His second conclusion was that court delay reduces the

jail-status" hypothesis in economic terms. rate of attrition. This finding, too, fit Landes' theoretical

model:

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One reason for the positive association between...delay...and convictions may be that the prosecutor becomes more selective with respect to the cases he prosecutes as trial delay

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increases. That is, he selects from an inventory of cases the ones he believes to have the greatest probability of conviction and the highest sentences if convicted in order to maximize his weighted conviction function.

Landes skillfully elaborates his argument with other findings concerning the effects of defendants' resources and prosecutors' calculations on case dispositions. But his findings seem to contradict the Banfield-Anderson conclusions. Although their implications are not entirely clear, they suggest that delay increases the probability of convictions for all defendants and that the conviction-inducing effects of delay are considerably lower for defendants not constrained by the "handicap of jail status."

How can we account for Landes' surprising results? The answer is that they appear almost entirely to be artifacts of the data he had available and the measures he constructed. Unlike Banfield and Anderson, who examined case-level data from one jurisdiction, Landes obtained two sets of aggregate data for his research:

The first source is an American Bar Foundation (ABF) study, in which over 11,000 felony defendants in 1962 were sampled from state court dockets in nearly 200 counties. From this sample we can estimate for several counties within most states the number of defendants released on bail and their average bail charge, the number going to trial, and the number dismissed, acquitted and sentenced. The second major source of data is for the 89 U.S. district courts where annually published statistics on...criminal cases are available. These data contain information...[on] criminal court queues, the number of cases going to trial, the disposition of cases, and the number of criminal defendants receiving subsidized legal services.

In other words, the units of observation are courts, not cases. Landes analyzed relationships between rates of detention, delay

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and conviction across court jurisdictions rather than among cases within those jurisdictions.

Orsagh's point is not that researchers must necessarily collect

their own data, but rather that the use of aggregate data is likely to produce biased results. This is because pre-collected aggregate data often do not provide direct operational equivalents for particular theoretical constructs of interest to the investigator. The result may entail important methodological compromises which take the form of so-called "proxy" variables, i.e. measures with uncertain linkage to the variables in question.

Landes employed two highly questionable "proxy" variables. To measure the conviction rate, he took the proportion of defendants receiving prison sentences in the two samples. Landes' justification for this decision is not clear. The most reasonable (and charitable) explanation is that the measure reflects his interest in the prosecutor's "weighted conviction function." 79 Clearly, however, this is not a good measure of the conviction rate, for it is unlikely that the rate of prison sentences bears a constant relationship to the rate of convictions across the jurisdictions studied. A less acceptable possibility is that this measure produced larger regression effects than the

Aggregate data have come to be employed with increasing frequency in criminal justice research. Orsagh suggests that one fact above all others may explain this trend:

Aggregate data are cheap--there are no survey instruments to design or test in the field, and no full-fledged, expensive gathering of data is required.

combined effects of more direct measures based on the proportions of defendants dismissed and acquitted.<sup>80</sup> Another and perhaps more disturbing possibility is that Landes regarded convictions not resulting in prison sentences as decisions having no particular significance for either the criminal justice system or defendants. Some evidence that this may have been true is found in his suggestion that dispositions resulting in suspended prison sentences or probation "should probably be viewed as nonconvictions."<sup>81</sup>

Even more questionable is the "proxy" Landes developed to measure delay in his analysis of the ABF data. Because no direct measure of delay was available in that data set, he used county population as an indirect measure, explaining that "longer trial queues are generally thought to exist in large urban areas."<sup>82</sup> That choice may have been extremely consequential for his findings, since he drew all of his conclusions about the effects of pretrial detention on attrition from the results of his analyses of the ABF data. Although admitting that "the uncertain relation between queues and population size and the lack of strong statistical significance of the population variable"<sup>83</sup> called for caution in interpreting the results, Landes later replicated his finding that delay increases the probability of conviction using the federal court data which provided more adequate measures of delay--i.e. actual elapsed time to disposition. In his concluding remarks, however, he reiterated the claim that delay leads to an increased conviction rate, making only cursory mention of the measurement problem discussed earlier.<sup>84</sup>

The results of Landes' study stand on precarious footing. His finding concerning pretrial detention and attrition reproduces an aggregate-level relationship subsequently shown invalid in several studies using case-level data.<sup>85</sup> His other finding concerning the delay-conviction relationship seems invalid on its face: how could delay possibly increase the likelihood of conviction? Landes claims that prosecutors "select" older cases according to convictability and sentencing severity. But does this mean that prosecutors are less "selective" with newer cases? If the explanation is correct, the proportion of cases with high probabilities of conviction and/or severe sentence would somehow have to increase with delay. Few scholars would be prepared to accept this suggestion. 86 Landes seems to argue that conviction rates increase among both jailed and bailed defendants with time, albeit perhaps somewhat less rapidly among bailed defendants. Given the inconsistent findings of previous research on delay, it seems almost fitting for Levin to have found results which failed to provide "any direct evidence that long delay in fact weakens the prosecutor's case."<sup>87</sup> Levin's study appears to solve the contradiction between Landes and Banfield-Anderson by declaring both wrong. But his research "design" relies on crude comparisons of delay measures and conviction rates in five criminal courts. Using his masterful knowledge of the details of case processing in each court, Levin identifies a number of factors other than delay--"especially the judges' basic attitudes"<sup>88</sup>--that may explain differences in conviction-rate

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differentials among the five. His analysis does not show that delay and attrition are unrelated. No analysis could prove such an assertion with aggregate data from only five courts--except perhaps for those five courts and as an aggregate-level relationship. Levin's generalizations are unjustifiably broad.

A curious feature of Levin's study is its equation of nonconviction dispositions with "leniency."

Whatever its source, the courts' leniency must be placed in proper prospective. The criminal court is not the major source of leniency in the criminal justice system. The largest proportion of persons "fall out" of the system--gain their freedom--at the level of the police and the prosecutor. Nonetheless there is a significant degree of leniency in the conviction and sentencing decisions of courts; some are very lenient, including the Pittsburgh and Chicago preliminary hearing courts; but the court's leniency does not seem to be associated with the amount of delay.

This attitude may be related to an apparent preference for administrative changes designed to increase the conviction

rate:

An important potential function of the criminal court judge (but one that few seem to take seriously today) is to encourage the police and prosecution to behave effectively and decently. They should gather more effective evidence, prepare their cases better, and be less willing to reduce and dismiss charges in cases that objectively warrant prosecution, as well as maintain due process and protect individual rights. Judges who face large caseloads are more likely to tolerate prosecutors' dismissals and carelessness with defendants' rights.

Levin appears to view court delay as more an advantage than a disadvantage for the criminal justice system. His findings persuade him that delay does not worsen the chances of conviction. He even argues that delay may reduce delay: "A possible benefit of a large or moderate amount of delay is that it discourages the parties for taking actions that would further add to delay

(going to trial or requesting additional continuances) and it stiffens the judges' resistance to such actions."91 Finally, he suggests that delay in criminal cases may confer "flexibility and conciliation" on case processing:

Many judges consider many felony cases less serious than their formal charge, especially when there is a "special" relationship between the defendant and victim. With the passage of time, victims in these cases may wish to drop charges. The Chicago preliminary hearing court judges use continuances to "allow the victim to 'cool off' and the perpetrator to make restitution." They also use them "to give defendants [especially vouthful offenders] a small dose of incarceration [while avoiding] a conviction record"; they continue the case for a week or two, set high bail, and then at the end of this period release the defendant without a conviction.92

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ten years earlier.

Nardulli's findings are the most credible yet reported on the relationship between detention, delay and attrition. Employing case-level data on felony proceedings from the same jurisdiction Banfield and Anderson studied, he discovers indications of both substantial attrition and fairly lengthy delay.

Over 70 percent of defendants in Chicago preliminary hearing courts received dismissals, findings of no probable cause, or acquittals following misdemeanor bench trials.<sup>93</sup> On the average, cases ultimately dismissed lasted more than three months (mean=142 days, median=106 days) and between four and five court appearances before being disposed.<sup>94</sup> These results broadly parallel the situation portrayed by Banfield and Anderson

As did Banfield and Anderson, Nardulli observes that the relationship between delay and attrition cannot be understood apart from the guestion of pretrial detention:

While only 19 percent of all defendants who had their cases dismissed were confined for the entire dispositional period (average confinement for these defendants = 76 days, median = 57 days), only 18 percent escaped with no time in detention. Forty percent of all dismissal defendants spent only one day in jail, 11 percent spent between two and seven days in jail, and the gemaining 16 percent spent between eight days and a year.

For analytic purposes, Nardulli transforms these differences into an interval-level measure which he calls the "months of confinement variable." The findings (see Tables BB-8 and BB-9) indicate that what we might term confinement-delay increases the likelihood of dismissal.

To interpret these results, it is important to recognize what the table does and does not show. First, the confinement-delay effect (X<sub>o</sub>) shown here for drug cases does not appear in a separate analysis of dismissals in general felony cases.96 The effect may therefore be related to several distinctive features of case processing in Chicago's drug courts: more cooperative defense attorneys, comparatively minor offenses, no pressures from angry civilian complainants, and especially the standard practice of reducing charges to misdemeanors and offering sentences of probation or time already served to confined defendants who plead guilty.<sup>97</sup> Nardulli suggests that case-processing differences which encourage speedy pleabargained dispositions expose confined defendants not pleading quilty to relatively serious "informal sanctioning" through pretrial detention. Officials have fewer rewards to offer defendants for pleading quilty in the general felony courts. Whether confined or not, defendants in non-drug cases may be more inclined to

Variable X, X,

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interaction term was entered.

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# Table BB-8

Regression Analysis for Cases Dismissed\* in the Chicago Drug Courts

Approximate Percentage of the Variance	Beta** Weight	F ** Value
Accounted for		121 (7.2)
	.21 (12)	0 (5.3)
2.0	01 (10)	10.2
1.0	- 24	10.2
3.0	_ 09	4.0
2.0	00	55.5
14.0	52	0
14.0	.00	4.0
1.5	14	33.0
1.5	44	23.3
1.5	.35	
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\*Dismissed cases include dismissals, findings of no probable cause,

\*\*Figures in parentheses in Table BB-8 are the relevant data before the

Y = Dismissal variable (0 = not dismissed, 1 = dismissed)

- $X_1 = Case seriousness$
- $X_2 =$ Strength of state's case  $X_3$  = Prior arrest record (0 = no prior arrests,

1 = prior arrests)

 $X_4$  = Presence of other pending complaints (0 = no other complaints, 1 = other complaints)

 $X_5$  = Presence of a defense counsel (0 = none present,

 $X_{b}$  = Defense counsel responsiveness scale (continuous version)

 $X_7 =$ Confined (0 = released at time of disposition,

1 =confined at time of disposition)  $X_8$  = Months spent in confinement for confined defendants

only.

Source: Nardulli, 1978, pp. 170-171, Table 6-5.

### Table BB-9

Predicted Dismissal Rates	ted Dismissal Rates Across Different Confinement Periods							
	(Not Confin	ed)	Months of Confinem			ent		
	0	1	2	3	4	5		
Predicted probability of dismissal	.49	.02	.19	.37	.54	.71		

Source: Nardulli, 1978, p. 172, Table 6-6.

court appearances.

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resist pressures to plead guilty. The net effect of confinement delay on the attrition rate thus may be even greater in preliminary hearing courts handling general felony cases.

Second, Table BB-8 indicates that pretrial detention has a significant negative effect  $(X_7)$  on the probability of attrition. This appears to contradict results of other studies which show that pretrial detention does not consistently increase the probability of conviction.<sup>99</sup> Nardulli does not examine the effect of pretrial detention on dismissals in general felony cases, as other researchers have done, but in drug cases only. This finding, too, may be explained by the dynamics of pleabargaining in Chicago drug cases. That is, the effect of pretrial detention shown in Table BB-8 may reflect the pecularities of case processing in drug courts which, as discussed above, give confined defendants strong incentives to plead guilty at early court appearances.

Third, the data indicate that confinement delay is a more important determinant of dismissal than any other variable except pretrial detention. Nardulli appears possibly to have underestimated the magnitude of the confinement-delay effect, since his measure includes months of confinement for "confined defendants only," <u>i.e</u>. for defendants confined at time of disposition, but <u>excludes</u> months spent in confinement by defendants released <u>before disposition</u>. The total effect of confinement delay on the probability of dismissal may therefore exceed that of detention alone (the latter effect, as indicated above, probably being specific to the context of drug court operations).

On balance, then, the results in Table BB-8 provide reasonably strong support for the argument that detention and delay interact in their effects on attrition. These results are limited to cases disposed in Chicago's preliminary hearing drug courts, but Nardulli's interpretation of dispositional practices in those courts<sup>100</sup> suggests that the same relationship may also obtain in general felony preliminary hearing courts. If so, the findings reported in Table BB-9 have particular importance for understanding the impact of detention and delay on attrition. They show that each month of confinement delay yields a steady and substantial increase in the probability of dismissal among cases disposed in the drug courts. To the extent that this pattern is also present among general felony cases (a question for which Nardulli's research unfortunately provides no answer), it brings us a long way toward resolving a number of contradictions in empirical research on the relationship between detention, delay and attrition.

### D. Conclusions

This chapter has recounted the history of an idea originally formulated by defense lawyers, subsequently adopted by social scientists first as an hypothesis and then as an established empirical generalization, and finally invalidated by other social scientists. Blumberg expresses the once-accepted outlook, arguing that:

the discomforts occasioned...[by pretrial detention] are employed as a weapon against the accused by the prosecutor and judge. A recalcitrant accused can be socialized Ŧ

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relatively quickly by an extended sojourn in the remand jail, including setting bail at a level high enough so that he cannot meet it. The common refrain heard in the remand jail, from those who have been there for an extended period, is a desire to plead quickly and get sentenced 101 so that they can be moved to a more commodious prison.

detention imposes constant costs on all defendants subjected to it. In statistical language, the "handicap-of-jail-status" hypothesis is that pretrial detention has a direct linear effect on the probability of conviction. The chapter has also suggested that the "confinement-delay" hypothesis may represent a modest step forward in the development of an empirically-based theory of criminal court decision-making. This hypothesis, which raises the possibility that detention and delay have an interactive effect on dispositions. does not seem to have been recognized when it first appeared. To date, there is only limited evidence for the confinement-delay hypothesis. Nardulli's findings have yet to be replicated in any other jurisdiction or for any other kind of offense. Indeed, it is possible that under certain circumstances confinement delay may increase the probability of conviction. A number of questions therefore await investigation in the future. The first concern is the generality of the confinement-delay effect. Does such an effect appear consistently in all jurisdictions and for all offe, ses? A second and closely related question concerns the direction of the effect. Is it always negatively associated with the probability of conviction? Third,

the causal significance of confinement-delay as a dispositional

In other words, the prevailing view has been that pretrial

factor needs to be established. Does the effect persist when simultaneous controls are used in analysis? Answers to these questions will be helpful in answering a fourth, namely, how to interpret the effect of confinement-delay.

Several possible explanations already exist. One is Foote's suspicion that the apparent relationship between pretrial detention and conviction might result from differences in evidential strength among cases of detained and released defendants. To appreciate the relevance of his concern over the possible spuriousness of the detention-conviction relationship, let us assume that we are dealing with offenses having relatively high degrees of seriousness (since it is now well-established that offense seriousness is the principal determinant of both the amount of bail and the probability of detention 102). Under that assumption, we can hypothesize that strength of evidence may account for both the extent of delay and the likelihood of conviction. That is, it may be that serious cases with strong evidence are more likely to move speedily toward conviction, whereas serious but evidentially weak cases are more likely to entail delay and ultimate dismissal. 103

Second, the confinement-delay effect may result from "deterioration" of evidence. This explanation refers mainly to changes in the strength of testimonial evidence(<u>i.e.</u>, eye-witness identifications, accounts of incidents by victims and witnesses, complainants' availability and willingness to cooperate with court officials) during the course of prosecution. Sometimes, however,

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the strength of non-testimonial evidence changes after initiation of prosecution (<u>e.g.</u>, crime-laboratory analysis proves a suspected narcotics substance to be "bunk," fingerprints "lifted" at a crime scene do not match a suspect's, blood stains found on a suspect's clothing turn out to be non-human). Of course, to the extent that evidence "deteriorates" over time, detained and released defendants would be expected to benefit equally from lowered chances of conviction. Notably absent from recent research on criminal court delay<sup>104</sup> is consideration of evidential "deterioration," even though the phenomenon is widely regarded as being the principal reason for any "loss" of convictions that may be attributable to delay.

A third possibility is to explain the confinement-delay effect as "informal punishment." According to this interpretation, the practice of detaining defendants who ultimately receive nonconviction dispositions is one of a number of techniques court officials routinely employ in conducting business. Pratrial detention is thus seen as an unauthorized but deliberate means of sanctioning defendants whom court officials believe cannot or should not be convicted of charges against them. The hypothesis that attrition represents "informal" or "pretrial" punishment enjoys considerable support among researchers.<sup>105</sup> In viewing pretrial detention, court delay, and attrition as elements

of what Nardulli calls the "dispositional strategy"<sup>106</sup> of court officials, however, we may run the risk of overstating an important truth about criminal court decision-making. It is unlikely that all pretrial detention of defendants whose cases

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are eventually dismissed can be explained in terms of "informal punishment." Rather the question would seem to be how much the rate of attrition can be explained in that way.

Attempts to resolve these issues will demand re-examination of several standard assumptions in empirical criminal justice research. Perhaps the most basic assumption is that decision-making reflects deterministic processes which make variations in outcomes largely explainable once principal determinants have been identified and properly measured. As both Feeley<sup>107</sup> and Myers<sup>108</sup> have recently observed, however, no empirical research on conviction-nonconviction outcomes yet reported has succeeded in accounting statistically for more than a small share of the total variation in outcomes. If we continue to produce results with similarly low levels of explanatory power, we are likely to find that any additional contribution made by confinementdelay to the conviction rate is quite modest. The persistence of large amounts of unexplained variation will lead us to pay closer attention to indeterminancy in legal decision-making. 109

Many researchers have also assumed that the effects of dispositional factors are additive and linear, although the results of sophisticated multivariate statistical analyses make that assumption increasingly untenable. Myers<sup>110</sup> is undoubtedly correct in maintaining that more complex relationships exist. Her own work provides an example of one such relationship. In that study,<sup>111</sup> a higher probability of conviction for black defendants appeared only after the dispositional effects of evidential strength were removed statistically. Another example

is an hypothesis linking acquisition of prior criminal records to the liabilities of socioeconomic status, minority group membership, age, and sex.<sup>112</sup> The confinement-delay hypothesis involves a relationship of similar if not greater complexity. Confinement delay may decrease the probability of conviction in some offenses (e.g., drugs) and increase it in others (e.g., burglary).

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Finally, a common though unarticulated assumption has been that all criminal courts make decisions according to the same set of factors and that they all assign similar weights to each of those factors. It is likely, however, that research on the dispositional effects of confinement delay (as well as other case-processing variables such as prosecutorial screening,

pleabargaining, and sentencing practices) will confront us with the problem of accounting for cross-jurisdictional differences in the strength, direction and form of relationships between decisions and the causal factors associated with them. An empirically-based theory of criminal court decision-making may need to explain why, e.g., confinement delay affects the probability of conviction in some jurisdictions but not others. Existing comparative studies<sup>113</sup> provide sufficient empirical evidence to suggest that the effects of particular case-processing variables differ across jurisdictions. Those studies also indicate that the interpretation of cross-jurisdictional differences requires a strategy of research combining the results of fine-grained statistical analyses with qualitative knowledge

acquired through intensive observation.<sup>114</sup> Through such efforts we can anticipate the development of more adequate theoretical models of decision-making in criminal courts.

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42. Ibid., p.200.

43. Ibid., p.206.

44. Ibid., p.205. Descriptive materials presented earlier in the study (pp.196-201) suggest that the difference may have been associated with certain peculiar features of bail administration in Baltimore. For one thing, police had effective control over bail-release decisions at the lower-court level. For another, the fragmented court system provided no incentive for lower-court judges to review arrest charges, with the result that defendants facing serious charges were likely to be both (a) detained and (b) remanded to the upper court.

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52. Ibid., p.746.

53. <u>Ibid</u>., p.211.

54. Ibid., pp.752-3.

55. Ibid., p.751.

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				80. <u>Ibid</u> ., pp.93-
69.	Ibid., pp.289-90. It will be recalled that Foote et al.			
	(1954, p.1054) raised a similar possibility with respect			81. <u>Ibid</u> ., p.94.
	to spuriousness, arguing that evidential strength may			
	increase the probability of both detention and conviction.			82. <u>Ibid</u> ., p.82.
70.	<u>Ibid.</u> , p.298.		6 J	83. <u>Ibid</u> ., p.94.
71.	. <u>Ibid</u> , p.289.			84. Ibid., pp.98-
		ne na serie de la constante de		
72.	. <u>Ibid.</u> , p.290.			85. Eisenstein ar
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74	. Landes, <u>op.cit</u> ., 1971.	a		correlation f
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77	. <u>Ibid</u> ., p.78.		normalization	v.5, pp.311-4
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convictions which would not otherwise have occurred (and thereby prevents attrition). Some writers estimate these to be as much as a third of all convictions. This chapter discusses the evidence for these and other theories about the impact of pleabargaining on attrition. While no system aspect creates greater dissatisfaction, pleabargaining is clearly a dominant characteristic of American criminal justice today. Pleas of quilty account for 80 percent or more of all convictions virtually everywhere.<sup>1</sup> While not all these pleas of guilty are attributable to pleabargaining, many are under any definition of pleabargaining, and most are under the broader definitions. Except for a few scattered jurisdictions which have attempted to abolish all pleabargaining, a great many of these guilty pleas are the result of explicit discussions between the prosecution and the defense, the defense and the court or all three concerning the consequences of the

The remaining pleas of guilty are not the result of explicit plea negotiations but rather instances in which the defendant pleads without prior discussions with either the prosecution or the court. Under the broadest definitions of pleabargaining many of these pleas are also considered to be the result of pleabargaining. Under these definitions pleas which result from practices and understandings that defendants who plead

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### PLEABARGAINING AND ATTRITION

Another important theory is that pleabargaining causes

guilty will receive lesser sentences than those who go to trial and are convicted are considered the result of "implicit" pleabargaining.<sup>2</sup> As most jurisdictions do have some differential, there are very few pleas of guilty not attributable to pleabargaining under these definitions. Even those jurisdictions which claim to have eliminated explicit pleabargaining usually continue to have this kind of "implicit pleabargaining."

Because pleabargaining under either definition is such a dominant aspect of the system and because the results of pleabargains are almost by definition always more ambiguous than those of trials, pleabargaining is blamed for virtually all the ills afflicting the system. On the one hand charge and sentence reductions are seen as unjustified detractions from deterrence and justice to victims, and to some extent as making it possible for the guilty to escape sanctions altogether.<sup>3</sup> On the other hand pleabargaining is seen as undermining the defense and leading innocent persons to plead guilty.

It seems clear that at least one of these claims is true--in a highly qualified way. The data available leave no doubt whatsoever that cases or charges against defendants are often dropped because of plea agreements in which the defendant agrees to plead guilty to charges in other cases. This was given as the reason for 22 percent of the post-filing nolles and dismissals in the Indianapolis PROMIS data and smaller percentages in other cities.4

While these dismissals detract from conviction rates based on all cases filed, they are hardly cases in which the defendant goes scot free. In all instances the defendant is found quilty of something. They are therefore more like charge reductions than dismissals. Whether they even result in sentence advantages to the defendant depends upon the nature of the agreements reached. It is often contended, for example, that the defendant receives the same sentence from the case in which he is convicted that he would have if convicted in each of the pending cases.<sup>5</sup> This issue of course is basic to the whole pleabargaining debate--to what extent does the plea represent simply a compromise reaching the same outcome as a trial and to what extent does it involve concessions in order to secure the defendant's agreement? Other than the studies indicated above, there is some rhetoric but no studies which indicate that pleabargaining causes fewer convictions. There are a great many studies, however, which indicate that pleabargaining causes a larger number of convictions. Barkai explains how this might come about: Innocent defendants may...offer pleas rather than contest their guilt at trial for several reasons. [They may]...because of the complexity of the criminal law, erroneously conclude that they have committed the crime charged when

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It is not unlikely that a large number of defendants are unaware of the exculpatory nuances of the law under which they believe they have committed a crime. This is especially true where individual elements of the crime are each

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they have not...[They may also...] because of prior experiences or pressures applied to them as they are processed through the criminal justice system, conclude that it is in their best interest to plead quilty although they know they did not commit the crime with which they are

Some authorities believe that there may be a large number of defendants in the first group:

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independent pre-conditions to conviction and each necessary of proof, or where the acts themselves are measured in fine degree when assessing whether a crime has taken place.'

Barkai also spells out some of the kinds of pressures

involved in this second reason:

An innocent defendant who is aware that he did not commit the criminal act might nevertheless decide to plead quilty because of: (1) the potentially overwhelming nature of the evidence against him; (2) the disparity in punishment between conviction by plea and conviction at trial; (3) a desire to protect family or friends from prosecution; (4) the conditions of pretrial incarceration; (5) a concern that fuller inquiry at trial may result in disclosure of additional facts which could increase the sentence in the present case or result in additional prosecutions; (6) a desire to expedite the proceedings because of feelings of hopelessness, powerlessness, or despair when faced with the power of the state; (7) pressure from family, friends or attorneys; and (8) "ignorance, deception, delusion, feelings of moral guilt, or self-destructive inclinations."

Barkai takes it as "given" that there are "innocent defendants who do plead guilty." Other knowledgeable commentators do the same.

One of the more extensive discussions of the issue is contained in the Georgetown pleabargaining study by Miller, McDonald and Cramer.<sup>10</sup> This study emphasizes the necessity for distinguishing between convictions involving defendants who are innocent because they did not commit the crime ("the factually innocent") and those who are "innocent" because there is insufficient legally admissible evidence for them to be convicted ("the legally innocent").

The study recites a number of cases from jurisdictions around the country in which judges, prosecutors and defense counsel make creditable reports of factually innocent defendants who plead guilty:

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A justice of the peace in Clark, County, Nevada...said that he knows defendants plead guilty in order to avoid going to trial even though they are innocent. He gave the example of old ladies who are brought into court for shoplifting. He said that a lot of times these elderly women are senile and don't know what they are doing when they go into a store and pick up an article and slip it into their purse. However, they plead guilty rather than going through the whole criminal justice process because it is too much of a strain on them.

and plead in the case. are innocent.

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Other officials indicated strong doubts that innocent defendants ever pleaded guilty. There was much greater agreement, however, that defendants whose cases might be difficult to prove often pleaded guilty. Prosecutors felt no compulsion to dismiss such cases, although they were often willing to

settle for the "half-loaf" of a lesser conviction. Some prosecutors are even willing to bluff in cases in which a witness is missing or unavailable, knowing that defendants are likely to go ahead

### A. Statistical Studies--Finkelstein

Several studies attempt to go beyond this kind of observational analysis and estimate the extent to which innocent defendants do plead guilty. One such effort is an analysis by Finkelstein. This study has been widely quoted as indicating that as many as one third of all defendants pleading guilty

This study is based on an analysis of guilty pleas and conviction rates in criminal cases handled by the federal district courts. The principal analysis concerns criminal cases terminated during 1970-1974 for the 29 largest districts.<sup>12</sup>

Comparing the rate of guilty pleas in these districts

with the rate of convictions the study found that districts with higher rates of guilty pleas also had higher conviction rates. Guilty pleas ranged from 35 percent of all convictions in the Northern District of California to 73 percent in the Southern District of Texas. Conviction rates on the other hand for these two districts were 59 and 87 percent.<sup>13</sup>

Using regression analysis the study concluded that pleabargaining accounted for nearly 70 percent of the greater number of convictions in the districts with the higher rates of pleabargaining. Because defendants everywhere pled guilty in at least 35 percent of the cases the study assumes that this is a normal number of guilty pleas. The 70 percent therefore applies only to pleas in excess of 35 percent.<sup>14</sup> Under this assumption in the districts with the highest guilty plea rates about a third of all defendants pleading guilty would not have been convicted if they had chosen to go to trial. The study does not attempt to determine the extent to which these defendants are factually as well as legally innocent but the author clearly believes that at least some are both.

The study found its analysis confirmed by a separate examination of plea and conviction rates for all federal district courts from 1908-1974.<sup>15</sup> This analysis found that plea rates during this time period had three general phases. Between 1908 and 1928 plea rates rose slowly from 30 to 70 percent. From 1929-1954 they increased much more slowly, peaking at 85.8 percent in 1951. From 1954-1974 the rates reversed and declined that period. 16

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to around 60 percent. Conviction rates generally followed suit, rising as the plea rates increased and dropping as the plea rates declined. Overall the increase in plea rates was found to account for 26 percent of the increase in convictions between 1908 and 1928 and the decrease in plea rates between 1954 and 1974 for 79 percent of the decrease in conviction rates in

In addition to the hypothesis that the greater number of convictions in the high conviction rate districts were attributable to their higher rates of pleabargaining the study considered whether the differences were due to other possible hypotheses--that the higher rates of conviction in the high conviction districts were attributable to lower workloads, greater staff competence, or differences in the kinds of crime handled. It found that none of these alternative explanations were valid.<sup>17</sup> One obvious question concerning the study's conclusion that higher rates of guilty pleas cause higher rates of conviction is how can the study be sure that it is not the higher rates of conviction in the high conviction rate districts which are causing defendants to plead guilty more frequently. The author considers this possibility, but rejects the hypothesis because

of his conclusion that the major alternative hypotheses--differences in workload, staff competence and crimes handled--do not explain the differences in conviction rates. In effect the study says that because there are no obvious indications that conviction rates vary for reasons other than the rate of pleabargaining, then pleabargaining must be the cause.

Even, however, if it is assumed that it is the high rate of convictions which causes the high rates of pleabargaining, rather than the pleabargaining which causes the convictions, the study says that many more defendants are still convicted than would be the case if all defendants went to trial. This conclusion is based on the assumption that plea cases going to trial would be convicted at the same rate as the cases which actually go to trial. Under this assumption 51 percent of the greater number of convictions obtained in the higher rate districts but not in the lower rate districts during 1970-1974 would have been attributable to the decision to plead. 18

This study represents an interesting attempt to develop more precise estimates about the effects of pleabargaining on conviction rates. It contains at least four basic flaws, nowever, which render its findings meaningless:

- (1) It does not consider the effect of different charging policies in the various districts.
- (2) It does not analyze adequately the differences among districts in the kinds of crime handled.
- (3) It ignores wide variations among districts in the likelihood of conviction in different kinds of cases.
- (4) It fails to develop any reasons for its assumption that the rate of conviction for the cases in which quilty pleas are entered would be the same as those for the cases which go to trial.

In the federal system many cases begin with investigations rather than with arrests by the police. Like their state counterparts, however, federal prosecutors exercise considerable control over which cases are filed in court. In 1976, for example,

U.S. attorneys nationally declined to file over 60 percent of all matters presented to them for criminal prosecution.<sup>19</sup> The studies available do not indicate the decline-to-file rate by district. The studies do indicate, however, that there is considerable variation among districts in the amount of screening done. As other studies have shown conviction rates are highly sensitive to screening procedures which weed out the weaker cases.<sup>20</sup> it seems likely that the individual district conviction rates used in the study are affected by the amount of screening done.

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A second problem with the study is its inadequate treatment of the differences in offenses among districts. The study acknowledges the possibility that such differences might explain the variations in conviction rates but dismisses the problem because drug violations constitute a major part of the caseload in most districts and because elimination of the immigration cases does not appear to change the calculations.

There is no attempt at statistical controls for offense. however, and a closer look at the data suggests that differences in the kinds of cases involved could make quite a bit of difference in the overall conviction rates for particular districts, particularly as the conviction rates vary considerably by offense. A comparison of the districts with the highest and lowest conviction rates, for example, shows enormous differences in the kinds of cases handled. Nearly a fifth of the cases in both districts were narcotics cases. In the high conviction

rate district, however, nearly 30 percent of the cases were immigration cases and 19 percent were marijuana cases. This compares with 3 percent immigration cases and 6 percent marijuana cases in the low conviction rate district. On the other hand 10 percent of the low conviction rate district cases were selective service cases, while this category accounted for less than one percent of the high conviction rate district cases.

Even more basic than these differences in the kind of case handled are differences in the likelihood of conviction based on differences in attitudes toward particular offenses. It is well established that government success rates in civil matters such as income tax vary enormously in different regions of the country.<sup>21</sup> It would be surprising if there were not similar differences in criminal matters. One does not have to be a genius in American regional variations, for example, to expect a difference in attitude between Houston and San Francisco in 1974 toward such offenses as marijuana and draft evasion. Table CC-1 indicates that there are in fact such differences. The conviction rates in this table may also be affected by differences in screening.

The problems thus far discussed relate to the study's principal method of estimating the number of defendants who are convicted as a result of pleabargains who would not be if they went to trial. This method involves a comparison of plea rates and conviction rates. The study also contains an Marijuana

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Narcotics

Selective service

Immigration

CC-10

### Table CC-1

### Conviction Rates in Two Federal District Courts--1974 (In percent)

	Offer		Convictions as a				
	OTTE						
	Pero	cent of	Perc	ent of			
	Cases	s Filed	Cases	Filed			
	High:	Low:	High:	Low:			
	Western	Northern	Western	Northern			
	District	District	District	District			
	Texas	California	Texas	California			
	19	6	86	30			
	17	19	73	29			
9	1	10	60	32			
	30	3	94	33			

Source: United States Administrative Office of the Courts, Federal Offenders in United States District Courts--1974.

alternate method of estimating the number of defendants convicted as a result of pleabargaining. This method assumes that if the cases which are not pleabargained were to go to trial, the resulting conviction rate would be the same as that of the cases which now go to trial.

In effect this method of estimation is based on an assumption that the cases which are pleabargained are similar to those which go to trial, or at a minimum that they are no stronger than the cases which go to trial. A fourth problem with the study is that this is by no means a self-evident proposition and no evidence is adduced to support it. The assumption is in fact contradicted by most of the social science literature and many of the legal writers about pleabargaining.<sup>22</sup>

It is interesting to note that while this study has been widely quoted in the legal literature,<sup>23</sup> it neither is discussed in the social science literature nor does it discuss the social science literature.<sup>24</sup>

Other recent studies based on federal district court data show much more concern for controlling district variations.<sup>25</sup> B. Rhodes-PROMIS Study

A more sophisticated attempt to provide estimates of the number of defendants convicted due to guilty pleas is a study by William Rhodes using PROMIS data for Washington, D.C.<sup>26</sup> This analysis also uses the results of cases which go to trial as a method of predicting what would have happened to guilty plea cases if the defendants had gone to trial. Unlike Finkelstein, had gone to trial. range, as shown in Table CC-2.

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It is difficult to know what to make of Rhodes' study. There is certainly some basis for believing that the factors which predict convictions in the cases going to trial should also predict reasonably well what would happen to other cases if they went to trial. Ultimately this proposition rests on an assumption, however, that both categories of cases are similar or that they are at least sensitive to the same kinds of indicators and that these are the indicators which are used in the model. Observers of pleabargaining often suggest, however, that the cases which go to trial are different from those which are

CC-12

nowever, Rhodes does not assume that the guilty plea cases are the same as the cases which go to trial.

Instead he uses the characteristics of the cases going to trial to develop a statistical model capable of predicting which cases going to trial will be convicted and which will be acquitted. He then applies this model to the guilty plea cases, finding that 30 to 40 percent of the defendants who pled guilty would probably not have been convicted if they

Taking advantage of the fact that the prosecutors in the District of Columbia themselves make estimates at the time of initial screening of the likelihood of conviction in each case, Rhodes develops a second method of estimating the likely outcome of trials in the guilty plea cases based on these prosecutorial judgments. This estimate too is in the 30 to 40 percent

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### Table CC-2

### Probability of Conviction: Actual and Predicted

	At Trial			By Plea		Nolle/Dismissal	
Charge	A	В	C	В	С	В	C
Assault	65%	6676	6952	6657	7156	59%	68%
Robbery	78	79	71	84	73	78	67
arceny	66	68	73	69	74	67	71
Burglary	67	67	73	68	74	64	70

Kev:

A Observed probability of conviction at trial = number of convictions total number of trials.

B Predicted probability of conviction had this case gone to trial, based on the regression equations reported in Table 1.

C Predicted probability of conviction, based on the prosecutor's estimate of the strength of the case at screening.

Number of cases analyzed: trials pleas'dismissals-

a picas uis	11131113	
Assault	246 404 806	
tobbery	185.336.653	
arceny	275,750 1166	
Burglary	177/534 610	

Source: W. Rhodes, Plea Bargaining: Who Gains? Who Loses? at 47 (Dec. 1978) (Institute for Law and Social Research).

settled, that the trial cases are in effect cases where outcomes either as to guilt or sentence are in doubt whereas the guilty plea cases are ones for which the outcomes are more certain.<sup>27</sup> One possible indication that there may be differences between the trial cases and the other cases is the operation of the statistical model with respect to the nolle and dismissal cases. The model predicts conviction rates of 59 to 78 percent for these cases.<sup>28</sup> These rates are astounding if true because they are virtually identical with those for the cases which went to trial and the cases in which there were guilty pleas. In effect the model predicts not only that defense counsel err 30 percent of the time in allowing defendants to plead quilty but that in addition prosecutors err 60 to 70 percent of the time in the cases which they dismiss or recommend nolle prosequi. In the absence of indications that counsel are highly incompetent -- and there are no such indications for the District of Columbia--these results at a minimum raise the question as to whether it is counsel who are making the errors or the model.

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It is of course not impossible that counsel are making errors of the magnitude indicated. And it is almost certainly true that some of the dismissal and nolle cases could be brought to conviction. Among other things the reasons given by the prosecutor for dismissals and recited by Rhodes indicate that many of these cases are really bookkeeping dismissals rather than dismissals on the merits.<sup>29</sup> These bookkeeping cases gum up the figures in ways that make it difficult to determine
the true conviction and dismissal rates and probably gum up the models as well. Even the hazy figures which remain, however, are enough to indicate that the model deals very poorly with witness problems insofar as the dismissals and nolles are concerned. 30

A second problem with Rhodes' analysis is that the model does not seem to work very well even as to the cases which go to trial. Rhodes himself acknowledges this:

Unfortunately, the regressions did not "fit the data" as well as the previous regressions on sentences. Still, using the regression results to predict the probability of conviction increased the proportion of correct predictions (relative to chance) from 54 percent to 68 percent for assault, from 65 percent to 79 percent for robbery, from 56 percent to 70 percent for larceny, and from 55 percent to 67 percent for burglary.

Moreover, even this analysis may be too generous. While the statistical model may show the improvements indicated over predictions based on coin flipping, it is not clear that it shows any improvement at all over predictions that all defendants will be found guilty.

The predictions by the prosecutors are even more delphic. If there were some accuracy to the predictions made, the figures would lend credence to Rhodes' calculations and conclusions. The predictions do not appear to be very accurate, however, and are probably best ignored.<sup>32</sup>

### C. Alaska Study

A third empirical study concerning the effect of pleabargaining on conviction rates is the Alaska pleabargaining

study. This study analyzes the elimination of explicit pleabargaining by the prosecutor in Alaska. Two study reports have been made: an interim and a final report.

Interim Report. The interim report analyzed the effect This finding is directly contrary to that of Finkelstein

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of the change upon misdemeanor conviction rates.<sup>33</sup> Noting that the change in policy had increased the number of trials, the interim study found that the overall rate of convictions had increased slightly--from 71 to 77 percent. Within these overall figures there were much larger changes in conviction rates for individual offenses, some up and some down. Increased conviction rates for a few offenses were found to be due to tighter screening by the state attorney general's office. More filings and lower conviction rates for several other offenses were found to be due to increased community pressure for action with respect to these crimes. The study drew no overall conclusions, but a fair interpretation of the data would appear to be that the elimination of pleabargaining neither increased nor decreased the proportion of misdemeanor arrestees who were convicted. and at odds to some degree with Rhodes' analysis. Finkelstein's analysis simply says that the more trials the fewer the number of convictions. Rhodes at one point at least goes somewhat beyond this, saying that the number of convictions goes down with an increased number of trials but up with an increased differential between sentences which result from trials and those which result from quilty pleas. The Alaska interim results

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would be consistent with Rhodes' analysis therefore only if there were an increase in the sentencing differential as a result of the elimination of pleabargaining.

The Alaska interim report refuses to draw firm conclusions about these matters but suggests that a differential exists and gives some indications that this differential has increased since the elimination of pleabargaining.

<u>Final Report</u>. The final report analyzes the effects on felonies of the ban on pleabargaining. The study indicates that the ban was largely effective in eliminating negotiations and sentence bargaining and that charge bargaining had never been particularly important.<sup>34</sup> As expected, the study found that the number of cases screened out by the prosecutor without filing increases slightly--from 10 to 12.9 percent of all felony arrests. The study also found that the number of cases going to trial increased, but only slightly--from 6.7 to 9.6 percent of cases filed.<sup>35</sup>

The study authors did not discuss the Finkelstein theory that pleabargaining results in more convictions or Rhodes' somewhat more complicated theories to the same effect. On the contrary they expected the ban to result in more convictions. "Since one purpose of the new policy was to reduce unjustifiable concessions to the defendant," they said, "we expected dismissals to become less frequent.<sup>36</sup>

The principal measure of convictions used in the study was the percentage of cases filed in court which resulted in active prison sentences of 30 days or more. Measured in this percent.<sup>37</sup> D. Conclusions

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way, "convictions" did increase slightly--from 17.2 to 18.9

This method of measurement combines the conviction and the sentencing decision, however. If convictions are measured alone, the rate declined--from 40.7 to 39.2 percent of cases filed. And if measured on the basis of felony arrests, the decline was somewhat larger--from 36.6 to 34.1 percent.<sup>38</sup> These declines are not very great and constitute further strong evidence that the effect of pleabargaining on convictions is much less than that suggested by either Finkelstein or Rhodes. The decline in conviction rates was greatest for the property crimes. There was also a change in sentencing patterns for these crimes. Prior to the ban persons who were convicted at trial were sentenced more severely than those who pled guilty.

After the ban, there was little difference in the sentences for these two groups.<sup>39</sup> These results are somewhat supportive of Rhodes' idea that the effect of pleabargaining on conviction rates is related to whether persons who are convicted at trial are punished more severely than those who plead guilty. The fact that the combined effects of eliminating both pleabargaining and the sentencing differential did not cause greater declines in the conviction rate, however, suggest that the theory is basically a weak one.

There is a widespread belief among prosecutors, defense lawyers, judges and researchers that sentencing concessions

to defendants who plead guilty lead to a greater number of convictions than a system in which no such concessions are made. Seeking to quantify these effects, Finkelstein and Rhodes have each concluded that pleabargaining accounts for around a third of all convictions. There are serious methodological flaws in these estimates, however, and it seems likely that insofar as felony cases are concerned the increase in convictions over a system with no pleabargaining at all is in most jurisdictions no more than 5 to 10 percent and possibly less.

The increase in convictions insofar as misdemeanors and minor offenses is concerned may well be greater. Many defendants charged with these offenses serve no additional time if they plead guilty and thus have a powerful incentive to plead.

While the principle has not been well demonstrated statistically, it seems likely that the increase in conviction rates for all offenses is related to the magnitude of the sentencing concessions made--the greater the concessions the greater the likelihood of a guilty plea. Because most jurisdictions already rely heavily on pleabargaining for processing cases it seems doubtful that conviction rates could be appreciably increased through greater use of pleabargaining without serious erosion of criminal sanctions.

1. President's Commission on Law Enforcement and Administration of Justice, The Challenge of Jrime in a Free Society (1967); K. Brosi, A Cross-City Comparison of Felony Case Processing at 35 (April 1979). 2. This term appears to have been developed by D. Newman,

Conviction (1965). See also H. Miller, W. McDonald, and J. Cramer, Plea Bargaining in the United States 84-121 (Sept. 1978) (National Institute of Law Enforcement and Criminal Justice). Note, Restructuring the Plea Bargain, 82 Yale L.J. 286 (1972)(suggesting that the defendant be informed of the discount factor); Kaplan, American Merchandizing and the Guilty Plea: Replacing the Bazaar with the Department Store, 5 Am. J. Crim. L. 215 (1977).

3. See, e.g., CLO News, March 1979, p. 3, col. 1 (Newsletter of Citizens for Law and Order).

4. K. Brosi, A Cross-City Comparison of Felony Case Processing 20 (April 1979).

5. M. Rubinstein, S. Clarke and T. White, Alaska Bans Plea Bargaining (July 1980) (National Institute of Justice).

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#### Appendix CC--Notes

6. Barkai, Accuracy Inquiries for All Felony and Misdemeanor L 16. Id. Pleas: Voluntary Pleas But Innocent Defendants, 126 U.Pa.L. Rev. 88, 96 (1977). 17. Id. at 304-309. 7. Clicque v. United States, 514 F.2d 923, 931 (5th Cir. 18. See note 14. 1975). 19. Comptroller General, Report to the Congress: U.S. Attorneys 8. Barkai, supra note 6, at 96-97. do Not Prosecute Many Suspected Violators of Federal Laws (Feb. 1978). See also Frase, The Decision to File Federal 9. Id. at 97. See also Alschuler, The Prosecutor's Role in Criminal Charges: A Quantitative Study of Prosecutorial Plea Bargaining, 36 U.Chi. L.Rev. 50 (1968); Newman, Conviction Discretion, 47 U.Chi. L. Rev. 246 (1980). 67-75 (1965). 20. J. Jacoby, The American Prosecutor: A Search for Identity, 10. H. Miller, W. McDonald, J. Cramer, Plea Bargaining in 11-1 Ĵ 195-215 (1980); P. Nardulli, The Courtroom Elite 160-61 the United States 84-121 (Sept. 1978)(National Institute (1978). Alschuler, Plea Bargaining and Its History, 79 of Law Enforcement and Criminal Justice). Colum. L. Rev. 1, 28 n.151 (1979) criticizes Finkelstein for failing to consider screening as an explanation of 11. Id. at 88-89. his findings. 12. Finkelstein, A Statistical Analysis of Guilty Plea Practices 21. Administrative Conference of the United States, Report in the Federal Courts, 89 Harv. L. Rev. 293 (1975). on Tax Administration (1977). 22. L. Mather, Plea Bargaining or Trial? 65 & ff. (1979); 13. Id. at 300. M. Rubinstein, S. Clarke and T. White, Alaska Bans Plea 14. Id. at 299-300. Bargaining 148-49 (july 1980) (National Institute of Justice). See also P. Nardulli, The Courtroom Elite 190-97 (1978). 1 15. Id. at 301-304. Ś CC-22

23. It is cited in at least 16 articles.

- 24. The only social science study cited by Finkelstein is Newman's Conviction. Finkelstein in turn is not mentioned in Rhodes' study, note 26 infra, or most other social science studies.
- 25. S. Flanders, Case Management and Court Management in United States District Courts (Sept. 1977) (Federal Judicial Center); Gillespie, The Production of Court Services: An Analysis of Scale Effects and Other Factors, 5 J. Legal Studies 243 (1976).

26. W. Rhodes, Plea Bargaining: Who Gains? Who Loses? (Dec. 1978)(Institute for Law and Social Research).

27. See note 22 supra.

28. Rhodes, Plea Bargaining: Who Gains? Who Loses?, at 47 (Dec. 1978).

29. Id. at 50-51.

30. Id.

31. Id. at 44.

32. Id.

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33. Alaska Judicial Council, Interim Report on the Elimination of Plea Bargaining (May 1977).

35. Id.

36. Id. at 121.

37. Id.

38. Data from table VI-2.

39. Id.

40. Callan, An Experience in Justice Without Plea Negotiation, 13 Law and Society Rev. 327 (1979) discusses the El Paso experience without differential sentencing. Other accounts of the El Paso experience indicates that the trial rate and the attrition rate increased. See also S. Wildhorn et al. Indicators of Justice: Measuring the Performance of Prosecution, Defense and Court Agencies Involved in

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34. M. Rubinstein, S. Clarke and T. White, Alaska Bans Plea Bargaining 148-49 (July 1980)(National Institute of Justice).

Felony Proceedings 98 (May 1977) which indicates that conviction rates increased in Portland when pleabargaining was curtailed. There is no discussion of possible changes in charging policy, however, in this account.

The reason most frequently advanced for failure of the criminal justice system to achieve its goals is that it has far too many cases for the available resources. Heavy workloads or too little resources have often been advanced as reasons for low apprehension rates, inadequate investigations, court delay, ineffectiveness of counsel, excessive pleabargaining and system attrition. A January 1981 statement to the New York Times by Manhattan District Attorney, Robert Morgenthau, is typical. Commenting on recent findings that 99 of every 100 persons arrested in New York on felony charges never serve a state prison term of more than one year and that more than 80 are not prosecuted as felons, Morgenthau said that if prosecutors had more money and there was better investigative work, more cases could be prosecuted as felonies.<sup>1</sup> This chapter discusses the relationship between resources and workload and convictions. It is generally assumed that larger workloads without more resources decrease effectiveness, while greater resources increase effectiveness if workloads are not increased. Although the general way in which workload and resources might influence the conviction rate is similar, the concepts involved are not identical and the possibility always exists that one may change without affecting the other. The clearest example of this is unused capacity. If current resources are not being used to their full capacity, workload

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#### Appendix DD

#### RESOURCES AND ATTRITION

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could presumably be increased without decreasing effectiveness. This means that the impacts of each should be examined separately. A. Police Resources

It is widely believed that greater numbers of police officers and greater police resources help to reduce crime. Just how these additional resources help with the problem is not usually discussed but there are at least three ways in which they might: (1) by deterring more crimes as a result of increased police presence, (2) by apprehending more criminals, and (3) by providing more evidence against criminals who are apprehended and thereby increasing the proportion of arrestees who are convicted.

There are a number of studies which explore the relationship between expenditures on police and crime in general. There are virtually none, however, which examine the impact of police resources upon convictions.

Even the relationship between police resources and apprehensions has not been studied to any great extent. The relationship does, however, show up as an intermediate variable in studies of the relationship between apprehension risk and crime and studies of the relationship between police resources and crime. Because there are some reasons for believing that the relationship between police resources and convictions may be similar to that between police resources and apprehensions,<sup>2</sup> this section examines what is known of the relationship between police resources and apprehensions.

(1) <u>Police Resources and Apprehension Risk</u>. The most thorough recent analysis bearing on the relationship between police

resources and apprehension risk was made by the National Academy of Science Panel on Deterrence and Incapacitation as a part of its analysis of the relationship between apprehension risk and crime.<sup>3</sup> In this analysis police resources was a key intermediate variable.

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Studies of this kind must begin with one of two alternate assumptions. One set of five studies examined by the panel assumed that police resources play an important role in apprehensions and that increases in resources "enhance the apprehension risk by applying more resources to the solution of each crime." Increased crimes on the other hand without more resources were assumed to reduce the apprehension risk.<sup>2</sup> This group of studies also assumed that the resources allocated to the "criminal justice system in general, and to the police in particular, are affected by the crime rate"<sup>4</sup> and that increases in crime result in increased police budgets.

A number of these studies found that higher clearance rates (generally described as increased apprehension risk) had an impact on crime. The National Academy panel, however, criticized most of these studies because they did not properly consider the effects which the crime rate has on expenditures for police services.<sup>5</sup> The one study which did consider this factor in a proper way found no relationship between expenditures for police services and the clearance rate.<sup>6</sup> The panel also considered the alternate set of assumptions which might be adopted, that is, that resources do not play an important role in apprehension. According to the panel:

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It can be argued that the apprehension risk is largely determined by the administrative policies governing police operations and that this is a matter of management style and not particularly influenced by crime rate. This management style might be reflected in such policies as police deployment strategies and the incentives offered for making arrests. For example, in departments where the law enforcement aspect of policing is emphasized, there are likely to be increased incentives for making arrests, resulting in a more aggressive pursuit of enforcement activities. This is likely to lead to increased contacts between the police and suspects, thereby increasing the apprehension risk in these departments. From this perspective, professional standards and organizational incentives, rather than the level of police resources, are the predominant determinants of the apprehension risk.

Other reasons for believing that resources played a minor

role in apprehensions were that:

Most apprehensions are made by the police either at the scene of the crime, in direct pursuit from the scene, or after a positive identification by a witness (Greenwood et al. 1975). Furthermore, work load studies of police indicate that the time spent making apprehensions represents only a small proportion of the patrolman's duty time (Blumstein and Larson 1970).

The panel concluded that it was not possible at this time to resolve the question as to whether the availability of police resources affects apprehension risk.<sup>9</sup>

The National Academy panel mentions but does not fully analyze a recent study by Wilson and Boland which suggests that there is a relationship between police resources and apprehension risk. This study found that a greater ratio of arrests to crimes resulted in decreased crime, that this ratio was in turn affected by the number of patrol units on the street, and that this in turn was affected by the number of sworn officers per capita.<sup>10</sup>

bearing on relationship between police resources and apprehensions are those which seek to analyze the relationship between police resources and crime. These studies typically proceed on the theory that increased resources will result either in increased deterrence or in increased apprehensions. The effect on convictions is generally not discussed.<sup>11</sup> Ignoring a number of older and very flawed studies which found that increased police resources resulted in increased amounts of crime, <sup>12</sup> the National Academy panel concentrated its attention on three more recent studies using comparisons among cities or Standard Metropolitan Statistical Areas.<sup>13</sup> In all three studies police resources and the crime rate were assumed to be simultaneously determined, that is, each assumed that increased resources help on the one hand to reduce the crime rate and that on the other that an increase in the crime rate leads to greater police resources.<sup>14</sup> The panel summarized the findings of the studies as follows: When police resources are measured by the annual expenditures on the police, the estimated effect of police resources is negative and more than twice its asymptotic standard error, which is consistent with the hypothesis that police resources have a crime-reducing effect. However, when police resources are measured by the number of police officers, the estimated effect is positive.

The panel then analyzed the studies in greater detail,

finding that two of the studies had probably used improper estimation methods. The one study which used fully plausible methods found that expenditures on police have a highly significant negative effect on crime. The panel concluded that:

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(2) Police Resources and Crime. Another set of studies

While this suggests that increased police resources do have an inhibiting effect on crime, the conclusion must still be regarded as tentative since the result derives from only one study.

The panel also noted a further problem in analyzing the

impact of resources:

Furthermore, that analysis relies on total expenditures as a measure of the level of police resources. To the extent that increases in resources are converted to higher salaries, to pensions, or to fringe benefits, they are not likely to affect either police performance or visibility. The use of theoretically more meaningful variables -- such as the number of units on patrol--would be a more satisfying measure of the impact of police resources on crime. From the analysis, it is also impossible to identify how much of the effect is due to increases in the actual apprehension risk and how much is due to changes in the perceived risk. An analysis that also includes the actual apprehension risk as a determinant of crime would permit, a separate determination of the sources of the effect.

#### B. Prosecutorial Resources and Workloads

There is a great deal of talk everywhere about the heavy workloads faced by prosecutors (and defense counsel). These workloads could affect conviction rates in at least three ways:

- --They could lead to more pleabargaining, with whatever effects increased pleabargaining has.
- --They might cause prosecutors to file fewer cases in court or to adopt particular screening strategies.
- --They might cause prosecutors to lose more cases in court due to poorer preparation.

The National Academy of Sciences panel considered some of these possibilities in the course of its discussion of the effects of the risk of conviction on crime. The panel noted that nearly all of the studies of the effects of convictions on crime assumed without detailed discussion that prosecutorial workloads had no effect on convictions.<sup>18</sup>

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It might be argued, for example, that prosecutors adapt to increased workloads principally by offering reduced prison sentences or probation in return for guilty pleas from those who could be convicted in court anyway.

The panel considered this to be a relatively weak argument,

however, and thought it more likely that increased workloads do affect convictions. The panel was unsure, however, as to

what the effects are. On the one hand the panel suggested:

Increased crime rates might...reduce the conviction risk giver, a crime if there is an increased propensity for overworked prosecutors to lose cases either because of hasty preparation or because of an increase in the proportion of cases dropped before trial.

On the other hand the panel suggested that increased workloads

might increase the use of pleabargaining which in turn could result in increased convictions because of quilty pleas (with

less severe punishment).<sup>21</sup>

The panel was not impressed with the one study found which

assumed that workload did have effects. It thought that the assumption was plausible, but that the study had a flaw in its method of reaching its conclusions.<sup>22</sup>

The econometric studies considered by the panel are useful

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for the insights they provide, but are not direct evidence on the impact of workload on convictions because this impact is always treated as an intermediate variable rather than the principal focus of the study.

There are, however, a number of studies which analyze

the impact of prosecutorial workload on pleabargaining more specifically. These are discussed in the next section.

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The panel thought that an argument to this effect could

(1) Pleabargaining and Workload. While it now seems clear that pleabargaining became widespread in many jurisdictions during the late 1800's and the predominant method of disposition in most jurisdictions by the 1920's, the practice was little noticed in the literature until the great crime surveys. 23 Even the first of these largely ignored the practice but the later surveys all commented on it extensively.<sup>24</sup> Workload pressure was a frequent explanation for the high rate of guilty pleas. Bettman's summary for the Wickersham Commission was typical. He thought the rates were attributable in large part to the "immense volume of cases thrown upon prosecutors," and along with others advocated increased criminal justice resources as one of the reforms urgently needed.<sup>25</sup>

Later analysts also attributed the practice in large part to workloads. Samuel Dash in the 1950's, Donald Newman, Albert Alschuler and Harry Subin in the 1960's and dozens of others in the 1970's.<sup>26</sup>

Almost from the beginning, however, workload was just one of a number of explanations, and was rarely put forward as the sole cause of pleabargaining. Many saw the phenomenon as a method for reducing risks for both the defense and the prosecution.<sup>27</sup> More recently a number of studies have directly challenged the idea that pleabargaining is due to workload pressure.<sup>28</sup>

Milton Heumann analyzed the rate of guilty pleas in the Connecticut Superior Court during the period 1880-1959 and in a number of individual courts, showing that the proportion

also analyzed the effects of a decrease in the workload of the Superior Court in 1971 which was not accompanied by a corresponding decrease in resources. This finding was that the decreased caseload pressure did not result in less pleabargaining. He concluded that "rather than being simply an expedient dictated by unmanageably large case loads, pleabargaining is integrally and inextricably bound to the 'trial' court."<sup>30</sup> A second recent study which challenges the idea that heavy caseloads lead to more pleabargaining is Nardulli's analysis of a group of Chicago felony cases handled in the Cook County Circuit Court in 1972-1973.<sup>31</sup> Using multiple regression techniques this study found that neither the caseload pressure on individual judges nor the caseload pressure for the court as a whole had any impact on the rate of guilty pleas. Rather the study found that during periods of heavy caseload pressure the judges increased the pressure to plead quilty by adding to the penalty generally imposed on defendants who were convicted at trial instead of pleading guilty. The author hypothesized that increased caseload pressure did not lead to more pleabargaining because the court was not operating at full capacity, a conclusion that was bolstered by observational evidence concerning the number of hours worked. 32 A number of other researchers report similar findings. One study using opinions from about half of all Illinois defenders, prosecutors and public defenders, in fact found that lower caseloads instead of reducing pleabargaining had a slight tendency to increase the rate of pleabargaining. 33

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of cases going to trial had changed little over time.<sup>29</sup> Heumann

A recent study which suggests that the amount of pleabargaining is related to case pressure is Malcolm Feeley's study of two Connecticut lower courts.<sup>35</sup> Comparing the decision process in New Haven where the workload was heavy and a nearby court where the caseload was only one third as great, Feeley found much more charge reduction in the heavy caseload court:

Thirty percent of all guilty pleas in New Haven involve a plea to a lesser (or substituted) charge, while this is the case for only 11 percent of the guilty pleas in the low-volume court. In the low-volume court it seems there is less need or pressure to settle cases by reducing  $\frac{36}{36}$ charges.

Eighteen percent of these charges were reduced in the comparison court, while 70 percent were reduced in New Haven. Because the character of the workload in the two courts, their charging practices and perhaps other aspects of court operations differ and because there are problems in defining pleabargaining, these figures cannot be taken as proof that caseload induces a great amount of pleabargaining.

Feeley himself is extremely cautious in interpreting this data. While the data tends to indicate that higher caseloads do induce greater amounts of pleabargaining, both his overall statistical data and his observational evidence lead him to conclude that caseloads are overrated as explanations for court practices. 37

This line of argument squares well with a number of other recent studies of court processes. While not bearing directly on the relationship between caseloads and pleabargaining, these studies suggest that caseload pressure and the availability

as delay than previously thought. 38 (2) Other Methods of Influence. Heavy prosecutorial caseloads may also influence conviction rates in a variety of other ways. They may induce more prosecutorial screening. They may force prosecutors to go to trial when inadequately prepared, or they may lead to more nolles or dismissals. The early crime surveys attributed the great number of nolles observed to high caseloads. Lashley in the Missouri survey, for example, found the Missouri prosecutors so overworked that they could not perform such rudimentary tasks as preparing instructions for juries, interviewing witnesses or attending coroner's inquests. 39 With the limited exception of the recent evaluations of career criminal prosecution units (discussed in appendix EE), no very current studies have been found analyzing the way in which resources affect case preparation and how this in turn affects convictions. While there are no empirical studies demonstrating the linkage between heavy caseloads and the widespread development of early screening, it seems clear that caseloads have played an important part in this development. The relationship between screening and convictions is discussed in appendix EE. C. Conclusions Common sense says that there is a relationship between resources and caseloads and the attrition rate. Too many cases means that some get slighted and that convictions suffer. It

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of resources have much less to do with such court problems

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is puzzling therefore to find the studies so equivocal about the effects of resources on convictions--slight effects on police performance and limited or no effects on prosecutorial performance.

Perhaps we should not be too surprised, however. Studies in other fields indicate that reducing workload also has a negligible effect in the performance of public schools, probation officers, and welfare workers. And Peter Drucker reminds us that resource problems are the most common complaint for problems of performance in almost every field of business and government. 40

What are policymakers to conclude about these findings, however? City and county fathers are regularly asked to pony up more money in hopes of gaining greater control over crime. Are they foolish to do so? Can the conviction rate be affected by more police, prosecutors or resources?

The studies available give us little guidance on these questions. They suggest caution about expecting too much from increased resources but are far too limited to indicate that increased resources cannot help.

Given the importance of the questions and the fact that resources are one of the few levers of control which policymakers have, it seems clear that there is a need for much more detailed study of these issues. The incremental effects of increases in resources within single jurisdictions have hardly been studied at all, and the same is true for the effects of gradual declines.

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There is an urgent need for studies of the effects of setting clearer priorities in the use of available resources. James Q. Wilson, for example, indicates that a sizeable reduction in investigations by the FBI resulted in a larger number of convictions.<sup>41</sup> This suggests that one reason the studies to date are so equivocal is that there is considerable room in many agencies for better management of existing resources. If this is true, additional resources are likely to result in better performance only if coupled with changes in the pattern

#### Appendix DD-Notes

1. N.Y. Times, January 1981, p.1, col. 6.

- 2. The principal similarity is the fact that both apprehensions and convictions require proof of a crime and a connection of this defendant to the crime.
- 3. National Academy of Sciences, Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates 16-63 (1978) (Blumstein, Cohen and Nagin, eds.).
- 4. Id. at 32. Five studies were examined: Phillips and Votey, An Economic Analysis of the Deterrent Effect of Enforcement on Criminal Activities, 63 J. Crim. L.C. & P.S. 336 (1972); Carr-Hill and Stern, An Econometric Model of the Supply and Control of Recorded Offenses in England and Wales, 2 J. Pub. Economics 289 (1973); W. Vandaele, "The Economics of Crime: An Econometric Investigation of Auto Theft in the United States," in American Statistical Association, 1973 Proceedings of the Business and Economics Section 611 (1973); K. Avio and S. Clarke, Property Crime in Canada: An Econometric Study (1974) (prepared for the Ontario Economic Council); Pogue, Effect of Police Expenditures on Crime Rates: Some Evidence, 3 Public Finance Quarterly 14 (1975).

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5. Id. at 32.

6. Id. at 33, discussing Pogue Effect of Police Expenditures on Crime Rates: Some Evidence, 3 Pub. Finance Q. 14 (1975).

7. Id. at 34-35.

8. Id. at 35.

9. Id. The panel also concluded that it was not possible to determine whether crime in general affects apprehension risk, i.e., whether crime and apprehension risk are simultan-

eously or non-simultaneously determined.

10. J. Wilson and B. Boland, "Crime" in The Urban Predicament 179, 220 (1976).

12. These are all older studies.

13. The three studies were McPheters and Stronge, Law Enforcement Expenditures and Urban Crime, 27 National Tax Journal 633 (1974); Swimmer, Measurement of the Effectiveness of Urban Law Enforcement -- A simultaneous Approach, Southern

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11. The studies are similar to the non-simultaneous studies in this respect.

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24. Compare R. Pound and F. Frankfurter, eds., Criminal Justice R Economic Journal no. 4, 40 (1974); Greenwood and Wadycki, in Cleveland (1922) with Missouri Association for Criminal Crime Rates and Public Expenditures for Police Protection: Justice, The Missouri Crime Survey, 148 (1926). Their Interaction, 31 Rev. of Social Economy 232 (1973). 25. National Commission on Law Observance and Enforcement, Report on Prosecution 96 (1931)(Report No. 4). 14. National Academy of Sciences, Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime 26. Dash, Cracks in the Foundation of Criminal Justice, 46 Rates 44-45 (1978)(Blumstein, Cohen and Nagin, eds.). Ill. L. Rev. 385 (1951); D. Newman, Conviction (1965); Alschuler, The Prosecutor's Role in Plea Bargaining, 36 15. Id. U. Chi. L. Rev. 50 (1968); H. Subin, Criminal Justice in a Metropolitan Court -- The Processing of Serious Criminal 16. Id. at 45. Cases in the D.C. Court of General Sessions (1976); A. Blumberg, Criminal Justice xi (1970). 17. Id. at 45-46. 27. See, for example, Alschuler, supra note 26, at 59-60; North Contraction Kaplan, The Prosecutial Discretion--A Comment 60 Nw. U. 18. Id. at 22-53. L. Rev. 180-186 (1965). 19. Id. at 43. 28. M. Heumann, Plea Bargaining (1978). See also Heumann, Plea Bargaining and Caseload Pressure, 1975 Law & Soc. 20. Id. Rev. 515 (1975). See also note 31-37 infra. 29. Id. at 27-33. 21. Id. 30. Id. at 156-57. 22. Id. at 44. 31. Nardulli, The Caseload Controversy and the Criminal Courts, 70 J. Crim. L. & Criminclogy 89 (1979). 23. Alschuler, Plea Bargaining and Its History, 79 Colum. L. Rev. 1 (1979). DD-17

32. Id. at 98 n. 29.

- 33. Jones, A Research Note on Caseloads, Plea Bargaining and the Operation of the Criminal Justice System, 5 Justice System J. 88 (1979).
- 35. M. Feeley, The Process is the Punishment 244-77 (1979). This study is notable for its care in defining terms and its attempt to develop meaningful indicators.

36. Id. at 253.

37. Id. at 262-76.

38. T. Church, A. Carlson, J. Lee, T. Tan, Justice Delayed (1978)(National Center for State Courts).

39. A. Lashly, "Preparation and Presentation of the State's Case", in Missouri Association for Criminal Justice, The Missouri Crime Survey 113 (1926).

40. P. Drucker, Management: Tasks, Responsibilities, Practices (1974) and Technology, Management and Society (1970).

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41. J.Q. Wilson, The Investigators 131 (1978).

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In criminal justice as in other lines of human activity superior organization often produces superior results. In this chapter we discuss the impact of organization upon conviction rates.

There is a considerable literature about what constitutes effective police organization.<sup>1</sup> This literature is largely based on general management concepts and concerns such matters as chain of command, appropriate span of control, and staff selection and retention. The benefits of following good principles of organization, staffing, and training are more or less assumed, and neither the standard texts nor the secondary literature make any strong effort to demonstrate that good organization produces good results.<sup>2</sup> Goals and possible impacts--to the extent that they are discussed at all--are defined in very general terms such as reducing crime or maintaining order. There is some discussion of questions such as whether it is better to try to prevent crimes or to apprehend criminals, but even as to these no effort is made to evaluate the impact of different forms of organization.<sup>3</sup> There are a great many ways in which police organization might affect conviction rates. Some of these are: --Convictions might or might not be established as an important agency goal.

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#### Appendix EE

SYSTEM ORGANIZATION AND ATTRITION

#### A. Police Organization

--Incentive structures might include or exclude convictions.

--Organizational elements which assist in obtaining convictions might be given generous or parsimonious shares of the internal allocations of resources.

#### (1) Incentives

One way in which organizations affect the behavior of employees and agency performance is through the use of incentives and performance standards. A great many police agencies employ arrests or clearances in this way but few appear to use convictions.

The literature abounds with discussions of the role that arrests play in promotion and retention of police officers. In some departments one of the principal criteria by which patrolmen are promoted to detective or to supervisorial posts is the patrolman's record for arrests.<sup>4</sup> It is not uncommon for patrol officers to have quotas or guidelines as to the number of arrests they are expected to produce each month.

This use of arrest as an incentive or performance criteria has frequently been criticized. James Q. Wilson, for example, says:

The central problem of the patrolman, and thus of the . police is to maintain order and to reduce, to the limited extent possible, the opportunities for crime. Neither objective is served by judging men on the basis of their arrest records.

Wilson argues that using arrest in this way tends to create a legalistic police style which results in higher arrest rates. He believes that this is true particularly for minor crimes but that rates for major crimes may be affected as well.<sup>6</sup>

If Wilson's conclusions are correct, it is possible that conviction rates are also affected. There is no empirical data

available concerning this, but common sense notions suggest that higher arrest rates are likely to mean lower conviction rates -- the idea being that the more serious and more flagrant offenses are likely always to result in arrests while the weaker and less flagrant cases result in arrests only in the more aggressive departments or by the more aggressive officers. While this result seems intuitively likely, it is also possible that the factors leading to nonarrest are related more to the desirability of intervention than to convictability and that conviction rates would therefore not be affected. Just as patrolmen are often judged by their arrest performance, detectives are often judged by arrest records or clearance rates. Instances of this were observed during the course of this study. Detectives in one department indicated that if they were running low on arrests or clearances at the end of the month they would go down to Smith park where something was always going on and make their quota of arrests and clearances.

the lowest conviction rate.

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No department was observed and none has been identified from the literature in which convictions are used as a measure of performance. In one department observed convictions were included in the monthly statistics maintained on the squad bulletin board, but this statistic did not seem particularly important to members of the unit. Significantly, the detective who consistently had the highest arrest rate also had by far

Recent concerns about societal and governmental productivity have led to several studies of police and criminal justice

product vity. Two of the more prominent of these are the study of indicators of performance by the Rand Corporation and the study of police productivity by the American Justice Institute.<sup>7</sup>

The American Justice Institute's Police Program Performance Measurement Project was the more detailed of these two systems. This system creates a framework for analyzing police performance built around five broad goals: crime prevention, crime control, conflict resolution, services and administration. For each goal there is a set of measurable objectives and for each measurable objective one or more effectiveness and productivity measures. The principal goal relating to convictions is that of crime control. This goal has six measurable objectives:

--Police knowledge of crimes

--Crime case closure

--- Case preparation and testimony

--Stolen property return

--Constitutional propriety

--Custody of prisoners

Convictions are considered to be neither a measurable objective nor an effectiveness or productivity measure. Crime case closure is defined to include some independent verification that the case has been closed but is not judged by convictions. Similarly case preparation and testimony is judged by police and prosecutorial satisfaction rather than by convictions.<sup>8</sup>

McIntyre illustrates police attitudes toward convictions from the reports of police agencies, taking these reports to be good indicators of agency concerns:

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The typical police department annual report usually contains statistical information setting forth...crimes reported or known to the police and crimes cleared by arrests. The report rarely mentions the number of arrests resulting in convictions or, of those convicted, how many go to jail.

not their concern.<sup>10</sup> on conviction rates. in performance.<sup>12</sup>

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McIntyre concludes that "it is not that the police are uninterested in the disposition of cases at the prosecution and adjudication levels," but rather "that their concern is secondary." He indicates that in some departments "police develop the notion that what the prosecutor and the judge do with a case is their business" and that the proper police attitude is to remain emotionally detached from these things which are not their concern.<sup>10</sup>

#### (2) Internal Organization of Detective Units

While there has been a little study of the effects on apprehensions and clearances of such things as generalist versus specialist detectives and whether investigators are assigned individually or in teams, there has been no analysis of impact on conviction rates.

The most thorough study of detective operations available is the Rand Corporation study by Peter Greenwood and Joan Petersilia.<sup>11</sup> Using comparisons among cities this study found that cities which used specialists had arrest and clearance rates that were similar to those which used generalists. Similarly, the study found that whether detectives were assigned to cases individually or in two-person teams made no systematic difference in performance.<sup>12</sup>

Neither this study nor the few others which have examined questions of this kind, however, examined the effects on

convictions.<sup>13</sup> As convictions are correlated to some degree with apprehensions and clearances, <sup>14</sup> the Rand study would suggest that these factors do not affect conviction rates either.

### (3) Investigative Resources

One of the more obvious ways that convictions might be affected by organizational structures is through the internal allocation of resources devoted to follow-up investigations. If detectives devoted a major part of their time to bringing about convictions in cases in which apprehensions were made or if it were clear that follow-up investigations by detectives increased convictions, the allocation of additional resources to detective bureaus could be expected to increase conviction rates. There are virtually no studies, however, which examine these questions directly.

There are huge variations in the investigative resources and manpower available in different police agencies. The variation runs from 2 to 18 percent of all police manpower.<sup>15</sup> While there is no study which attempts to determine whether these huge differences make any difference in the proportion of arrestees who are convicted, the Rand study indicates that these huge differences do not make any difference in apprehension or clearance rates.<sup>16</sup>

The study does not discuss the effects of investigative resources on convictions directly. It indicates that detectives already spend more time processing cases in which an arrest is made than in solving cases.<sup>17</sup> It also indicates that there

appears to be a relationship between the thoroughness of investigation and the conviction rate. Comparing a small number of robbery cases in two jurisdictions, it finds the conviction rate considerably higher in the department with the more thorough investigative process. The study indicates that the thoroughness of investigation is related to the stringency of the prosecutorial screening but does not discuss whether there was also a relationship to investigative resources.<sup>18</sup> The study also indicates that most police agencies do not process physical evidence as effectively as they might. 19 Given the limited effect which the study says that investigators have on the solution of cases, the question which the study leaves open is whether these resources could be better spent in trying to nail down the cases in which arrests are quickly made.

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### (4) Post-Arrest Investigations

Once a suspect has been arrested he is normally booked into custody and subjected to certain routine procedures, including fingerprinting, photographing and a record check. Many suspects are in addition interrogated. Other investigative steps may also be taken at this time: interviewing co-suspects, identifying and interviewing other witnesses, following up on leads established at the time of the crime or the arrest.

In many jurisdictions responsibility for these actions falls to the detective bureau until the case has been presented to the prosecutor and to investigators assigned to the prosecutor after the prosecutor has accepted the case. There are great

differences from jurisdiction to jurisdiction. however, in (1) how much investigation of this kind is done before charging, (2) whether the prosecutor's investigative staff picks up the case and when, (3) whether the prosecutor has independent investigative resources, and (4) the quantity of investigative resources available to the prosecutor.

While prosecutors in widely different types of systems all attest to the critical importance of independent investigative resources, there has been virtually no empirical research into the results of such investigative activity, the resources needed, the methods of organization, or the methods of employment. $^{20}$ 

#### (5) Special Police Units

In recent years many police agencies have responded to the crime problem by creating special units to deal with particular problems. Street crime units have been used to ferret out robbery offenders through the use of decoys and other similar tactics. Stores have been set up as covers for fencing operations designed to identify burglars and other thieves for later apprehension.

While most of these special units have been set up to improve police apprehension capabilities, they may have significant effects on conviction rates as well. Often these special operations result in very solid cases because the offender is caught in the act and the principal witnesses are police officers.

When a street crime decoy posing as a skid row drunk is robbed, for example, the offender is generally faced with a victim who is a police officer and with other police officers who both observed the crime and were involved in the

apprehension.<sup>21</sup> Under these circumstances the likelihood of conviction is high. This situation should be contrasted with the more normal situation in which a real skid row drunk is robbed. If the crime is reported and an offender apprehended--both of which are often questionable--there is a strong likelihood that the victim will not show up at the trial. Moreover, even if the victim does show up at trial, he may be a poor witness and in any event is likely to be subject to a considerable amount of discrediting by virtue of his skid row status. The rate of conviction in this kind of case is consequently generally quite low.<sup>22</sup> While it might be possible to increase convictions in this kind of case with better investigative techniques or with strategies which involve taking better care of the victims, it seems likely the results per unit of effort expended might be greater if devoted to decoy type operations. While there has been some evaluation of the impact that special police units have on street crime and on arrests, little has been done to study their impact on convictions.<sup>23</sup> Similarly there has been no serious comparison of these special units and more normal tactics in terms of resources employed. (6) Legal Advisory Units Many police departments in recent years have established legal advisors to assist the agency and individual officers with legal problems.<sup>24</sup> The duties of these advisors vary considerably from jurisdiction to jurisdiction, covering such

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things as advice to patrol units, training bulletins, and development of departmental policy guidelines. In many departments the goals of the units include improving the agencies' capability for handling cases in an increasingly complicated legal system. In a few departments there has been an attempt to implement this goal more specifically by monitoring case performance and using the information obtained as a means for reducing police errors in case processing.

One such unit is the Dallas Police Legal Liaison Division. This unit had as one of its original goals the "reduction of police error in the preparation of legal documents and in adherence to legal procedures." To assist in accomplishing this goal, the unit reviewed each "prosecution report" prepared by the department. 25

As a result of these efforts, the percentage of cases no-billed by the grand jury due to police error is reported to have dropped from 13.8 to 4.3 percent of the cases presented.<sup>26</sup> In addition, the proportion of cases dismissed as a result of police error dropped from 6.4 to 2.6 percent. Both these declines were found to be highly significant statistically (.001 for the no bills and .01 for the dismissals), and the evaluation suggests that the unit may produce as many as 1,000 additional convictions each year.

Evaluation also indicates, however, that the number of cases no-billed and the number of cases dismissed did not decline in proportion to the decline in police errors. Both categories

(.7) Other Police Activities examined.

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Thus while most studies indicate that case outcomes are largely determined by the time they leave police hands, neither police organization nor police incentives pay much attention to convictions. Police operations are overwhelmingly directed toward the goal of apprehending criminals and clearing cases.

did decline slightly--no bills from 23 to 20 percent and dismissals from 19.9 to 19.1 percent, but neither had a very large decline, and it is possible that the overall effects were much less than the declines in police error might suggest.<sup>27</sup> Another unit of this kind is the police review section of the Washington, D.C. Police Legal Advisors Office. 28

Virtually every facet of police organization, staffing, and training has the potential for having impact on conviction rates. Just as special street crime units may increase conviction rates by producing particular kinds of apprehensions, so in theory at least particular patrol strategies might also produce more convictable arrestees. Quicker response times or greater

attention to alarm systems might catch more offenders in the act; team policing might produce better information exchange among officers, and improved police-community relations might produce more information from the community.

Because convictions have generally not been thought to be a police responsibility, however, the effect of these and other organizational arrangements on convictions has not been

#### (8) Conclusion About Police Organization

This is not to say that the police should cease to be interested in apprehensions and clearances or that they should focus solely on convictions. If any criminal justice system functions have an impact on crime, it seems likely that apprehensions and clearances have an effect which is independent of the impact of convictions. On the other hand if police operations are critical to the question of whether suspects are convicted or not, it seems desirable for this consideration to be taken into account at the police level as well as the prosecutorial.

# B. Prosecutorial Organization

While the police have generally paid little attention to conviction rates, prosecutors have often been preoccupied with them. The knowledge available about the impact of various forms of organization on convictions, however, is no greater. Aside from issues of workload which have already been

discussed, there are many possible ways in which prosecutorial organization might affect conviction rates. Among these are:

--Office policy toward case screening

--Whether the office is organized so that a particular case is handled by a single attorney (vertical) or whether cases are passed from one subunit to another as they proceed from filing toward trial (horizontal). --Whether there is a special unit for handling the more serious offenders or the more serious cases. --Levels of training and experience in the office.

One of the more obvious ways in which prosecutorial organ-(1) Screening ization may affect conviction rates is through its policy toward

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a reasonable doubt.

brought to it by the police. Jacoby describes cies in her work on prosecutorial policy making:<sup>29</sup>

Transfer.....No screening

- Unit.....Individual decision making by assistant prosecutors
- Legal sufficiency..Charges issued if elements of crime present
- System efficiency.. Charges issued if elements of crime present and no obvious defects; emphasis on early dispositions and moving cases
- Trial sufficiency..Charged only if conviction at trial very likely
  - rehabilitation... Prosecution only if rehabilitation or treatment is not suitable for the defendant

The policy with the most obvious effect on convictions is the trial sufficiency policy. Under this policy the prosecutor files in court only those cases which he believes will result in a conviction at trial. Presumably this means that the prosecutor files only those cases in which the defendant is guilty beyond

Jurisdictions which follow this kind of policy eliminate virtually all the weaker cases at the screening stage. They are likely to have conviction rates which are very high if measured in terms of cases filed in court but rates considerably lower if measured in terms of arrests.

Jurisdictions which follow the system efficiency policy will file cases on which there is clear probable cause to believe that the suspect is the offender but do not require that there

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must be enough evidence at the time of filing to convict the suspect. Consequently these jurisdictions eliminate fewer cases at the screening stage. These jurisdictions can be expected to have conviction rates which are lower than the trial sufficiency jurisdictions if measured in terms of cases filed in court but higher if measured in terms of arrests.

Jurisdictions which allow individual discretion in screening are likely to be much more erratic than the jurisdictions with a consistent policy. It is not possible therefore to predict how this discretion will be exercised in the absence of specific information about the jurisdiction.

While it is clear that different prosecutorial agencies follow different screening policies, and it is possible to guess at the results of these various policies, there has been no detailed, systematic study as to the impact that these policies have on conviction rates. In particular there has been no examination of the effect of the various policies on conviction rates computed on the basis of arrests. Even in the hypothetical analyses which have been undertaken to try to describe the characteristics of these different kinds of policies and their likely effects, there has been virtually no discussion of their impact on conviction rates.<sup>30</sup>

There has been a similar lack of study of the efficiency effects of screening. It is widely believed, for example, that some form of screening is more efficient than no screening. If true, offices which employ appropriate forms of screening should have greater resources to employ in other ways than

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those which do not. At least theoretically, these offices should be able to translate these resources into higher conviction rates (if this is how they choose to employ the resources).

### (2) Horizontal Versus Vertical Processing

One major organizational issue which is currently of concern to many prosecutors is whether cases should be handled from charging through trial by a single prosecutor or whether it is better to have one team of attorneys handling all the cases at charging, another team handling all the preliminary hearings and a third, fourth or fifth team handling the cases at trial and other case stages. The first of these methods is called vertical processing and the second horizontal.

Thomas Dewey is often given credit for introducing the horizontal form of organization when he was the Manhattan District Attorney in the 1940's.<sup>31</sup> This form of organization was thought to be more efficient than individual case handling because it required less movement by attorneys from court to court and because the more senior attorneys could focus their attention on trials and the more serious cases. This form of organization was felt to be particularly appropriate for offices serving large metropolitan areas and by the early 1970's had become something of the norm for these jurisdictions.

More recently, however, there has been renewed interest in individual case handling. Prosecutors favoring this system believe that the advantages of horizontal processing are outweighed by the loss of individual responsibility which the system entails.<sup>32</sup> They feel that decisions throughout the process are

likely to be better handled by one prosecutor who is held accountable for the case. They feel that if the prosecutor who makes the charge knows that he will have to try the case that he will be more careful in charging, will prepare the case better and will fare better in his contacts with victims and witnesses. Some of this renewed interest in individual case handling arises out of the experience of the career criminal prosecution units discussed below.

While the belief in vertical prosecution has recently led a number of outstanding prosecutor's offices such as those in Los Angeles and Milwaukee to shift from horizontal to vertical prosecution, there has been virtually no study of the impact that these systems have on conviction rates.<sup>33</sup>

(3) Incentives

We do not know much about prosecutorial attitudes toward conviction rates. It is clear that these rates sometimes become involved in political campaigns and that some prosecutors are concerned about them for that reason. Other prosecutorial executives are interested in these figures for reasons of internal office management, while many individual prosecutors are concerned about their own personal performance.

In many jurisdictions, however, conviction rates are so low that it seems doubtful that there is any real prosecutorial interest in them. This situation is perhaps most characteristic of those jurisdictions in which case filing is handled by the police and there are large numbers of dismissals or nol prosses. attitude well:

He then contrasts this attitude with the police attitude previously described concerning convictions. His conclusion

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is that:

It is possible of course that prosecutors in these jurisdictions are interested in conviction rates, but make their comparisons with other jurisdictions operating in much the same way. It

is so easy to manipulate conviction rates as they are customarily counted, however, that it seems more likely that conviction rates are simply not that important in these jurisdictions. Often the concern is more for moving cases, for securing heavy sentences in the more serious cases, and for dealing successfully with the more visible local crime problems than with such abstractions as conviction rates.

Even in those jurisdictions for which conviction rates are important the interest is almost always in convictions in cases filed by the prosecutor rather than in convictions in cases in which there is an arrest. McIntyre summarizes this

One of the most pressing problems of prosecutors is to maintain an even and steady flow of cases through their offices and the courts. This pressure results not only from a defendant's right to a speedy trial, but also from judges who have an interest in keeping their dockets free and smoothly flowing. An excessive number of pending cases presents one of the most sensitive problems in the adjudication process as it affects the prosecutor's office. Hence the prosecutor's most immediate and practical goal is to win as many convictions as possible within the personnel limits of his office and the time-consuming procedures necessary in the courts. Evidence that these concerns are of paramount interest to the prosecutor can be seen in the emphases given in his report. Typically these reports are limited to the number of cases prosecuted, the classes of crimes, the number of convictions obtained, and the cases pending at the end of the reporting period.

[t]he measures of efficiency and worth of police and prosecutors' offices are quite different, both as to the quality and quantity of case handling by their agencies.

#### (4) Career Criminal Prosecution

In 1969 Wolfgang, Figlio and Sellin published the first results of their study of delinquency in a Philadelphia birth cohort, indicating that 15-20 percent of the serious crimes were committed by less than five percent of the offenders.<sup>35</sup> Shortly thereafter the U.S. Attorney's Office in Washington, D.C., developed a career criminal prosecution program aimed at providing increased prosecutorial attention to those criminals creating disproportionate amounts of crime. This program and others developed around the country with the assistance of LEAA funds seek to increase conviction rates and conviction levels, increase incarceration rates and the length of terms, increase bail amounts and pretrial detention and speed up the disposition of cases.<sup>36</sup>

To bring about these goals the career criminal prosecution programs have generally set up special units to handle the career criminal cases. Typically these units have implemented the program by providing for:

continuous case handling by a single attorney or team of attorneys, reduced caseloads, increased investigative support, more stringent pleabargaining policies, efforts<sub>37</sub> to increase incarceration and to reduce processing time.<sup>37</sup>

While the career criminal programs which have come into being have largely common purposes and many common features, they differ considerably in the kind and proportion of cases targeted. Some programs focus rather narrowly on robbers and burglars who are either currently very active or who have particularly bad records. Others focus on a broader group of offenses and encompass a larger part of the total number of office cases. San Diego is typical of the carefully targeted group, focusing primarily on the more serious robbery defendants and taking in less than 2 percent of the office's cases. New Orleans on the other hand handles both felony and misdemeanor defendants who have two prior felony convictions or five prior arrests. The unit caseload makes up ll percent of the office workload.<sup>38</sup> The results of the career criminal programs can be evaluated in a number of different ways. Some have criticized the programs to date on the grounds that the criminals targeted for special prosecution tend to be older criminals who are well past their peak in the number of crimes committed and have suggested that the program is in effect locking the barn door after the horse has been stolen.<sup>39</sup> Other evaluations have found some, although limited, success in increasing sentences or pretrial detention rates.

Whatever their other accomplishments, however, the empirical evaluations to date show little effect on conviction rates. The Mitre Corporation evaluation of four sites by Chelimsky and Dahmann involving quite different kinds of programs showed no effect.<sup>40</sup> A regression analysis by Rhodes using Los Angeles cases likewise showed no effect.<sup>41</sup> An interim evaluation of 12 California programs by the California Office of Criminal Justice Planning did find an effect but it was quite limited.

This study showed that the conviction rates for the kinds of cases targeted had increased from 89 to 93 percent as a result of the programs. 42

Chelimsky and Dahmann suggest that the meager results to date insofar as conviction rates are concerned may be due to the fact that these programs largely focus on improving case prosecution once an arrest has been made and a decision to pursue the case has been reached. They are to a large extent intensifications of effort or organization rather than a radical departure from past methods of prosecution.43

Ultimately the results to date lead Chelimsky and Dahmann to question the extent to which increased prosecutorial attention and resources can affect outcomes in these cases:

It is unclear to what extent these specific programs and the limited system performance results associated with them represent a real stic approximation of the kind of impact other prosecutorial efforts might have on alternative target populations in these sites. Whether more effort, a different configuration of project activities, or a different target population would lead to different results cannot be determined from this research. It is clear, however, that the belief that simply providing the prosecution with added resources will produce direct effects on criminal justice system performance measures does not fully consider the complexities of that system and the limited role that the prosecution plays in its operations.

Rhodes' analysis also raises questions concerning the extent to which increased resources can affect outcomes. Aside from his general findings of no effect, he found that insofar as individual cases were concerned the likelihood of conviction actually went down as the amount of time spent on the case increased. He ultimately concludes that there is an explanation for this, but his findings still raise warning flags about the efficacy of additional resources. This explanation has

three parts:

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First, the most difficult cases probably required the most expenditure; these were also most likely to be difficult cases in which to obtain a conviction. Second, trials are the most expensive means of disposing of a case and unlike guilty pleas, which are relatively inexpensive, they sometimes result in acquittal. As a result, it is not unreasonable to expect a negative correlation between expenditure and conviction. Third, a strong defense may have caused the prosecutor to increase his expenditure on any given case, with the effect of holding the probability of conviction constant, rather than increasing it.

Rhodes' final conclusion is that:

Given the data constraints of this study, it is difficult to judge the effectiveness of expenditures on the probability of conviction of career criminal cases.

(5) Victim-Witness Assistance

While crime victims and witnesses have often been neglected or abused by the criminal justice system, within recent years there have been a number of efforts to better their situations by improving the likelihood of compensation, providing services and minimizing the more onerous features of the criminal justice system. Child abuse clinics, rape crisis centers, battered women's shelters and victim-witness assistance units are all outgrowths to some degree of these efforts. In addition to their other functions these organizations all have some relevance to the attrition problem because of the unwillingness on the part of many victims and witnesses to cooperate in the prosecution of cases.

One of the earliest estimates of the impact of victim-witness noncooperation was that made by the National Advisory Commission

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on Criminal Justice Standards and Goals. Finding that non-attendance was responsible for 60 percent of all dismissals in the New York City Criminal Court, the Task Force described this factor as "the most prevalent reason [throughout the country] for dismissal of cases for want of prosecution and a significant contributor to overall dismissal rates."46 A 1972 Washington D.C. study using PROMIS data found that nearly a quarter of all felony arrests were ultimately rejected or dismissed because victims and witnesses were uncooperative. 47 A later and more extensive D.C. study found that witness noncooperation accounted for 38 percent of all nonconviction cases. 48 The Vera study in New York indicated that an even greater proportion of attrition cases was attributable to victim noncooperation. "Deep sample" cases showed that 86 percent of rape dismissals, 83 percent of the assault dismissals, 69 percent of robbery dismissals, 82 percent of the burglary dismissals, and 36 percent of the grand larceny dismissals were attributable to complainant noncooperation.49

Taken together these and other studies indicate several different kinds of victim-witness problems:

(1) Unable to locate--lists of victims and witnesses generally come in the first instance from the police officer who takes the report of the crime or who makes the arrest. Often there are victims or witnesses who are present at the time the report is taken or the arrest made who cannot be located later. Sometimes the victim or witness gave a phony name or address. Sometimes the police officer recorded the name or address erroneously. Another problem may be the transiency of the victim or witness, particularly if some time period is involved.

In addition to persons present at the time the report is taken lists of other victims and witnesses may include L

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names supplied by persons who were present or be names acquired in other ways. Often the names and addresses of these persons are not known fully or accurately and cannot be obtained from other sources. In some instances the persons may not have in fact been victims or witnesses or may not even exist. A final problem may simply be inadequate efforts to locate the victim or witness on the part of the authorities.

(2) No show--This category includes those victims and witnesses who have been subpoenaed or otherwise requested to appear in court who do not do so.

(3) Unwilling to cooperate -- This category includes those victims and witnesses who are unwilling to participate in the prosecution of the case. Often this category includes both those victims and witnesses who make definite statements of their unwillingness to participate and those who are assumed by the authorities to be unwilling to participate either on the basis of their demeanor and general attitude or on the basis of their relationship with the defendant.

The differences among jurisdictions in the degree of these problems appear to be enormous. These differences are difficult to measure both for the reasons indicated in chapter 3 of the Final Report and because of differences in the terminology and methods used in different studies. Existing studies nonetheless give some hint of the variations. In the Brosi cross-city comparison of attrition rates, for example, the amount of attrition attributable to witness-related problems varies from seven to 20 percent as a percent of all felony cases, as shown in Table EE-1. And if witness-related attrition is measured only as a percentage of the attrition cases, the variation is even greater--from 9 to 42 percent. The variation among jurisdictions as to the kinds of witness-related problems appears to be even greater. Brosi. for example, indicates that 62 percent of all witness-related

#### Table EE-1

## Attrition Due to Witness Related Problems

	At Screening	Post- Screening	Total Attrition Due to Witness Related Problems	Total Witness Related Attrition as Percent of All Attrition
Cobb County	13	-	13	42
District of Columbi	la 6	8	14	20
Los Angeles	4	3	7	29
New Crleans	19	1	20	9
Source: Brosi, A C	ross-City Comm		20	31

City Comparison of Felony Case Processing (1979), pp. 14, 16, 20.

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To help in coping with the general problem of victim-witness As indicated by Davis, many of these projects were located

cooperation LEAA began in 1974 to fund victim-witness assistance projects, funding 19 such projects that year as part of its Citizens' Initiative Program and ultimately more than 90 projects. within or worked closely with prosecutors' offices:

Many programs set explicit goals of reducing victim-witness disaffection and non-cooperation, and designed their program efforts with the current research findings on causes of victim-witness non-cooperation in mind. Thus, one set of program activities were designed to reduce the "costs" to victim-witnesses of having to appear in court: projects created reception centers to provide comfortable and secure places for victim-witnesses to wait while in court, and aided clients in collecting witness fees from prosecutors. A number of projects began or expanded the use of stand-by telephone "alerts" to keep victim-witnesses from having to appear in court except when it was determined on the day that a scheduled hearing was to take place that the victim-witness was actually needed.

Another set of activities, common to many of the victim--witness programs that were allied with prosecutors' offices aimed to provide clients with a clearer understanding of their obligations in their role as prosecution witnesses and of the court process. These activities included

post-filing nolles and dismissals, is attributable to no-shows while the percentage in Indianapolis is only 2 percent, as shown in Figure EE-1. Fifty percent of the Indianapolis witness-related nolles and dismissals on the other hand are shown as "unable to locate unavailable" while there are no such cases in New Orleans.

Other studies seem to confirm this kind of variation. In Brooklyn victims and witnesses appear in court when requested by prosecutors only 34 percent of the time, while in Milwaukee the appearance rate is over 60 percent.<sup>50</sup>



Source: Brosi, p. 21.

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distributing witness brochures; explaining court procedures, notifying witnesses of upcoming court dates, and (in some projects) informing persons of the dispositions reached in their cases.

Evaluations to date indicate that these projects have been useful in reducing the burdens that the criminal justice system sometimes imposes on witnesses, and it is possible that these effects may ultimately translate into greater citizen willingness to assist in the criminal justice process in the future. The effects on citizen willingness to participate in the cases which brought them into contact with the system in the first place, however, have been limited.

The largest of the projects was the Victim/Witness Assistance Project established by the Vera Institute of Justice in the Brooklyn Criminal Court in 1975. This project offered a wide range of services including a crime victim hotline, emergency repair services for burglary victims, transportation to court, a secure waiting area, child care services at the court and efforts to limit the number of required appearances.

Project evaluations indicate that victims and witnesses thought the services offered were highly useful and that they met many of the needs felt by victims and witnesses. The evaluations also indicate that the program was successful in reducing the number of court appearances required of victims and witnesses. These successes were not translated, however, into increased rates of appearance by victims and witnesses, and as a consequence the project was found to have had no effect on the rate of dismissals for failure to prosecute.<sup>52</sup>

Similar results were reported from the first-year evaluations of a sizeable Los Angeles project and a major project in Milwaukee.<sup>53</sup> The final report of the Milwaukee project, however, indicated a 6 percent decrease in the number of felony dismissals because of witness problems.<sup>54</sup> This project was able to decrease the number of witnesses indicating unnecessary trips by 50 percent and the number of unnecessary trips by 28 percent. The project was also able to locate nearly 90 percent of the witnesses needed in court whose subpoenas were returned by the Sheriff's Office. An experiment carried out during the project indicated that victims and witnesses notified by telephone had a considerably better appearance rate than did those subpoenaed in the regular way--75 as compared to 63 percent.

Unit activities also appear to have had a favorable impact on prosecutions in sensitive cases and in cases in which victims or witnesses required special protection because they were threatened or intimidated.

Much of the problem of victim-witness cooperation is tied up with victim-offender relationships. Many crimes involve family members, lovers or friends as both offenders and victim-witnesses. In many instances after an initial period of anger the victim or witness decides that he or she prefers to maintain the relationship rather than prosecute the culprit. In one study in Brooklyn over half of the cases dismissed due to uncooperative witnesses involved prior relationships.<sup>55</sup> In jurisdictions with strong screening policies these cases

are generally screened out prior to charge. In other jurisdictions the relationship may not become apparent until the victim or witness is summoned to court. While victim-witness assistance units generally have not This lack of effort on the part of victim-witness assistance It is conceivable that other kinds of victim assistance Overall victim-witness assistance units and programs,

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ignored the cases involving relationships with offenders and have generally provided services to these victims and witnesses, they generally have not devoted major attention to persuading these victims and witnesses to proceed with the case. units is in line with the general willingness of police, prosecutors and courts to treat all but the most serious of these cases in the way in which the victim prefers.<sup>56</sup> This general acquiesence has been ascribed to the realization that many victims of this kind helped to precipitate or cause the event and are not wholly innocent.<sup>57</sup> It also reflects an understanding that the victim will have to live with the results of the choice. programs such as rape crisis centers, battered women's shelters and the like have had some impact on the willingness of victims and witnesses to go forward with prosecutions.<sup>58</sup> This is not the purpose of most of these organizations, however, and thus far there has been virtually no research concerning these questions. while undoubtedly important in some cases, appear to have had a very limited effect on appearance and conviction rates.<sup>59</sup>

#### C. Conclusions

It seems axiomatic that good organization should result in better performance. There is very little evidence available, however, about the effects of organization upon felony convictions and that which is available mostly indicates what does not work.

While there are many reasons to believe that police performance is the key to a greater number of convictions, the police are organized more to make apprehensions and to maintain order than to produce convictions. Officers understand what is necessary for an arrest but often are quite unclear about what is necessary for a conviction. The police priorities are not necessarily wrong. Apprehensions and order maintenance are important functions. If more convictions is an important goal, however, the police priorities may need to be altered.

Prosecutors on the other hand are organized to produce convictions and have in recent years tried a number of innovative measures designed to produce more convictions, including career criminal and victim-witness programs. The research to date indicates that these new programs have not had any major effect on convictions. These findings may be too pessimistic because the research in this area has been difficult and the findings are far from conclusive. What seems likely, however, is that the limited effects are due largely to the fact that in many offices the weaker cases are screened out and never filed, and that the cases which survive this process do not require a great deal of extra attention.

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#### Appendix EE--Notes

1. The classic works are L. Fuld, Police Administration: A Critical Study of Police Organizations in the United States and Abroad (1909); R. Fosdick, (1915); A. Vollmer, The Police and Modern Society (1936); B. Smith, Police Systems in the United States (1940); O. Wilson, Police Administration (1950). Some more recent works are O. Wilson and R. McLaren, Police Administration (1977); President's Commission on Law Enforcement and Administration of Justice, Task Force: The Police (1967); National Advisory Commission on Criminal Justice Standards and Goals, The Police (1972). J. Munro, Administrative Behavior and Police Organization

2. This is not necessarily a criticism, and there are some exceptions in any event. See, e.g., V. Cizanckas and D. Hanna, Modern Police Management and Organization (1977); H. Goldstein, Policing a Free Society (1977). See also Shanahan, Hunger and Wheelen, Organizational Profile of Police Agencies in the United States, 7 J.P.S. & Adm.

3. This problem is not peculiar to the criminal justice field.

4. This was true in several departments observed in this

- 5. J. Wilson, Varieties of Police Behavior 291 (1968).
- 6. J. Wilson and B. Boland, "Crime" in <u>The Urban Predicament</u> (1976)(Gorham and Glazer, eds.).
- 7. S. Wildhorn, M. Lavin, A. Pascal, S. Berry and S. Klein, Indicators of Justice: Measuring the Performance of Prosecution, Defense, and Court Agencies Involved in Felon Proceedings (May 1977)(National Institute of Law Enforcement and Criminal Justice; O'Neill, Needle, and Galvin, <u>Appraising the Performance of Police Agencies: The PPPm</u> <u>System</u>, 8 J. Police Science & Admin. 253 (1980); Needle, PPPm: A System for Measuring Police Effectiveness and Productivity (Nov. 1978)(American Justice Institute).
- 8. O'Neill, Needle, and Galvin, supra note 7, at 10-12.
- 9. McIntyre, Impediments to Effective Police-Prosecutor Relationships, 13 Am. Crim. L. Rev. 200, 226 (1975).

10. Id. at 226-27.

11. P. Greenwood, J. Chaiken, J. Petersilia, and L. Prusoff, The Criminal Investigation Process 58-59 (vols. 1-3)(Oct. 1975) (National Institute of Law Enforcement and Criminal Justice).  $\widehat{\nabla}$ 

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13. See, e.g., R.M. Ward, <u>The Investigative Function: Criminal</u> <u>Investigation in the United States</u> (unpublished thesis, Univ. of California, Berkeley)(1971); C.F. Peterson, <u>Admin-istration of the Investigative Function--Specialization</u> <u>Within the Detective Unit</u> (unpublished master's thesis, Michigan State University, 1968).

14. National Academy of Sciences, <u>Deterrence and Incapacitation</u>: <u>Estimating the Effects of Criminal Sanctions on Crime</u> Rates 19-63 (1978)(Blumstein, Cohen and Nagin, eds.).

15. J. Chaiken, Survey of Municipal and County Police Departments at 18, vol. II in the Criminal Investigation Process (Oct. 1975), indicates that on the average 14.5 percent of sworn employees in medium and large-sized departments were detectives. Three departments reported 6 percent and one 31 percent. The 1970 Survey of Municipal Police Departments by the Kansas City, Missouri Department showed variations from 2 percent in Oaklahoma City to 18 percent in Cincinnati.

16. P. Greenwood, <u>et.al.</u>, supra note 11. See also Gates and Knowles, <u>An Evaluation of the Rand Corporation's Analysis</u> of the Criminal Investigation Process, Police Chief, July 1976. at 20.

18. Id. at 4, 124-33.

19. Id.

20. One useful study is Glick and Riccio, Productivity of Detectives: A Study of the Investigative Function of Police Juvenile Units, 2 J.P.S. & Adm. 138 (1979).

- 21. But see Goldstein, Improving Policing: A Problem-Oriented Approach, 25 Crime and Delinquency 236, 237 (1979).
- 22. Rates were not calculated separately but many cases of this kind were analyzed at both field sites.
- 23. K. Webb, B. Sowder, A. Andrews, M. Burt and E. Davis, Specialized Patrol Projects (Jan. 1977) (National Institute of Law Enforcement and Criminal Justice). See also A. Halper and R. Ku, New York City Police Department Street Crime Unit: An Exemplary Project (1976)(National Institute of Law Enforcement and Criminal Justice).
- 24. This was partially in response to the recommendation of the President's Commission on Law Enforcement and Administration of Justice, The Challenge of Crime in a Free Society (1967).

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26. Id. at 87-91.

27. Id. at 91-92.

28. See, e.g., Forst, Lucianovic and S. Cox, What Happens After Arrest? at 77 (Aug. 1977)(Institute for Law and Social Research); Alprin, D.C.'s Case Review Section Studies The 'No Paper' Phenomenon, Police Chief, April 1973, at

36.

30. Id.

32. This point was uniformly made by a number of prosecutorial offices contacted in this study.

25. H. Wise, The Dallas Police Legal Liaison Division: An Exemplary Project (March 1976)(National Institute for Law Enforcement and Administration of Justice).

29. J. Jacoby, The Prosecutor's Charging Decision: A Police Perspective (Jan. 1977) (National Institute of Law Enforcement and Administration of Justice).

31. This kind of organization may be related to the kind of court organization in use.

33. The exceptions to this are the evaluations of the career criminal programs.

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- 34. McIntyre, Impediments to Effective Police-Prosecutor Relationships, 13 Am. Crim. L. Rev. 200, 227 (1975). See also Abrams, Internal Policy: Guiding the Exercise of Prosecutorial Discretion, 19 U.C.L.A. Rev. 1 (1971); P. Greenwood, S. Wildhorn, E. Poggio, M. Strumwasser and P. DeLeon, Prosecution of Adult Felony Defendants (1976).
- 35. M. Wolfgang, R. Figlio, and T. Sellin, Delinquency in a Birth Cohort (1972). See also Wolfgang, Crime in a Birth Cohort, 117 Proceedings of the American Philosophical Society 404 (1973).
- 36. See Curbing the Repeat Offender: A Strategy for Prosecutors (Sept. 1977) (National Institute of Law Enforcement and Criminal Justice) (PROMIS Research Project Publication No. 3); P. Greenwood, Career Criminal Prosecution: Potential Objectives, 71 J. Crim. L. & Criminology 85 (1980).
- 37. Chelimsky and Dahmann, The Mitre Corporation's National Evaluation of the Career Criminal Program: A Discussion of the Findings, 71 J. Crim. L. & Criminology 102, 104 (1980).
- 38. J. Dahmann and J. Lacy, Criminal Prosecution in Four Jurisdictions: Departures From Routing Processing in the Career Criminal Program 116, 127 (June 1977) (Mitre Corp. Technical

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41. Rhodes, Investment of Prosecution Resources in Career Criminal Cases, 71 J. Crim. L. & Criminology 118 (1980).

42. California Office of Criminal Justice Planning, California Career Criminal Prosecution: Preliminary Report to the Legislature 48 (Jan. 1979);

44. Id. at 105.

45. Id. at 122.

and Goals, Courts (1973).

Report 7550); California Office of Criminal Justice Planning, California Career Criminal Prosecution: Preliminary Report to the Legislature 48 (Jan. 1979).

39. See, e.g., P. Greenwood, Career Criminal Prosecution: Potential Objectives, 71 J. Crim. L. & Criminology 85

40. Chelimsky and Dahmann, supra note 37, at 102, 104.

43. Chelimsky and Dahmann, supra note 37, at 102, 104.

46. National Advisory Commission on Criminal Justice Standards

47. F. Cannavale and W. Falcon, Witness Cooperation 21 (1976).

48. Id at 28.

- 49. Vera Institute of Justice, Felony Arrests: Their Prosecution and Disposition in New York City's Courts (1977).
- 50. R. Davis, Victim/Witness Non-Cooperation: A Second Look at a Persistent Problem (1980)(unpublished paper).
- 51. Id. at 6-7. See also Newton, Aid to the Victim: Part 2: Victim Aid Programs, 8 Crime & Delinquency Literature 508 (1976); Bickman, Research and Evaluation: Cook County State's Attorney Victim Witness Assistance Project, 1 Victimology 160 (1976); National District Attorney's Association, Help for Victims and Witnesses: An Annual Report (1976).
- 52. Id. at 8-9. See also Vera Institute of Justice, Further Work in Criminal Justice Reform (1977); Vera Institute of Justice, Impact Evaluation of the Victim/Witness Assistance Project's Appearance Management Activities (May 1976).

53. Id. at 9.

54. Evaluation Policy Research Associates, Ltd., Milwaukee County Project Turnaround, Evaluation Final Report, Executive Summary, at 5 (Jan. 1979).

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57. Id.

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59. R. Rosenblum and C. Blew, Victim/Witness Assistance (July 1979) (National Institute of Law Enforcement and Criminal Justice). Victim compensation appears to have an equally limited effect. See, e.g., Doerner and Lab, The Impact of Crime Compensation Upon Victim Attitudes Toward the Criminal Justice System, 5 Victimology 61 (1980).

55. R. Davis, supra note 50.

56. R. Davis and B. Smith, Criminal Offenses Between Acquaintances: The Response of Criminal Courts (unpublished paper,

58. G. Bryant and P. Cirel, A Community Response to Rape: An Exemplary Project 83-86, 91-92 (March 1977); Forcible Rape: A National Survey of the Response By Prosecutors 21-25 (March 1977)(National Institute of Law Enforcement and Criminal Justice.

