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DELINQUENT GENERATIONS REVISITED

THE STATISTICAL ANALYSES APPROPRIATE FOR TESTING COHORT HYPOTHESES OF THIS TYPE

1. Introduction.

Criminologists have by now had a surfeit of articles on this theme. Yet a continuing debate about appropriate methods of analysis is important if criminology is to evolve as a scientific discipline. Moreover the substantive point at issue is important: are some birth year cohorts more delinquent than others, or can all the variations in crime or delinquency rates be attributed to the age of the population under study and the year of study?

Stott (1962), for example, used the Delinquent Generations Hypothesis (D.G.H.) (with some modifications) to examine the "hypothesis that the greater deliquency proneness of males born then was due to their having suffered congenital impairment of temperament" (Stott, p. 783). Without Wilkins' formulation there would have been no excess delinquency proneness to examine!

On the other hand in two multivariate analyses of criminal statistics in England and Wales since the war, the numbers or the percentage of the population in a given age-group have been used as predictor variables, even though different birth-year cohorts are involved at each observational point. Their assumption that a constant coefficient is appropriate over time to capture the effect of a given age-group denies the D.G.H. Thus Ahamad (1967), investigating the trend in total indictable offences from 1950 to 1963, used an equation of the form:

$$F_{1j}{}^{***} = \frac{1}{1000}(-8280 + 0.00390 \ X_{6j})$$

where $F_1{}^{***}$ accounts for 97.8% of the variance of the first principal component in a simultaneous analysis of 18 different offence groups and X_6 are the numbers in the age-group 13-19.

Willmer, (1968) investigating the trend in breaking and entering offences from 1952 to 1967, used an equation of the form:

$$y = -496.9 + 0.299x_1 + 0.211x_2$$

where $x_1(t)$ = Nos. males in the home population aged 18-22

and $x_2(t)$ = Nos. males in the home population aged 13-15.

We shall consider the specific hypothesis that Wilkins (1960) tested, i.e. "children born in certain years (for example during wartime) are more likely to commit offences than others, and that this tendency remains from childhood to adult life".

From an examination of the data on recorded delinquents for England and Wales for 8-20 years-olds from 1946-57, he concluded that children who passed throught their fourth and fifth years during wartime were more likely to be delinquent. Since his original article, similar hypotheses have been tested and apparently supported in Denmark by Christiansen (1964), in New Zealand by Slater, Darwin and Ritchie (1965), an in Poland by Jasinsky (1966). The conclusions, especially for England and Wales, have been disputed, partly on methodological grounds, by Prys Williams (1962), Rose (1968), Stott (1962) and Walters (1963). Other articles dealing with the question of age specific delinquency are those by Møglestue (1965) and Willmer (1968).

First of all we should distinguish between identified delinquency, i.e. the detected authors of recorded delinquencies, and delinquency, i.e. the commission of offences. We shall return to this distinction later (see pp. 343 ff). For the moment we shall be considering delinquent rates, i.e. the proportion of a group who are identified as delinquent, and not the delinquency rate, i.e. the (unknown) proportion of a group who commit offences [1].

In this case, therefore, we want to test hypotheses about the numbers of delinquents in a particular birth year group. Authors have normally used data about age-specific delinquent rates in different years; but we cannot immediately infer the number of identified delinquents in a particular birth year cohort, from the delinquent rates observed

[1] Throughout this paper, except in quotations, delinquency will refer to the commission of offences and not to the identified authors of recorded offences. This leads to some clumsy locutions but preserves conceptual clarity. It should be emphasised that the delinquency rate is unknowable in two ways: firstly not all recorded offences are traced to a perpetrator, and secondly there is the "dark number" of unreported incidents which, if they had been reported, would have been recorded.

in the succeeding years. For recorded delinquent rates vary from year to year and are different for different ages. How, therefore, do we legitimately infer the delinquent rate for a cohort? The most thorough critique ends with: "It is beyond the bounds of this paper to make proposals for an alternative and valid method of distinguishing between the delinquency of generations" (Rose, 1970).

We therefore think it is legitimate to explore the possibilities of the various methods of statistical analyses which are available and examine the inferences which may be drawn through their use.

The Problem.

Wilkins' original article considered the following type of matrix of observations which has been the basis of all subsequent argument about recorded delinquents.

	Age	Young	Old
Year		a_1	a_s
Early	y_1		
Late	yr		

FIG. 1. THE PROBLEM.

Wilkins was thus concerned to analyse the variations in this $r \times s$ matrix, and specifically he hoped to be able to find if there were any effects due to the birth-year of a particular cohort. Some method is required to partial out the variations in recorded rates, by age and year.

2. Previous Approaches.

The Expectation Method.

Wilkins assumed that we should standardise for an independent effect of age on delinquent *rates* and an independent effect of year on delinquent *rates*. He thus assumed that the expected rate for any age-group (r) in year (s) was given by

$$Ers = \frac{\left(\sum_{r=1}^{R} D_{rs}\right)\left(\sum_{s=1}^{S} D_{rs}\right)}{\sum_{r=1}^{R}\sum_{s=1}^{S} D_{rs}}$$

where r = age-group R = number of age-groups

s = year S = number of years

D_{rs} = Delinquent rates for age-group r in year s, i.e. the ratio of the number of detected authors of recorded delinquencies to the relevant population.

In words, the expected delinquent rate for age group s in year r is

$$\frac{\text{Average rate for year } s \text{ (all age-groups)}}{\text{Average rate for all years (all age-groups)}} \times \text{Average rate for age-group } r \text{ (all years).}$$

He then assumes D_{rs}-E_{rs}, i.e. the differences between the actual and expected delinquent rate, are totally accounted for by a birth year (diagonal or simply interactive) effect. To this extent, his analysis is *determinate*; he does not specify a distribution of possible delinquent rates in age-year cells [2].

Wilkins then looked at the pattern of deviations of the actual observed delinquent rates in each cell from those expected due to the supposed independent effects of age and year. He finds a pattern of deviation approximately as in Fig. 2.

Age	Young	Old
Year		
Early	+	—
Late	—	+

FIG. 2. WILKINS PATTERN.

Thus we find an array of + signs along, or close to, the main diagonal. Since the entries in these cells correspond to the same birth cohorts, he concludes that these birth cohorts had an excessive number of delinquents "that some birth years are associated with excessive criminality is sustained by the current analysis .. Moreover it seems that disturbance of social or family life had the most marked effect on sub-

[2] Wilkins himself says that this method allows for the disturbing influence of age, population, crime patterns and age × population interaction. It is not clear to what extent he has allowed for the effect of population and an age × population interaction on the crime rate.

sequent criminality if they occurred when the children concerned were passing through their fifth year".

Christiansen (1964) carried out a similar analysis on the available data for Denmark from 1952-58 for 15-24 year olds. From a short discussion of the Danish situation during the war years he observed that "it is hardly correct to compare conditions in the two countries before 1943/4".

His conclusion which seemed to confirm Wilkins' hypothesis was that "A corresponding rate [refers to a *particularly* marked rise] was found in Denmark ... for the age groups born 1938/40-1942/3, the excess crime rate here being about 30 per cent".

Jasinski (1966) carried out a similar analysis on the available data for Poland from 1951-1962 on 10-20 year olds. He reported two peak delinquent generations — those born between 1936/7 and 1942/3 and those born between 1945/6 and 1951/2. He then split up the data and examined the same type of matrix for 10-16 year olds and 17-20 year olds concluding that "Four and five-year old children received as a legacy from the war the greatest 'delinquency-proneness'. The second peak must be treated as a result of external factors affecting the rates for juveniles and young adults in the individual years of the period 1951 to 1962".

Slater, Darwin and Ritchie (1965), (see below), analysed a similar set of data for New Zealand from 1947-60 on 8-20 year olds. They reported that "Social disturbance associated with the Second World War, 1939-45, had affected certain groups of children born during or just before it in such a way as to render them more likely than others to appear before the Children's Court later in life" (p. 146).

Criticisms of the Expectation Method.

We shall concentrate on the methodological criticism in this paper. This does not mean that we think the other criticisms unimportant but that discussion of them should arise in the more general context of *any* manipulation of criminological data. Two such criticisms that stand out are:

(i) "The method uses as its basic data the number of findings of guilt and not the number of persons found guilty" (Jasinski, p. 17).

(ii) "there were considerable differences in the pattern of the type of delinquency recorded over the sixteen years of the study." (Prys Williams, p. 6).

In assering the correctness of the *method*, various authors have commented on later *results* as invalidating the conclusion. Thus "... if the analysis has predictive value — that is, if the same trends persist beyond the period of direct study — one should expect the incidence of delinquency to rise in higher age-groups in later years and to fall in the same age-group, as the delinquency-prone youths grow older. Such does not appear to have happened." (Prys Williams, p. 1).

In general, however, there have been 3 main strands of methodological criticism:

(*a*) The first is that there are other explanations for the pattern of delinquent rates in the years under consideration. Thus:

(i) Walters (1963) chaimed that "given the evidence on indictable offences in the Home Office Report, the alleged support for the D.G.H. is a statistical illusion due to two more or less independent trends in the data. When these trends are removed any statistical support for the D.G.H. disappears." (Walters, p. 391).

He claimed that "there has been a marked decline in the rates for young males (i.e. 8-11) from the year 1951 or 1952 to 1957" and "that at least part of the decline is due to police not pursuing cases which they would have pursued in earlier years". Similar arguments were advanced by the other authors. Secondly there is "clearly the rise in delinquency rates of young men (age 17-20) in the years 1956-7 ... it seems likely that this is a real trend".

Walters showed that this would lead us to expect a table of the following sort which is similar to that actually observed.

He removed these trends by substituting artificial figures for the age-year cells which are missing, and then found that there was no resultant effect corresponding to the delinquent generations [3].

	Age	Young	Old
Year		a_1	a_s
Early	y_1	High	Low
Late	y_R	Low	High

[a] "High" and "Low" refer to within-column comparison.

FIG. 3. DELINQUENT RATES [a] FOR TWO AGE GROUPS IN TWO TIME PERIODS.

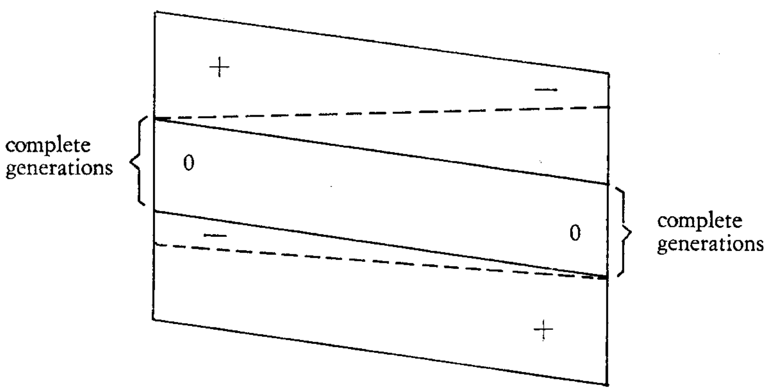
[3] Other critics, notably Walters and Rose, have stressed the importance of cautioning in changing the pattern of early delinquent rates. The *specific* hypothesis used is however, imprecise. As Layzell shows in his paper, Walters' discussion of cautioning would lead us to expect a rise in cautioning as crime rates fall: instead there seems to have been a general rise in cautioning as crime rates rise.

Wilkins replied that Walters had not discussed "whether the assumptions it is necessary for him to make are more or less reasonable than those necessary for my model" and that on these grounds the D.G.H. is still tenable. It seems unlikely that Walters' objection applies to all the cross-country phenomena as well.

(ii) Layzell (1967) suggested that it was unnecessary to postulate two distinct trends as Walters had done, and proposed that the available data "can be explained by one simple trend ... a change in delinquency pattern. It appears that less crime is being committed when younger and more when older" (Layzell, p. 3).

If this trend occurs consistently over all the generations considered, then, "The earlier generations considered will appear, in comparison with the rest, to be committing much more crime in their earlier years and much less in their later years. The middle generations will be considered average. The later generations will appear, in comparison, to be committing less crime in their earlier years and much more in their later years", (Layzell, p. 4).

The result of such a possible delinquent shift would be as follows:



NOTES TO TABLE: The outline of a "normal table" is represented by dotted lines. Sloping lines represent the passage of generation through the table.

FIGURE 4.[4] RESULT OF POSSIBLE DELINQUENCY SHIFT.

Because the analysis can consider only the central rectangular portion of the table we are omitting the age year cells when the non-complete[5]

[4] Reproduced from LAYZELL (1967). This paper helped us greatly with this section, it is undoubtedly the most sophisticated attempt to use the expectation method for this sort of table.

[5] A non-complete generation is one on which we do not have delinquency data throughout the whole age range.

generations are most delinquent. This also gives an appearance of increased delinquency down the main diagonal. Owing to restrictions of space Layzell did not attempt to give sociological evidence for his postulated shift [6], so that he concluded "Wilkins' hypothesis is a little more doubtful and the effect of it a little smaller than is apparent from his articles".

But his general hypothesis of a shift in the crime pattern is a more plausible cross-country phenomenon than Walters'. For we know that the passage from childhood to adulthood has been lengthening in all modern industrial societies, partly because of the complexity of the adult role. So individuals remain youthful adolescents longer.

(*b*) The second criticism is that the method used is inappropriate. Jasinski summed up the weaknesses in the method as follows:

1. "The method does not eliminate all external factors influencing the level of delinquency rates; it eliminates only those factors which affect equally all generations under examination.
2. The results produced by the method are relative in the sense that the differences between expected and observed rates depend to some extent on the length of the examined period and the number of generations" (p. 179/80).

Walters emphasises the first point, "we should not normally use data to give expected values when there are marked and different trends in time series" (p. 395).

In the particular case of the D.G.H., Layzell sums up the problem as follows: "[the] analysis so far discussed will, on application to a set of crime rates containing the change in delinquency patterns mentioned, automatically give an apparent increase in delinquency for the centre generations of those considered." (Layzell, p. 8).

Rose (1970) attempted a systematic criticism of both Wilkins' and Christiansen's data. Of the English data, he claimed that: "Within the post-war period, whathever the time period chosen for an analysis of the form described above, the generations identified as having the highest positive deviations will, in general, be in the middle rung of the set of birth groups which are under consideration".

This obscures the critical point: that it is only *because* he postulatess a certain trend in delinquent rates during the postwar period that this hypothesis is substantiated. This conclusion therefore seems a little

[6] He did observe, however, that this shift coincided with the raising of the school leaving age and we know that the penultimate year at school is the modal age for detected delinquents. But this could not account for all of the effect and is not a universal phenomenon.

premature "... the method is most certainly inappropriate for analysing time series data when identifiable trends in time are present. In statistical terminology the method is not 'robust' in these circumstances: that is to say, it generates completely different results when different samples (i.e. time periods) are analysed." (Rose, p. 11).

We must first of all decide whether or not there have been any identifiable time trends in the data — we cannot assume them *a priori*.

(*c*) And finally that since the method does not distinguish between first offenders and recidivists, there is an unknown correlation between the rates for each cohort in different years [7]. The extreme case would be where for one cohort the same boys kept on committing offences in different years, while for another cohort every boy committed only one offence during the whole period.

This raises a central problem, which we must tackle later; what factors (apart from age and year) affect the recorded delinquent rates and what is their relative importance?

Other Approaches.

1. Layzell, examined Walters' claim that there were two independent trends giving an appearance of certain generations being delinquent. He calculated "the average shape of a crime graph for a generation ... by finding the ratio of each age crime rate to the total crime for a generation ... The average of ratios is then taken (cutting across generations) for each age, to give an average ratio ... [which ratios] are then re-applied to the total crime rate for a generation, to give expected values for the crime rates. The excess of the actual crime rate, over the expected crime rate [for each age-year cell] is calculated as a ratio of the expected crime rate, as in the Wilkins' analysis" (p. 8).

If Walters' two definite trends existed there should be a group of negative deviations at bottom left and a group of positive numbers at top right. For this analysis determines how we expect crime rates to change over individuals' lives (and so discounts the year effects in Wilkins' analysis); if Walters is right we expect the shape of the crime graph to have changed since the war. The actual results are reproduced below (Table 4).

This table does not verify Walters' hypotheses. On the other hand we can see that there are high negative numbers down the transverse diagonal with increasing positive numbers away from the centre along the main diagonal. Layzell took this as a vindication of *his* hypothesis that there had been a shift in delinquents from earlier to later years.

[7] We are indebted to Monica Walker for the emphasis on this point.

TAB. 1. PERCENTAGE VARIATIONS FROM AVERAGE GENERATION CRIME PATTERN IN COMPLETE GENERATIONS.

Born. Year												
37/38	7.6	7.0	28.3	24.2	25.6	34.2	21.0	4.3	−19.8	−29.5	−33.4	−22.0
38/39	4.3	24.5	16.7	26.0	33.9	19.4	− 3.0	−13.4	−20.7	−22.5	−16.0	− 6.4
39/40	18.3	13.8	21.2	24.9	15.8	− 3.5	−13.7	−14.7	−13.8	− 8.3	4.1	− 3.3
40/41	4.5	11.0	14.5	13.4	− 5.8	15.7	−15.2	−10.1	− 1.7	12.6	7.4	5.5
41/42	7.5	11.1	0.2	− 8.8	−17.5	−17.3	− 7.9	4.4	15.2	7.3	4.1	5.1
42/43	4.4	− 7.6	−16.6	−25.3	−20.1	−12.0	1.4	12.6	11.3	11.0	10.3	10.9
43/44	−19.3	−22.1	−30.7	−25.9	−17.1	− 4.8	9.3	11.4	13.0	10.5	15.5	7.1
44/45	−27.3	−37.9	−33.5	−28.5	−15.0	0.3	7.9	14.1	16.3	19.0	13.3	2.9

This approach highlights an important point: to test hypotheses about generations, we should consider data only when it is available for a *complete* generation, since it is not clear how the introduction of "half-finished" generations affects the results. But, as before, his scheme of averaging rates is *ad hoc* and awkward: it is not clear what assumptions are made, so it is not clear precisely what hypotheses are being tested about the residuals.

2. For these latter reasons we were at first attracted by the possibility of using a scheme which defined the expected distribution of data points in each age year cell. In this way we can estimate important parameters with some idea of their significance. Slater, Darwin and Ritchie, after their "replication" of Wilkins' analysis, considered two possible criticisms; that the

(*a*) "apparent trends result from the method of analysis rather than from accident of data".

(*b*) "need is for some estimate of the statistical significance of the results".

As a result they supposed "that the variable r_{ij} has the Poisson distribution with mean $N_{ij} \cdot y_i \cdot a_j \cdot b_{i-j}$, where N_{ij} is the population at risk, y_i is a year factor, a_j an age factor, and b_{i-j} a birth year factor" (and r_{ij} are the observed delinquent rates).

Now, when r_{ij} has a Poisson distribution with mean μ (and therefore variance $=\mu$), then $x = \frac{r_{ij}}{\mu}$ is a variable whose distribution is such that mean $=1$ and variance $= \frac{1}{\mu}$.

Consider $f(x) = \sqrt{x} \cdot \log x$; the Taylor Series approximation to $f(x)$ in the region of m is $f(x) = f(m) + f(m) \cdot (x - m)$. We can write this as

$$\frac{f(x)-f(m)}{f'(m)\sigma_x}=\frac{x-m}{\sigma_x}.$$

But since

$$f'(m)=\left[\frac{\sqrt{x}}{x}+\log x\cdot\frac{1}{2\sqrt{x}}\right]_{m=1}=1$$

$$\text{and } f(m)=0, \qquad \text{when } m=1$$

we obtain

$$\frac{x-m}{\sigma_x}=\frac{\sqrt{x}\cdot\log x-0}{1\cdot\left[\frac{1}{\mu}\right]^{\frac{1}{2}}}.$$

But since $\frac{x-m}{\sigma_x}$ is standardised (mean$=0$; variance$=1$) so also is $\sqrt{r_{ij}}\cdot\log\left[\frac{r_{ij}}{\mu}\right]$, which we can therefore take as approximately $N(0,\ 1)$[8].

The difficulty with this approach[9], is that we cannot assume that the a's, y's, and b's are all independent, for the set of cells (b_{i-j} : such that $i-j=k$) is identified by the appropriate a_j and y_i.

Thus they fitted 47 unknown (14 year constants, 10 age constants, and 23 birth year constants) by least squares, with three linear restrictions on the parameters to obtain a solution. Their results are as follows:

" (i) If only y_i and a_j are fitted, the residual sum of squares — that is the sum of squares of (1) or approximately

$$\Sigma(r_{ij}-E_{ij}'^{2}/E_{ij}'^{1}) \text{ where } E_{ij}'=N_{ij},\ y_i,\ a_j,\ B_{i-j}\ 650.$$

(ii) If y_i, a_j and b_{i-j} are fitted, it is 213 (p. 146).

(v) If y_i and a_j and two birth year constants, one for the years 1937-44 and one for all the other years, are fitted, it is 443".

So there is a strong reduction in the residual sum of squares by the addition of a birth year effect (from 650 to 213); moreover, nearly half of this reduction in the residual sums of squares could be accounted for by supposing only that the birth years 1937-44 were different

[8] The more usual transformation of a Poisson variable r_{ij} the square root, $\sqrt{r_{ij}}$.
[9] Personal communication on mathematical/statistical theory from J. H. Darwin to R. A. Carr-Hill.

from the rest, "of the 650 residual after fitting y_i and a_j, about two thirds can be attributed to a birth year effect, about one seventh to random variation, and the rest to factors not yet considered." (p. 146).

But we can already see the advantages of using an explicit model of this sort. We can discuss more precisely the effects of different postulated factors and their significance in quantitative terms.

3. Further Analyses.

Analysis of Variance.

We decided to attempt an analysis of variance using a basically additive model with the delinquent rates. Thus the delinquent rate per capita in any given age-year cell was assumed to be given by $DR_{ij} = a_i + y_j + C_k$ where DR_{ij} is the delinquent rate for age i and year j

a_i is the effect due to age group i

y_j is the effect due to year j

and C_k is the effect due to cohort k.

This is analogous to the New Zealand analysis, although Darwin seems to have used a *multiplicative* model for estimating the delinquent rate in any given age-year cell.

We conducted two analyses in the light of the following questions:

a) How much of the data can we legitimately analyse? For if we are interested in isolating a *birth-year* effect, we can only sensibly consider, *for analytic purposes*, those cohorts on which we have data for the whole age range in which we are interested. Otherwise we shall be basing our inferences about the differential effects of different birth years on data for cohorts measured over different birth years on data for cohorts measured over differing spans of their history. To take an example: if we choose to span the age range 8-20 years then we have complete data on the number of boys at each age found guilty in the years 1946-63, so that we have comparable data for the cohorts born in the years 1937/8 to 1942/3. If, to take another example, we restrict the span of the age range to 10-20 years we have complete data for the years 1946-1965, that is for the cohorts born in the years 1935/6 to 1944/5. The first example gives us complete data for 6 cohorts over 13 years, and the second gives us complete data for 10 cohorts over 11 years. In fact we carried out both of these analyses.

b) Should we (as in the methods above) use all the information in a rectangular table, in order to standardise for the age effect and the year effect? For, in isolating a *birth year* effect we are looking at complete cohorts, and so we want to standardise only for those age-effects and year-effects which affect the particular age-year cells through which the cohorts pass. There are four theoretical possibilities; we can standardise by age, or by year, either by using all the information in the table or by using only that information contained in the age-year cells belonging to complete cohorts. It seems reasonable to argue that we should calculate the age effect only from those age-year cells belonging to complete cohorts since the behaviour of the like-aged delinquents in other years should have only marginal relevance to the behaviour of the cohorts. On the other hand, years do have distinctive delinquent patterns determined, to a large degree, by public and plice attitudes, which are not directed at any *specific single* age group; so we should estimate the year-effect from as much data as possible. And so we estimated cohort effects after standardising (i) for the year effect, by using all data in the age-range considered; and (ii) for the age effect by using only the data for the cohorts (a trans-cohort standardization for age).

Results: 6 cohorts.

Giving a delinquent a value of 1 and a non-delinquent a value of 0 we estimated the within cell sum of squares as follows: the mean probability of being delinquent from all the data in the table is $d=0.01505$ and the total number of boys in the full table is $n=72{,}030{,}000$ so that the total number of delinquents is n.d. But since each person is an observation on the proportion d the total sum of squares is;

$$(\text{n.d.}) \times (1-0.01505)^2+(72{,}030{,}000-\text{n.d.}) \times 0.01505^2$$
$$\approx \text{n.d.}(1-d) \approx 1{,}067{,}000.$$

Moreover, the total degrees of freedom are the total number of boys n, (minus 1), so that the total mean square is $\text{n.d.}(1-d)/n=d{:}(1-d)$. Therefore, by the model, the mean for the cohort in a given year

= overall mean number of delinquents

\+ average (deviation) number of delinquents in that year

\+ trans-cohort (deviation) delinquents for that age

(+ a birth year (deviation) effect on cohorts).

If we ignore the two bottom rows of Rose's table (because they are incomplete) we are left with data for 18 years and so we fit 17 constants for the deviations of years about the general mean, and as there are 13 age groups in the table we fit 12 constants for the age groups. Each of these 12 constants is fitted to the age data across the cohorts only, *not* to all the available data for the age to which it refers. The 29 axes representing years and age groups do not form an orthogonal set and the analysis took their correlations into account in fitting the constants (see Fisher 1936). It is not, however, possible to make allowance for the correlation of errors which results from the fact that a boy who is a delinquent in one cell has a higher probability of appearing as a delinquent in another cell (see below).

When we come to the fitting of constants for the birth-cohort effect it is evident that this effect may be schematized in any number of ways. However, two basic approaches may be distinguished

a) the provision of a dummy variable for each cohort such that members of the cohort have a score of, say, 1 and non-members have a score of, say, 0.

b) the provision of a linear polynomial ordering the six cohorts in time, with four other, higher-order, polynomials to take up the remaining degrees of freedom for cohorts.

The sum of squares for cohorts would be the same whichever schematization of the model is chosen, so long as it takes up all five degrees of freedom for the six cohorts. We have presented the results from the second approach, since this would enable us to decide whether there had been any discernible trend in the cohort effect.

The polynomials representing the cohort effects are not orthogonal to one another or to the 29 axes of the basic model and so once again the computation has to take correlation of axes into account (see Fisher, 1936).

TAB. 2.

Source	Degrees of Freedom	Sum of Squares	Mean Square
Between Cells	233	3413.3	14.6
Model (Years + Ages)	29	1436.0	49.5
Model + Polynomials	34	1444.6	42.5
Increment due to Polynomials	5	8.6	1.72
Within Cell (Residual)	n-234	1,064,000	.01482
Total	n-1	1,067,000	.01482

So the addition of the polynomials to the model does have a significant effect, but the contribution to R^2 is only $\frac{8.6}{1{,}067{,}000}$ which is very small. And of this 8.6 only 6.1 is due to a *linear* ordering of the 6 cohorts.

Results: 10 cohorts.

We used a similar method of estimation and found that the mean probability of being delinquent was .01778.

Therefore, using the same model, mean delinquency for the cohort in a given year = overall mean number of delinquents + average (deviation) number of delinquents in that year + transcohort (deviation) delinquents for that age + a birth year effect (deviation) on cohorts.

So the addition of the polynomials to the model does have a very significant effect, but the contribution to R^2 is only $\frac{23.0}{1{,}069{,}000}$ and of the 23 only 17 is due to a linear ordering of the ten cohorts.

It will be noticed that, in both cases, there is a large amount of unexplained sum of squares. Not all of this is due to the inadequacy

TAB. 3.

Source	Degrees of Freedom	Sum of Squares	Mean Square
Between Cells	219	2735.3	12.5
Model	29	1629.1	56.2
Model + Polynomials	38	1652.1	43.5
Increment due to Polynomials	9	23.0	2.56
Within Cell	n-220	1,066,000	.01746
Total	n-1	1,069,000	.01746

of the model, for there are some contributions from the upper right and lower left triangles of the data matrix which are irrelevant to the cohorts.

We could have extended the analysis further using this table in the following ways:

a) Recent work on the analysis of aggregated qualitative data seems to show that we should consider a model like

$$\log\left\{\frac{DR_{cjk}}{1-DR_{cjk}}\right\}=a_i+y_j+c_k\ .$$

This is the logistic form of the relationship between the delinquency rate and the factors which we suppose determine it.

b) We could have tried to reduce the numbers of degrees of freedom in the analysis by combining single age and year groups into clusters. These should have apparently similar delinquency patterns in terms of the observed delinquency rates and be such that we expect the effects of age, year and generation to be homogeneous within each cluster.

We decided not to continue with this scheme of analysis for a number of reasons. Without further evidence as to either the exact form of the relationship or the exact way in which we should cluster the age or year groups, it does not seem worthwhile to speculate on these points. Moreover, as we pointed out, there is a difficulty with interpreting the analysis of variance table, since the samples for each cell of the table are not independent of each other. This means that the population figure of 72,030,000 is artificially inflated. Only if we are prepared to say that the factors that effect the probability of being brought before the court in each year are independent for different years, can we make straightforward inferences. But all of the analyses we have done seem to lead to similar small, but positive results. And the problems which we broach in the next section seem more important.

On this analysis, therefore, the general hypothesis that there is a birth-year generation effect on delinquency rates is sustained, but surprising if there had been *no interaction* effect in such a large table; but this does seem to be systematic in a very minor way. We are not, however, in a position to defeat or confirm the *specific* delinquent generation hypothesis. All we can say is, "whether being born in a certain year has any effect on the likelihood of a child being brought before the court in any subsequent calendar year or at any calendar age".

Factors Affecting Becoming Delinquent.

This conclusion is not very substantial. First of all, it considers only the effects of age year on the likelihood of becoming an identified delinquent. Thus we cannot conclude from an investigation of this sort, that certain birth years, *per se*, predispose to becoming delinquent — without taking into account all the other factors which are known to affect the probability of being identified as a delinquent. For example, since the data is on findings of guilt (whether first or subsequent) we cannot distinguish a birth year effect on the likelihood of becoming delinquent from a change in police practice, e.g. a more thorough use of records in their search procedures.

Some people have therefore suggested that the only way to gauge the number of delinquents of any given birth-group is to find out how many individuals of that birth-group have become delinquent before, say, adulthood, or before retirement. This means following up the careers of a given cohort over a long period and this is not always possible. The National Survey of Health and Development followed up a sample of 5,362 children born one week in 1946. The sample was weighted in favour of middle class families for their own purposes. After adjustment they found that 10.4% of males had been convicted of one or more indictable offence, and 14.6% had been convicted of any offence before they were 18.

Alternative methods all have some disadvantages such as the need to eliminate double counting, and the need to estimate the size of the liable population. But they can give us a cheap estimate of the expected delinquent potential *now* rather than in 10 years time. The best attempt to date (by Alan Little) proceeds as follows. He used Table 5 of the Supplementary Statistics to calculate the number of individuals at any age found guilty of indictable offences during each year, and the Registrar General's estimates for the size of the population in each age group. Since the figures were not available before 1959 he used the age-specific rates for 1962 and found that 16% of boys had been convicted of an indictable offence before they were 21. Although this procedure ignores the effects of population mobility (death or immigration), the fact that boys born in e.g. 1942 could be 19 in both 1961 and 1962, and the changing likelihood of becoming delinquent, his estimates are not seriously affected. It seems likely that between 14% and 17% of males would have been found guilty by 21 for these cohorts.

This method does seem to give an adequate answer to a modified version of the original question. We can determine, "how likely children born in certain years are to be brought before the courts compared with children born in other years, and how this pattern continues into adult life".

But we have not really dealt with the major criticism, for we have not isolated a birth-year effect independent of all other factors on the probability of becoming a delinquent.

And more important, we have not allowed for the changing (and mostly unknown) relationships between actual incidents and recorded offences, and not even for the gap between recorded offences and identified delinquents. We cannot answer the *original* question about the delinquency potential as distinct from the likelihood of becoming delinquent of different birth years without making some attempt to allow for area variations and time changes in these relationships. In other words we are taking up the problem we first posed.

4. Use of an Ongoing Model.

None of the methods so far has considered the effect of "other factors", apart from age and year, on delinquency except in so far as a) they affect the "delinquent" rate through either age or year; b) they contribute to the residual.

This does not seem legitimate. We know, both theoretically and empirically, that many other factors affect the delinquency rate of a given age group or in a given year *directly*. Therefore, unless we allow for these other factors appropriately, we shall "contaminate" the direct and interactive effects of age and year on delinquency rates and, therefore, on delinquent rates. Hence we cannot be secure in our inference about the "generation effects".

In theory, the "correct" way, therefore, would be to use a model which stipulates which factors should affect the delinquency rate and how; and to enter corresponding variables in an appropriate scheme of multivariate analysis. In fact, the general strategy should be to move from features of the population to the recorded offence rates, rather than working from the given delinquent rates. But a consensual theory of this sort is not available. Some work has been done, however, on the structural equations determining the offence rates and clear up rates in 1961 and 1966. A description of the model used is given in the Appendix.

We shall show how this model can be used to test hypotheses about the effect of the age groupings in the population on the offence rates, and, in particular, a hypothesis corresponding to the D. G. H.

Using cross-sectional data for England and Wales 1961 and 1966 Carr-Hill and Stern (1971) found the following coefficients and t values, for the effect of the age distributions of the population of an area [10] on its indictable offence rate.

The coefficients in this table indicate the degree to which inter-area variation (ceteris paribus) in the proportion of young males in the population will affect the offence rate. To be exact a 1% increase in the proportion of young males implies an increase of b in the reported offence rate (where b is the coefficient and p the clear up rate).

We can see that the proportion of young males aged 15-24 in

[10] Indicated by the proportion of the population who were male and aged 15-24.

the population has a significant positive effect on the offence rate in 1966 but not in 1961. But, since the coefficients are not consistent between the two years, we cannot conclude that an increased proportion of young males implies an increased offence rate (as do, for example, Ahamad and Willmer) without further investigation ... Our first thought, on obtaining these results was that different cohorts affect the offence rates in different ways.

TAB. 4. EFFECT OF AGE DISTRIBUTION (MALES 15-24/POPN.) ON THE OFFENCE RATE IN ENGLAND AND WALES.

Effect on Offence Rate	1961: 64 Urban Police Areas	1966: 66 Urban Police Areas	1966: 110 Urban & Rural Police Areas
Coefficient	+ 0.09	+ 0.45	+ 0.40
t value	(0.54)	(2.80)	(3.79)

This led us to try a more subtle examination of the possible cohort effects. Therefore, we attempted a similar analysis with three age-group variables 15-19, 20-24, and 25-29 in both years with the following results. Note that those born in 1941-1946 would be 15-19 in 1961 and 20-24 in 1966, and so on.

TAB. 5. EFFECTS OF DIFFERENT AGE DISTRIBUTION (MALES 15-19, 20-24, 25-29, PER POPN.) ON THE OFFENCE RATES IN ENGLAND AND WALES.

Effect on Offence Rate	1961: 64 Urban Police Areas			1966: 66 Urban Police Areas			1966: 110 Urban & Rural Police Areas		
	15-19	20-24	25-29	15-19	20-24	25-29	15-19	20-24	25-29
Using *p*	—.25	—.11	+.40	+.05	+.23	+.23	+.24	+.22	+.20
Clear up	(0.53)	(0.48)	(1.28)	(0.17)	(0.95)	(0.68)	(0.012)	(1.06)	(0.74)

Since the D.G.H. supposes that those born during 1937-41 would have been most affected by the war, we expect that the coefficient of 20 - 24 year olds would be higher in 1961 than it would be in 1966 and vice versa for 25 - 29 year olds. As we can see, no such conclusion can be drawn from our investigation. Indeed, we cannot make any firm statements about the deliquency of any specific age-group or cohort. We can perhaps speculate that there has been a shift in the ascription of delinquency towards the younger age groups, which bears out Layzell's general conclusions.

5. Discussion.

Thus we are unable to separate out the effects of different five-year age groupings on the gross indictable offence rate [11], because of the simple correlations between adjacent age groupings being so high, which implies serious multicollinearity. *A fortiori* we would not be able to separate out the effect of single ages in the offence rates. We cannot, therefore, in the present state of statistical arts, decide whether one age group or one cohort contributes more than another to the gross offence rate.

Yet, apart from the specific D.G.H., many explanations of the origin of crime and delinquency have postulated immaturity [12], or the adolescent situation, as an intermediate variable which accounts for the onset of offending in the teens and which is to be explained in terms of other underlying psychological or structural factors. And the delinquent generation phenomenon was interpreted in terms of Bowlby's claim that there was a link between offender's personality and juvenile delinquency.

Glueck's account of delinquency, for example, stresses the stages of psychological development in becoming an adult. They claim that delinquents are more "backward" psychologically. Cohen's account, on the other hand, deals with the development of a malicious, negativistic, and non-utilitarian ethic among working class children subjected to the middle class school. Moreover, many other sociological theorists take teenage-specific delinquency as the explicandum, since it appears to be the quantitatively more important phenomenon, and adult crime is usually seen as a continuation of behaviour patterns started in youth. For example, the D.G.H. itself was postulated as an explanation for an apparently worrying rise in teenage delinquency in the 1950's, which was also for some, the symptom of a widespread moral degeneration. If teenage delinquency could be "curbed" it was assumed that the "crime problem" was solved.

We would, therefore, expect in general, that areas with larger numbers of teenagers than normal for their population size, would have more offences.

In the same way, youthful delinquents are assumed to be easier

[11] One criticism of our analysis was that we had not considered offence types separately in the model. But we would not have been able to specify many of the important factors with respect to specific offences.

[12] Recent explanations have occasionally been couched in terms of a "generation gap". This refers to the supposed emergence of an autonomous "youth establishment". This can be taken either to refer to immaturity in the sense above, or to Cohen's account of delinquency.

to catch and convict; for later on in their careers they will have learnt some sophistication. So that we would also expect that the presence of a larger number of teenagers, who commit more offences, would raise the clear up rate; and since sophistication presumably applies to all stages of the legal process we would expect there to be an even larger effect on the conviction rate [13]. None of these effects appear consistently.

Our explanation, therefore, of the results, cannot simply be in terms of youthful delinquency, or the lack of it. As Carr-Hill and Stern (1971) show elsewhere, a major intervening factor in the generation of different recorded crime rates seems to be area-patterns of, or time changes in, police behaviour. In other words, whatever the "true" offence rate and the factors affecting it, the recorded data cannot be explained solely in terms of "criminogenic" factors [14].

Thus police patterns, in general, changed from informality to formality during the early '60s, and the police force became more bureaucratised. Whereas previously if the police noticed a juvenile offender they may not officially have bothered to "book" him, but instead sent him home, now they would record an offence. In this way the police *were* spending time on but ignoring (for the purpose of official statistics) some of the more easily detectable offences. Thus we would expect the change from 1961 to 1966 to be such that the numbers of teenagers would apparently become important in the determination decrease in importance in the determination of the clear-up rate from zero to a positive coefficient; and that the numbers of teenagers would decrease in importance in the determination of the clear-up rate from having a negative coefficient to zero. And this is indeed what we found.

Thus, we found it necessary to explain the *majority* of the variations in the *recorded* crime rates between areas and over time, in terms of variations in patterns of police behaviour, so it is difficult to see how we can legitimately make inferences from the previous types of analyses about the offending patterns of any given sub-group of the population. For if the trends observed in recorded crime and delinquency rates cannot be ascribed to the factors which are supposed to affect the amount of crime and delinquency, we cannot use them, let alone the criminal and delinquent rates, to test hypotheses about the offending patterns of specific age-groups in the population. And, in general, the official figures do not lend themselves easily to any interpretation about the crime patterns of any sub-group.

[13] Carr-Hill and Stern examined relationships involving convinction rates as well as clear up rates.

[14] A criminogenic factor is a factor like social class, which is supposed to predispose towards delinquency.

We would like, in theory, to examine the age-year specific rates in both the original table and a table of offence rates using such a structural model. And if we could make reasonable assumptions about the differential importance of the various factors in affecting different age groups, we would be able to elaborate the relationships between police behaviour and delinquent rates considerably. Even given that we cannot ascribe the offence committed to a particular age group (unless they have been cleared up), we would be able to proceed using the series of equations for the determination of age-specific clear-up rates and only one equation for the gross offence rate and one equation for the recruitment of policemen [15]. But the data we require, in order to specify important effects, are available only in census years. It, therefore, seems unlikely that we shall be able to proceed any further with such a model.

6. Conclusion.

We must, therefore, conclude that, although recorded offences are rising rapidly and especially those which can be attributed to teenagers, it does not seem possible to make inferences about the delinquency of a specific cohort or age-group from official statistics. On the other hand in the table which has been the main focus for discussion there is an interactive effect of age and year on the age-year specific clear-up rates.

The methodological lessons are that

a) the expectation method does not allow us to estimate any parameters of the model;

b) the analysis of variance still suffers from an over specification so that we ascribe too much weight to the age and year factors;

c) in both cases we are unable to allow for the differential effects of the crime pattern on different age groupings. Moreover, apart from the criticisms in terms of the different offence types, we find that a thorough explanation would require information that is unavailable;

d) and lastly the "correct" statistical approaches, even with the appropriate data, suffer from serious multicollinearity; it seems unlikely that we shall ever be able to fully meet this difficulty, since adjacent age groupings inside one country are likely to be intercorrelated.

The most promising avenue would seem to be to combine the

[15] It is disputable, of course, whether the age-specific clear-up rates do depend on either the gross offence rate or the gross cop/pop ratio. In this case we would be left with a two equation model.

estimation of the cumulative number of delinquents in a cohort, with an estimate (from the sort of structural model that we have considered) of the effect of other factors in order to infer the delinquency of a cohort. But this would require both precise theoretical knowledge about the interrelations between crime and its social control and detailed empirical data before the powerful statistical techniques available can be used. And whether or not there is a cohort effect it can be of no more than marginal importance after we have allowed for age and epoch.

7. Appendix.

The Model.

The model that we are considering arose through an attempt to account for the observed variations in recorded offence rates and recorded clear-up rates in the different police areas of England and Wales in the two census years 1961 and 1966. Since we were concerned to estimate the population parameters of the process generating the observed offence rates, we had to develop a causal model of this process. We had started with the assumption that the offence rate depends partly on the risk in terms of the probability of detection and that the strength of police presence would inhibit some offences and so depress the recorded offence rate. But the probability of detection presumably depends on the amount of police effort, and the scale of the problem (in terms of the numbers of recorded offences) would affect the clear-up *rate*. And lastly the allocation of police manpower, which depends partly on local authorities, would be affected both by the apparent magnitude of the crime problem and the apparent efficiency of the police force in dealing with it.

A set of interdependencies of this sort gives rise to a simultaneity problem when we try to estimate the population parameters, for if O/pop (the recorded offence rate) depends on p (the clear-up rate) and p depends on O/pop, then we cannot interpret directly the correlation between O/pop and p as a uni-directional effect *even when we have allowed for other factors*. In fact, the problem is insoluble unless we have other information. The usual approach is to develop an *identified* model, by bringing in a sufficient number of exogenous variables into each equation of the system. In this way we were led to estimate the following model

$$O/pop \sim p, cop/pop, X,$$
$$p \sim cop/pop, O, X_2$$
$$cop/pop \sim O, p, X_3$$

with O/pop, p as in text, cop/pop is number of policemen per capita and X_1, X_2, X_3, are vectors of exogenous variables.

In the light of our results we had to make important modifications to the interpretations which we placed on the equations of the system. We found that we were explaining most of the changes in estimates as due to changes in police behaviour and that the simplest account of the third equation was in terms of recruitment.

In the basic model we used only one gross measure for the age distribution of a given area. In the text we have shown how this "comprehensive" model can be used to answer the much more specific questions about the existence of possible generation effects. And, in general, this would seem a good research strategy just as a general equilibrium model in economics is used to answer specific questions about the effects of certain changes.

But we may find that we have to develop a specific model to answer this question, where the age-group specific delinquency rates for each year are the primary explicandum. To answer the question properly we shall have to decide whether the interdependency between age-specific offence rates and age-specific conviction rates is important. And the available evidence suggests it is: Willcock and Stokes showed that the number and type of self-reported offences among adolescent boys depended on their estimate of p.: and police forces tend to have specific drives against juvenile gangs because of their level of offending. (If so, we cannot test any such model since we only know the age-specific conviction rates for recorded offences, and not the age-specific offence rates.

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