SEAT BELT SAVINGS

IMPLICATIONS OF EUROPEAN STATISTICS

SUMMARY

Available data for eight western European countries which introduced a seat belt law between 1973 and 1976 suggest that it has not led to a detectable change in road death rates. A simple model suggests that the law was followed by an 11% increase in injury rates. However, there is evidence that the model is too simple and it is concluded that the data are consistent with a 'no change' hypothesis. The results are not compatible with the Department's "1,000 plus 10,000" estimates (for front seat vehicle occupants) which are an extrapolation from the observed apparent savings among voluntary wearers.

J E ISLES

STG DIVISION (Dept. of Transport)

April 1981 — Well before parliamentary debates!

ument — None of the advocates of a belt law who quoted the "1000 plus 10000" estimate were disabused by the DTP, even though on the best evidence available at the time it was clearly untenable.

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1. INTRODUCTION

The Department has for some years quoted figures of 1,000 lives and 10,000 serious injuries which might be saved among front seat occupants of cars and light vans, if all available seat belts were worn. These estimates mainly depended on the observation that belted drivers had lower casualty rates than unbelted drivers, and the (debatable) assumption that without belts the two groups would have had the same casualty rate. Because of their uncertainty, the estimates were heavily rounded down Repeating the calculations for 1979 suggests more modest possible savings, about 900 lives and 9,000 serious injuries. Legislation to compel seat belt wearing would not achieve 100% wearing. 85% wearing might be expected to achieve about 80% of the possible savings (assuming the wearing level is currently 26%, and that compulsory wearing is as beneficial as voluntary wearing), ie 720 lives and 7,200 serious injuries. Assuming compulsion had no effect on other classes of road user, this would mean a reduction of 11% killed and 9% seriously injured, based on the 1979 casualty totals for all road users.

2. Mr Adams of University College, London, has recently published a comparison of total road deaths 1970-78 in groups of countries with and without an 'effective' seat belt law, and argues that deaths have been reduced less in the 'law' group than in the 'no law' group. Various criticisms might be made both of his methods and of the inference drawn from them. Probably the most telling are:

(a) The variation in trend between countries is greater than a 'seat belt law effect' of the order of magnitude considered above. Thus any beneficial effect could be concealed by rejecting certain countries, or by allocating marginal cases to one group rather than the other, without impartiality.

Answer: Each country was examined separately as well, so this variation was not concealed. Individual graphs and 4 short comparisons.

(b) Even if the 'observed' differences are accepted as real, they may not be a law effect. It is possible that pressure for a seat belt law was greater in countries where casualties were falling in numbers more slowly, or even rising, and less in those which had achieved reductions in casualties by other safety measures. In other words, the difference might be the cause of the law, rather than the law being cause of the difference.

3. Nevertheless, international comparisons provide the only information about the effect of compulsory seat belt wearing, both on car occupants and on other road users. (It has been argued that drivers forced to wear belts drive more dangerously, leading to increased numbers of casualties among pedestrians, cyclists and motor-cyclists - the 'migration hypothesis'.) It is desirable to check whether international data are consistent with estimated savings based on GB data (which relate only to vehicle occupants); conceivably they might indeed now yield better estimates.

4. THE DATA

Adams' 'law' group contained 13 countries: Belgium, Denmark, Finland, France, West Germany, Netherlands, Norway, Spain, Switzerland, Sweden, Israel, Australia, New Zealand. France, which did not compel wearing in urban areas within the period considered, and Switzerland, which repealed its law after only 20 months, should properly have been excluded. Israel, Australia and New Zealand have also been excluded from the present analysis as casualty and traffic data were not readily available in sufficient detail. This probably has the (unintended) effect of making the 'law' group more homogeneous since the remaining eight countries are in western Europe.

Italy + Britain
over 100 million population and
40 million vehicles ≠ too small a control!?

5. Adams' 'no law' group contained 4 countries: Great Britain, Italy, United States, Japan. Japan, which had a seat belt law, should properly have been excluded. Casualty and traffic data for United States were not readily available in sufficient detail. The two remaining (western European) countries are too few to provide a satisfactory 'control group' for assessing the law effect, and the analysis must allow for this.

* No. Not enforced. Very low wearing rates - 5 to 10%.

6. The main source of overseas data was the annual "Statistics of Road Traffic Accidents in Europe", published by the UN Economic Commission for Europe. Traffic data were supplemented using "World Road Statistics", published by the International Road Federation. The groups of casualties considered were:

(a) Car users, whom a law mainly seeks to protect;

(b) Pedestrians
(c) Cyclists and motorcyclists } whom, under the migration hypothesis, a law might endanger.

(d) Others (mainly occupants of buses, coaches and goods vehicles), on whom the net effect a law might have is unclear.

In each group, numbers killed and injured were considered separately.

7. Car traffic data were available for each country. For two-wheeled traffic, the available overseas data relate only to motor vehicles, often excluding mopeds. In investigating trends in casualty rates, the available data have been taken as a proxy for total two-wheeled traffic. Similarly, 'other traffic' is represented either by bus and coach traffic or by goods traffic, where only one was available. Pedestrian casualty rates were calculated per 100 million vehicle kilometres of motor traffic, for as much motor traffic as was covered by the published data. Since these always included car traffic, any error involved is unlikely to be important. Because of the inconsistencies between the classification of casualties and of traffic, rates were reduced to the form of an index in order to identify trends rather than comparing absolute values.

8. Because of inconsistency between publications, frequent revisions and occasional gaps in the data, various adjustments were necessary to the raw data. These involved apportionment of known totals by comparison with adjacent years, splicing, occasional interpolation, etc. It is doubtful whether the results would have been appreciably different had the latest estimates been sought from the countries concerned. Since it was the traffic figures which were mainly affected by these adjustments, and some of the traffic figures were suspiciously round, numbers of casualties have also been analysed, without reference to traffic figures, to obtain

separate estimates of the law effect. However, the analysis based on casualty rates is believed to be more reliable.

9. THE ANALYSIS

Results are presented first for numbers killed, then for numbers injured. Car user deaths are discussed in some detail to illustrate the methods used throughout. Subsequent results are presented in summary form except where evidence of a significant law effect requires closer examination.

10. CAR USER DEATHS

Figure 1 shows the trends in car user deaths in each of the eight 'law' countries during 1970-78. Although six countries seem to show a similar trend, two are outliers. In such circumstances, it is well known that the median is likely to be a more stable estimator of central tendency than is the arithmetic mean. Figure 2 shows the trend of the median for 'law' countries, with upper and lower quartiles defining the boundaries of what might be called a '50% confidence region'. The 'no law' countries are plotted individually. The curve for Great Britain lies wholly, and that for Italy almost entirely, within the 50% confidence region. Thus the trend for 'no law' countries does not differ detectably from that for 'law' countries.

11. A possible explanation is that the 'law' effect, which is here spread over the years 1973 (Finland) to 1976 (Denmark, Germany), is too small to show up. Figure 3 is a plot of the median trend, with upper and lower quartiles, in which year 0 is that in which the law took effect, and the indices have been rebased so that the year three years before the law took effect = 100. If there were a law effect, one would expect most of it to be between years -1 and +1. From inspection of the figure it appears that the effect cannot be large.

12. Numbers of casualties and casualty rates for certain classes of road user have varied by a factor of 2 or more for some countries during the period 1970-78. One

cannot safely hypothesise an underlying linear trend, since that would imply that in such cases there would in the near past or future be a negative number of casualties. More reasonable is to suppose an underlying exponential growth or decay, so that the trend in log (casualties) or log (casualty rate) is linear. Let C_t be the number of car user deaths t years after the law becomes effective ($-5 \leq t \leq +3$).

Let $z(t) = 0$ for $t < 0$

undefined for $t = 0$

1 for $t > 0$

Then the model

$$\log C_t = a + bt + cz(t) + e_t$$

corresponds to an exponential trend in the number of casualties, with a jump of c (law effect) between the years immediately preceding and following the law, and an error term. If the model is correct and the e_t are independent and identically distributed normal variables, we can estimate c and test its significance, by straightforward multiple regression. These assumptions are unlikely to be quite right, but the estimated uncertainty in c will be a useful guide to its reliability. It should be noted that the law effect may not be instantaneous, so that this method of analysis would tend to underestimate it. Other effects such as a package of simultaneous safety measures, or the fuel crisis in the case of countries introducing the law in or around 1974, cannot be separated from the law effect. One must hope that error from this source is counterbalanced by safety measures in other years and the fuel crisis in other countries, which would be confounded with the trend – as would any tendency for the law effect to wear off with time. A more complicated model would be needed to take these points into account.

13. The fitted equation is

$$\log C_t = 1.982 - 0.00225\ t - 0.02488\ z(t)$$

where the standard error of the coefficient of z(t) is 0.02507. That is to say, the seat belt effect is estimated to be a reduction of 5.6% ± 5.8% of car user deaths.

This is of course not significant. Figure 4 shows the fitted curve. Although a drop in the law year (or, more probably, in the year before the law became effective) is plausible, the fit is clearly not very good. A drop of 5.6% in car and light van user deaths would be a saving of 145 lives, for GB 1979. The possible saving of 720 lives mentioned in para 1 would be a drop of 27.8%, about 4 standard deviations away from the estimate based on international data. This would imply that the international data are compatible with a 'no change' hypothesis, and not compatible with the Department's previous estimates.

Repeating the analysis for each country separately, we have the following results.

	Law year	Law effect	Standard error
Finland	1973	- 16.5%	16.5%
Spain	1974	- 19.2	5.3
Belgium	1975	- 30.3	36.4
Netherlands	1975	+ 10.3	17.9
Norway	1975	+ 5.4	6.0
Sweden	1975	+ 5.8	3.8
Denmark	1976	+ 14.2	19.5
Germany	1976	+ 12.7	9.7
Lower quartile		- 17.8	
Median		+ 5.6 (increase)	
Upper quartile		+ 11.5	

Belgium clearly does not fit the exponential trend model, as is reflected by the large standard error, so its result may be disregarded. Only for Spain is the reduction 'significant', and inspection of Figure 1 shows that this is probably because it happened to introduce its law in the same year as the 1974 fuel crisis, which caused a reduction in casualties in most countries in that year. For countries introducing the law after 1974, the fuel crisis effect is taken as part of the trend, thus swamping any law effect.

15. It is clear, then, that the law effect cannot reliably be estimated using numbers of casualties, because the reduction in traffic due to the fuel crisis leads to a reduction of casualties which becomes confounded either with the law effect or with the trend. Casualty rates are more likely to yield sensible estimates. Figure 5 shows that these show a more consistent trend than numbers of deaths (Figure 1); Belgium would not be an outlier but for the suspect 1970 figure. Figure 6 shows the median and quartile trends, with 'no law' countries plotted separately. Great Britain and Italy show a slightly slower drop in casualty rate than the median of the 'law' group, but their curves are mostly below the upper quartile so the difference is clearly not significant. Figure 7 shows the median and quartile trends in fatality rates, where year 0 is that in which the law took effect. There is no evident jump near year 0.

16. Let R_t be the fatality rate t years after the law becomes effective. As before, we postulate the model

$$\log R_t = a + bt + cz(t) + e_t$$

which is in fact precisely equivalent to the model for numbers of deaths in the case when traffic follows an exponential trend. The fitted equation, illustrated in Figure 8, is

$$\log R_t = 1.891 - 0.0335\ t + 0.01003\ z(t)$$

where the standard error of the coefficient of z(t) is 0.03183. That is to say, the implied seat belt effect is an increase of 2.3% ± 7.3% of car user deaths. This is consistent with a 'no change' hypothesis, but about 4 standard deviations away from the estimated saving of 720 lives per annum for GB.

17. OTHER ROAD USER DEATHS

The law effects for other classes of road user, estimated using median trend in numbers of fatalities and fatality rates, are as follows.

	Number		Rates	
	Law effect	Standard error	Law effect	Standard error
Pedestrians	- 4.9%	5.2%	- 1.8%	2.8%
Two-wheeler users	- 7.6	5.6	+ 8.0	14.5
Others	0.0	8.5	+ 5.4	15.0

None of the effects are significant. Applying the results to GB 1979 figures, the implied net effect of a seat belt law is calculated below.

	Based on numbers		Based on rates	
	Effect	Standard error	Effect	Standard error
Car users	- 136	141	+ 56	177
Pedestrians	- 104	110	- 38	59
Two-wheeler users	- 112	83	+118	215
Others	0	28	+ 18	49
All road users	- 352	199	+ 154	289

The standard errors for 'all road users' have been calculated assuming the errors for individual road user groups are independent. The calculation based on rates is believed to be more reliable, but neither method gives a result significantly different from 'no effect'.

18. INJURIES

In international statistics, the injured are not classified as serious or slight as in GB. Definitions of 'injured' vary, as probably does completeness of reporting, from country to country. The following table shows the ratio of numbers injured to number killed reported by each of the 'law' countries and in GB.

	1970	1978
Belgium	69	34
Denmark	21	23
Finland	15	14
Germany	28	35
Netherlands	21	27
Norway	21	28

	1970	1978
Spain	21	21
Sweden	17	20
Median for 'law' group	21	25
Britain (serious + slight)	47	50
(serious only)	12	13

Although other explanations are possible, it seems likely that the median for the 'law' group corresponds to a lower severity limit intermediate between those of GB serious and slight categories.

i.e. derived using the methods which produced the 1000 - 10 000 (or 100% wearing rates in G.B.

19. The results in para 1 suggested that voluntary wearing, at the level which has been achieved by compulsion in some 'law' countries, might lead to a 9% reduction in the seriously injured casualty total for GB. Repeating the calculation for serious plus slight suggests a 1% reduction - again, for all road users. Thus for severities typically counted in 'law' countries, the expected reduction might be of the order of 5%, or less if there is also less complete reporting.

20. Using the same method as for deaths, estimates of the law effect for injuries based on median trend in 'law' countries are as follows.

	Numbers		Rates	
	Law effect	Standard error	Law effect	Standard error
Car users	+ 2.4%	6.3%	+ 11.2%	11.8%
Pedestrians	+ 7.5	3.7	+ 10.7	2.1
Two-wheeler users	0.0	4.3	+ 10.6	7.2
Others	+13.4	10.8	+ 12.1	4.9

21. The predominance of positive effects (increased numbers of injuries) is alarming, although only two are 'significant', those for pedestrians and (marginally) others (using rates, but not using numbers). These are respectively approximately 5 and 2½ times their standard errors,. though it should be remembered that, because the assumptions of the model are unlikely to be correct, the calculated standard errors

are lower limits to the actual uncertainty in the estimated law effects.

22. The fitted curves (solid in Figures 9 and 10) provide a fit which does not appear much more satisfactory visually than exponential curves fitted without allowing a jump (dashed curves in Figures 9 and 10). The low standard error, for the estimated law effect in the case of pedestrians, arises from the particularly snug fit to the medians; but this is apparently fortuitous in view of the scatter (see quartiles in Figure 9). However, since pedestrians account for 20% of casualties in Britain, and the law effect seems to be significant for them, closer scrutiny is called for. i.e. significantly more pedestrians injured.

23. Pedestrian injury rates for individual 'law' countries are shown in Figure 11. The curves obviously bunch very closely. Figure 12 shows that, compared with the 'law' group, the trend in Britain and Italy lagged behind at the beginning of the 1970s; Italy had caught up by 1977 but Britain continued to lag behind. These differences are unlikely to have much to do with seat belts. There does not seem to be evidence here that 'law' countries suffer much heavier pedestrian casualty rates in comparison with trends in 'no law' countries.

24. Repeating the analysis for each country separately, we have the following results.

all positive

	Law year	Law effect	Standard error
Finland	1973	+ 9.4%	10.8%
Spain	1974	+ 3.3	5.2
Belgium	1975	+ 8.5	6.0
Netherlands	1975	+ 9.3	8.2
Norway	1975	+29.4	16.4
Sweden	1975	+ 7.1	8.0
Denmark	1976	+ 6.8	4.9
Germany	1976	+ 8.2	4.4
Lower quartile		+ 7.0	
Median		+ 8.4	
Upper quartile		+ 9.4	

Although none of the results is individually significant, it is remarkable that the estimated effects are all positive, whether the law was introduced before, during, or after the fuel crisis. Under the hypothesis that a seat belt law has no effect on the number of pedestrians injured, and assuming the underlying trend is indeed exponential, the prior probability of obtaining eight positive results is 1 in 2^8, or 1 in 256.

25. Applying the effects in para 20 to GB 1979 injury figures (serious plus slight) would give the implied net increase in injuries resulting from a seat belt law as follows.

	Based on numbers		Based on rates	
	Effect	Standard error	Effect	Standard error
Car users	+ 3,530	9,266	+ 16,473	17,356
Pedestrians	+ 4,845	2,390	+ 6,912	1,357
Two-wheeler users	0	3,841	+ 9,468	6,431
Others	+ 3,640	2,934	+ 3,287	1,331
All road users	+12,015	10,721	+ 36,140	18,606

The net effect based on rates, which are believed to provide the more reliable estimates, is about twice its standard error, and marginally significantly different from zero. It differs by nearly three standard deviations from a reduction of about 5%, estimated in para 19 above as the effect which European data might have been expected to show.

26. It was argued in para 22 that the estimated standard error for the law effect in the case of pedestrians was misleadingly low. Rather than claiming an effect on pedestrians only, it would be more reasonable to infer from the effects in para 20 based on rates, which are believed to be more reliable, that the data show a constant effect for all road users of +10.9% (with standard error 1.8%). This result is counter-intuitive. One might expect, for example, a reduction for car users, but (under the migration hypothesis) an increase for pedestrians and two-wheeler users, and no change for other road users. An 11% increase for all road users is suggestive

of a departure in the trend from the exponential model rather than a law effect.

27. Thus the data can be explained by either of the following possibilities.

(1) The effect is real, and due to the seat belt law.

(2) The effect is not real, because the exponential trend model is incorrect. None of the plots shows a convincing jump in casualty rates at the law year. Rather, they suggest that a jump is produced by the model because of the constraint that the exponential decline after the law must be at the same rate as before.

28. A country which introduced safety measures each year, each reducing casualty rates by a fixed percentage, would achieve exponentially declining casualty rates. However, if earlier measures were more effective than later ones - either because the potentially more productive measures are taken first, or because there is some irreducible minimum below which, however many safety measures are taken, it is not possible for casualty rates to fall - the rate of decline would slow down more quickly than predicted by the exponential model. A similar effect would be observed if reporting of injuries became more complete in later years. One could allow for this in the model by, for example, fitting a quadratic instead of a linear trend for log R_t. However, it would be too arbitrary to do so without evidence from 'no law' countries that this was indeed the way casualty rates behaved. The two 'no law' countries considered here are unlikely to be sufficient to provide this justification. However, the fact that pedestrian injury rates in Britain and Italy have not fallen faster than in 'law' countries at least weakens the evidence for a law effect.

29. It is therefore felt that a more detailed analysis, using a better and more extensive data base and more complicated models, would be needed to determine whether there is a real increase in injuries following a seat belt law, or whether there is no discernable effect. (It does not seem likely that it would show a significant reduction, to judge from European data.)

30. CONCLUSIONS

It is difficult to assess the effect of seat belt legislation in countries which legislated within a few years of the fuel crisis in 1974, particularly with few comparable data for countries without a seat belt law. A simple model suggests no change in death rates, and an 11% (± 2%) increase in injuries for all classes of road user, to have been the effect of the law. However, comparison with two 'no law' countries - and common sense - suggest that this increase results from the model being too simple, and that there is no significant law effect. A larger data base would be needed to test a more realistic model.

31. This 'no effect' conclusion appears to be at variance with the Department's estimates (for front seat vehicle occupants only). However, these related to voluntary wearing. It is perhaps conceivable that the estimated savings might be realised if voluntary wearing rose to 100%, but it is hardly possible to verify this.

Significance of this qualification not clear.

An odd conclusion. Bears no relation to evidence reviewed. Says, in effect, that anything is conceivable.

General comment: The calculations based on rates suggest a perverse effect for both deaths and injuries. This effect is not statistically significant for deaths and only marginally significant for injuries.

If reductions of this magnitude had been found for seat belts, I suspect the conclusion would have been much less tentative. The text contains evidence of a reluctance to accept the result e.g. p. 9. "alarming"!

Figure 1: Car user deaths (1970 = 100), 'law' ...

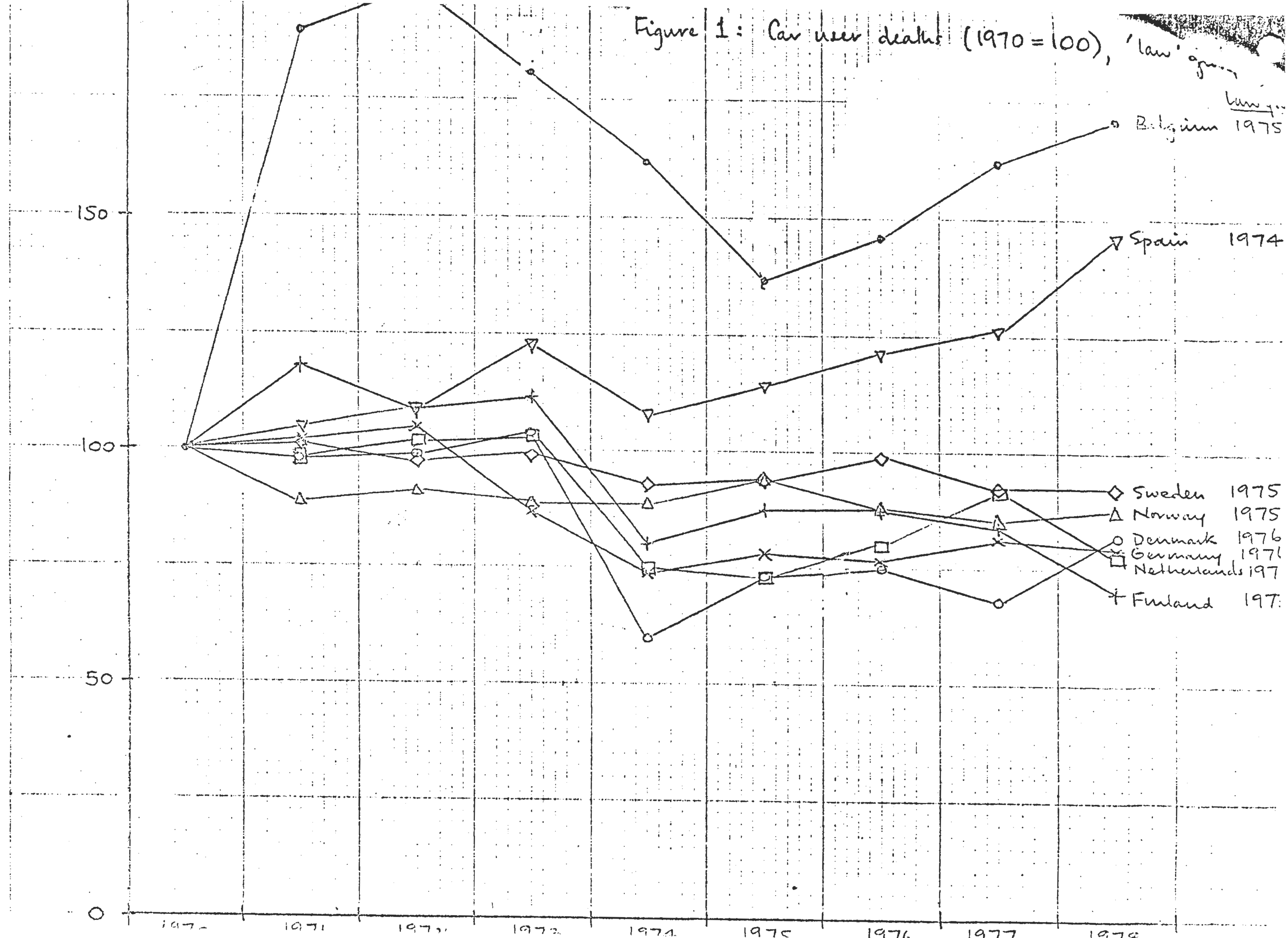


Figure 2: Car user deaths (1970 = 100)

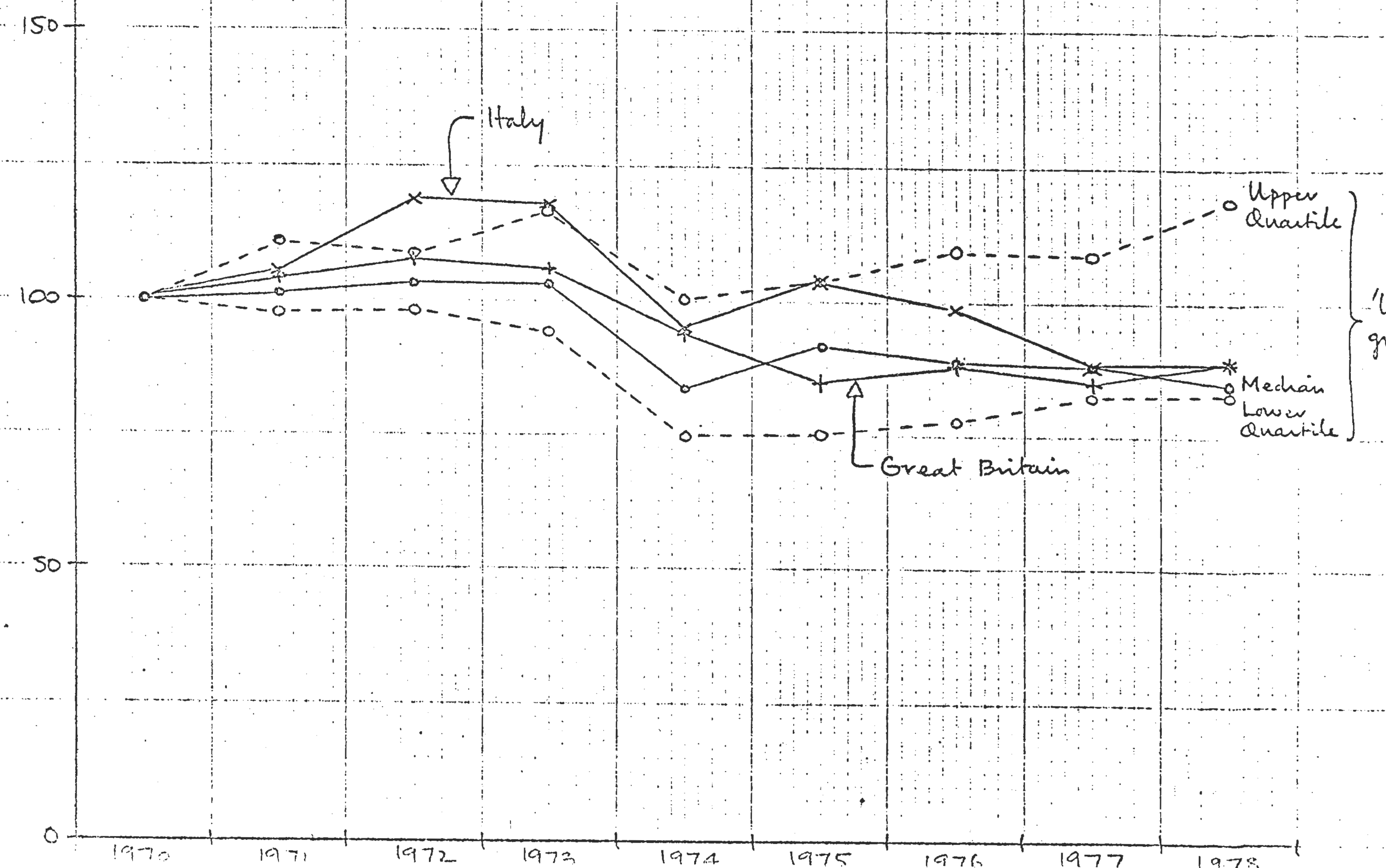


Figure 3: Car user deaths, 'law' group (law year -3 = 100)

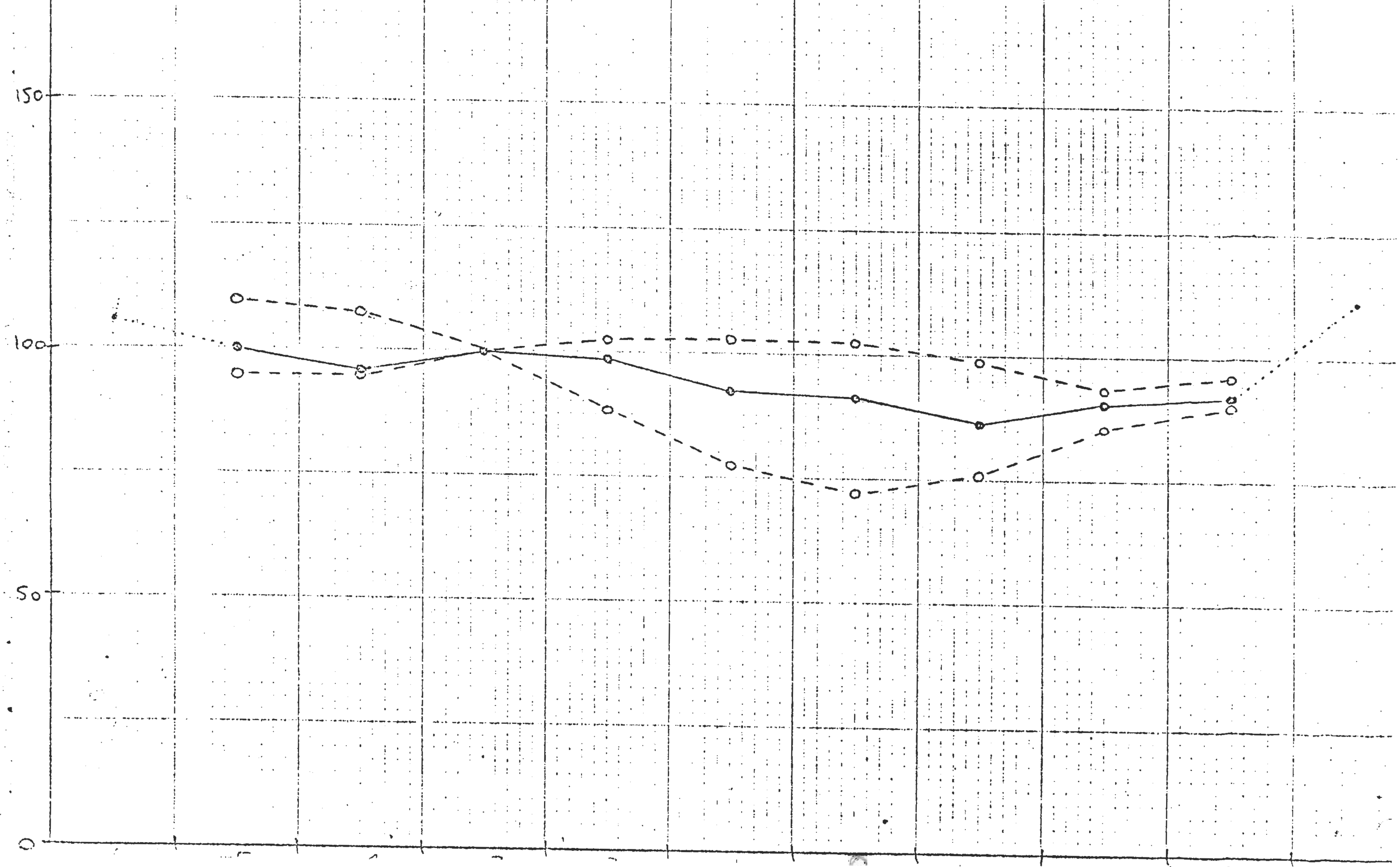


Figure 4: Car user deaths, 'law group' (law year -3 = 100)

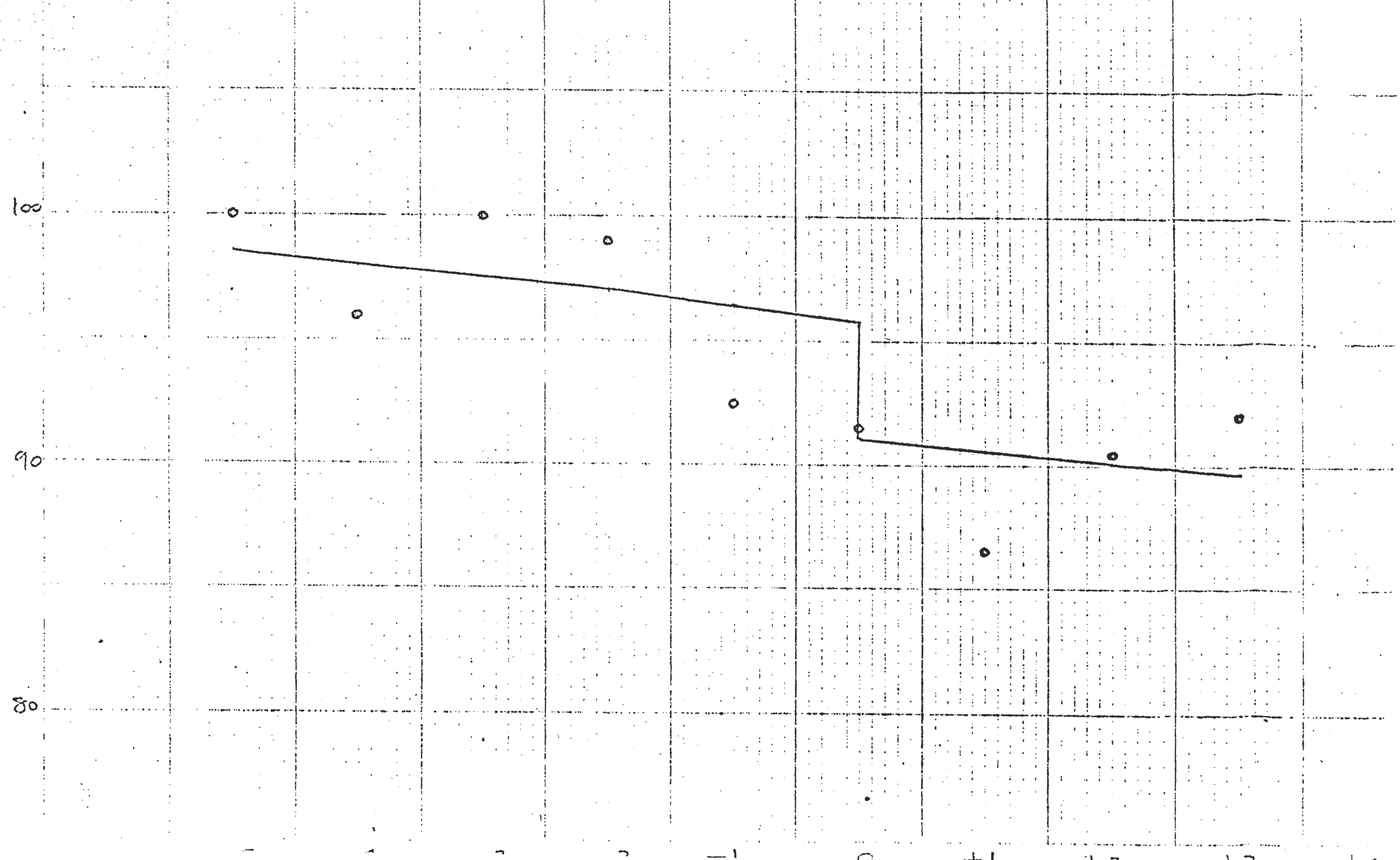


Figure 5: Car user death rates (1970=100), 'law' group

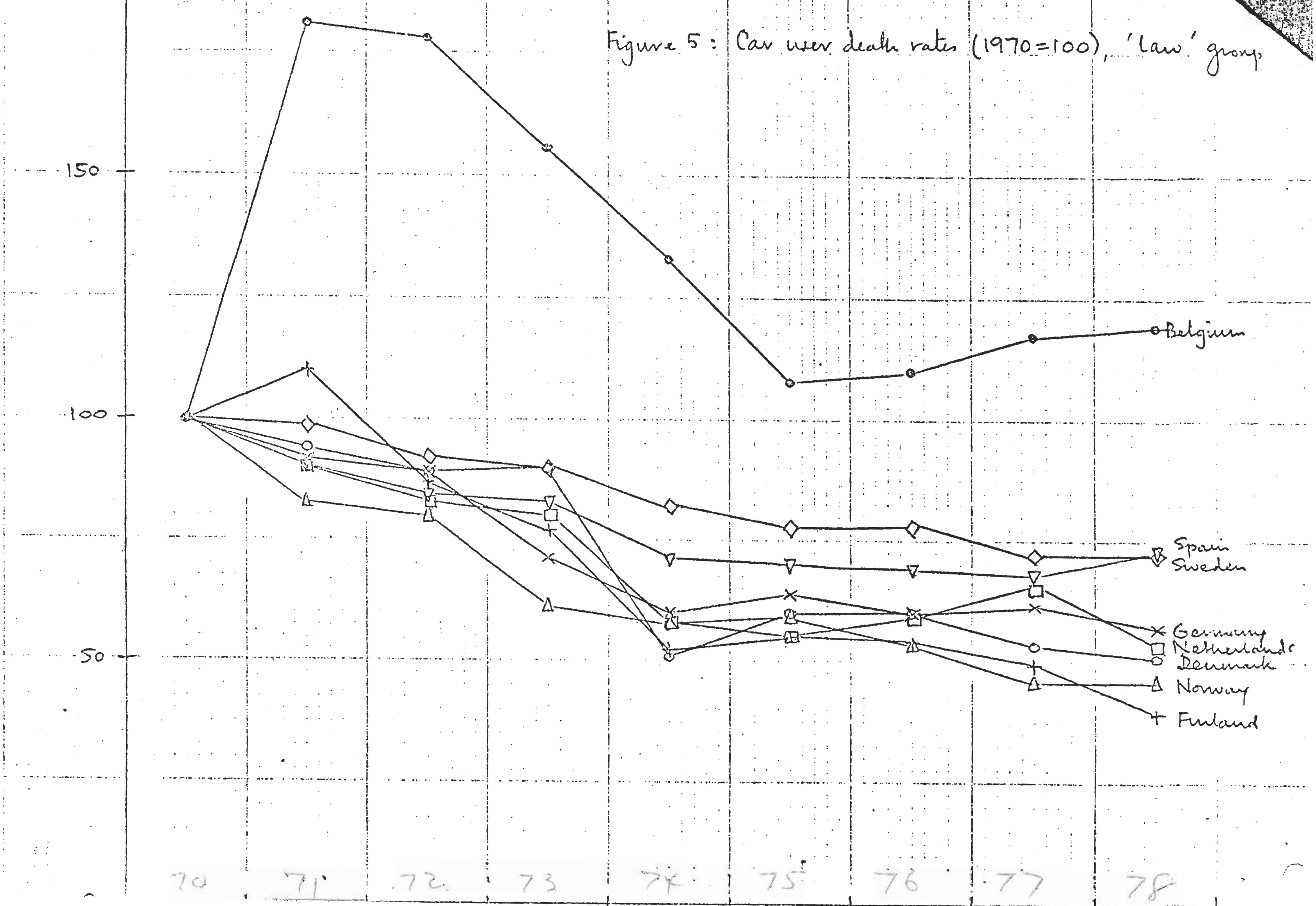


Figure 6: Car user death rates (1970 = 100)

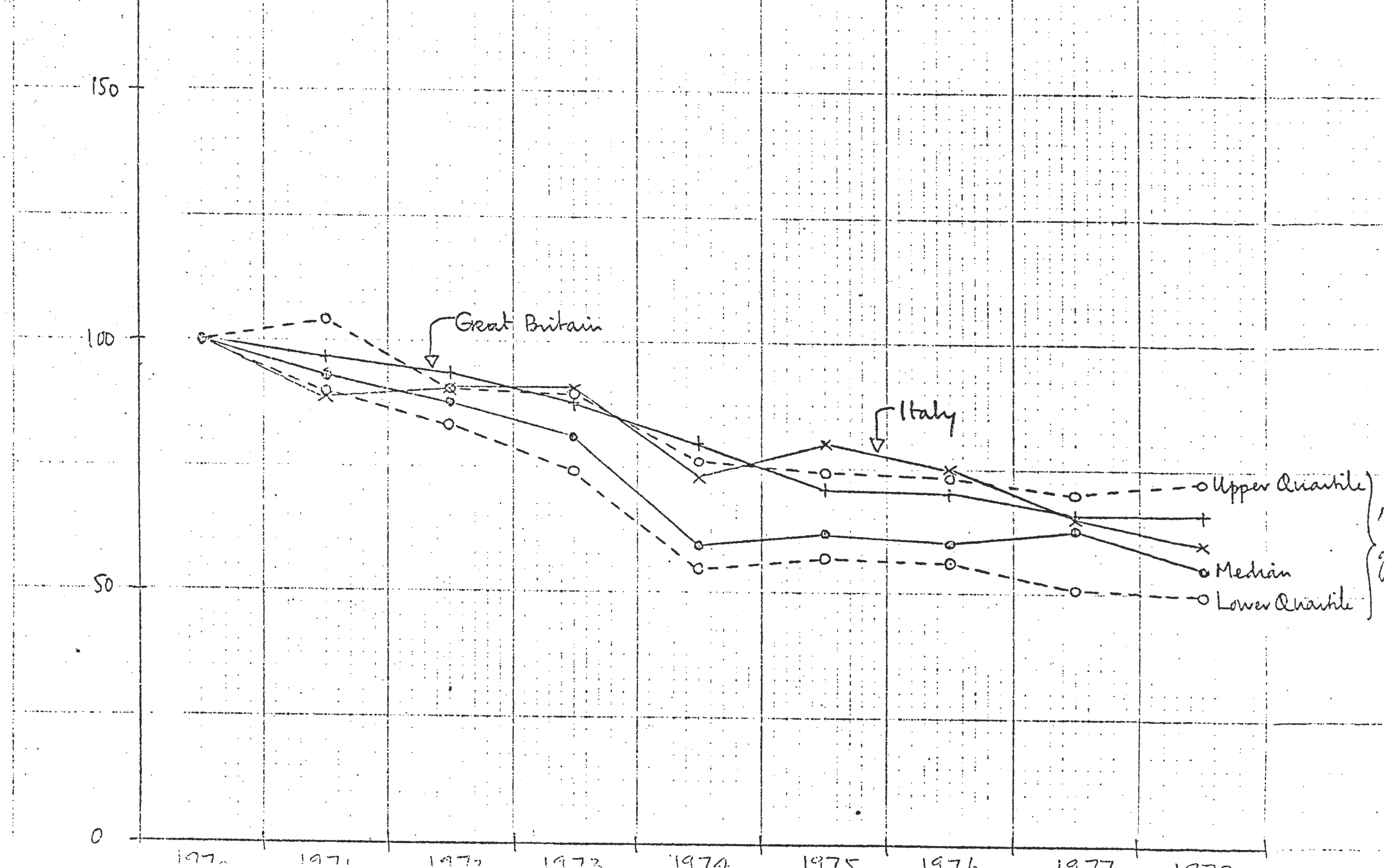


Figure 7: Car user death rates, 'law' group (law year - 3 = 100)

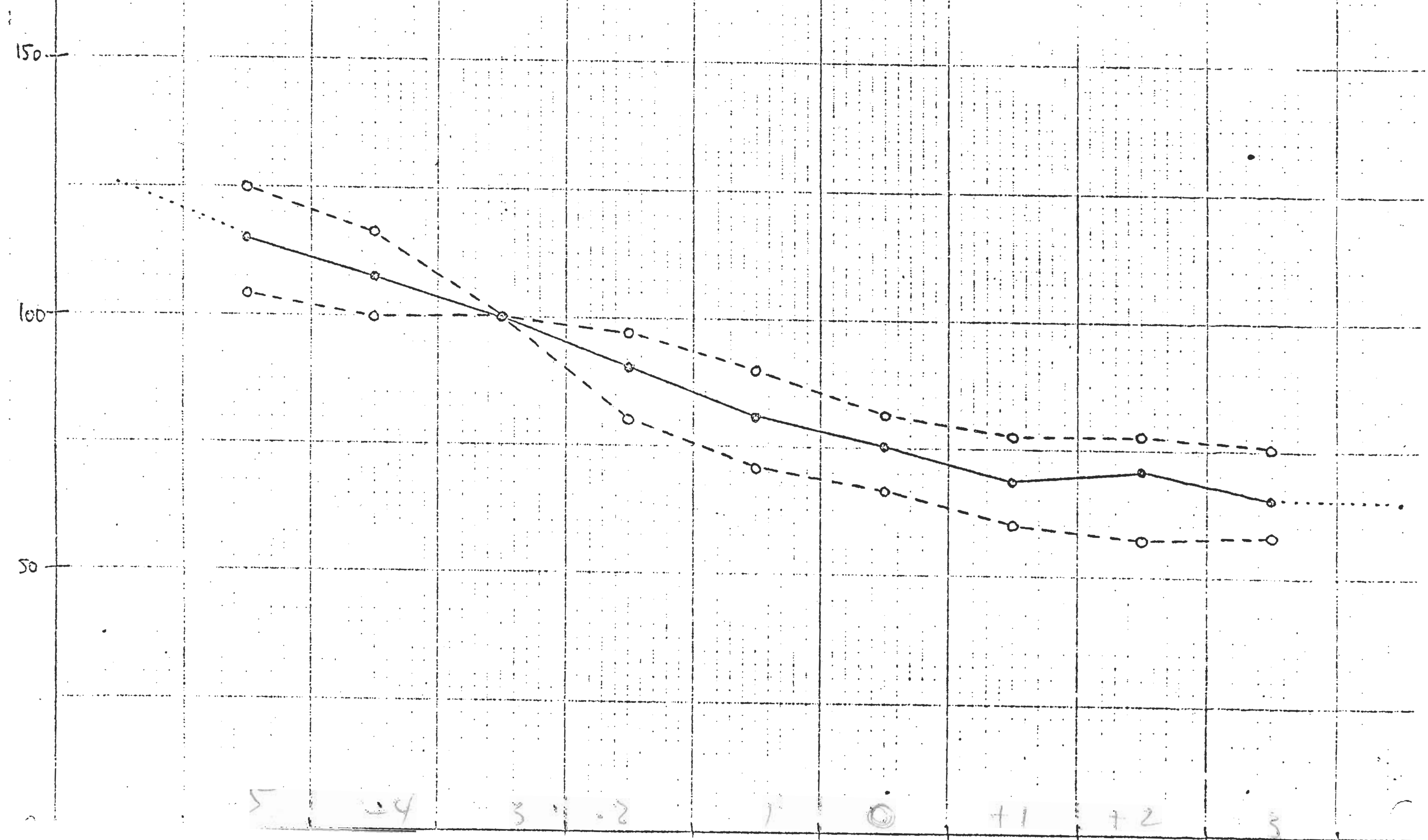


Figure 8: Car user death rates, 'law' group (law year - 3 = 100)

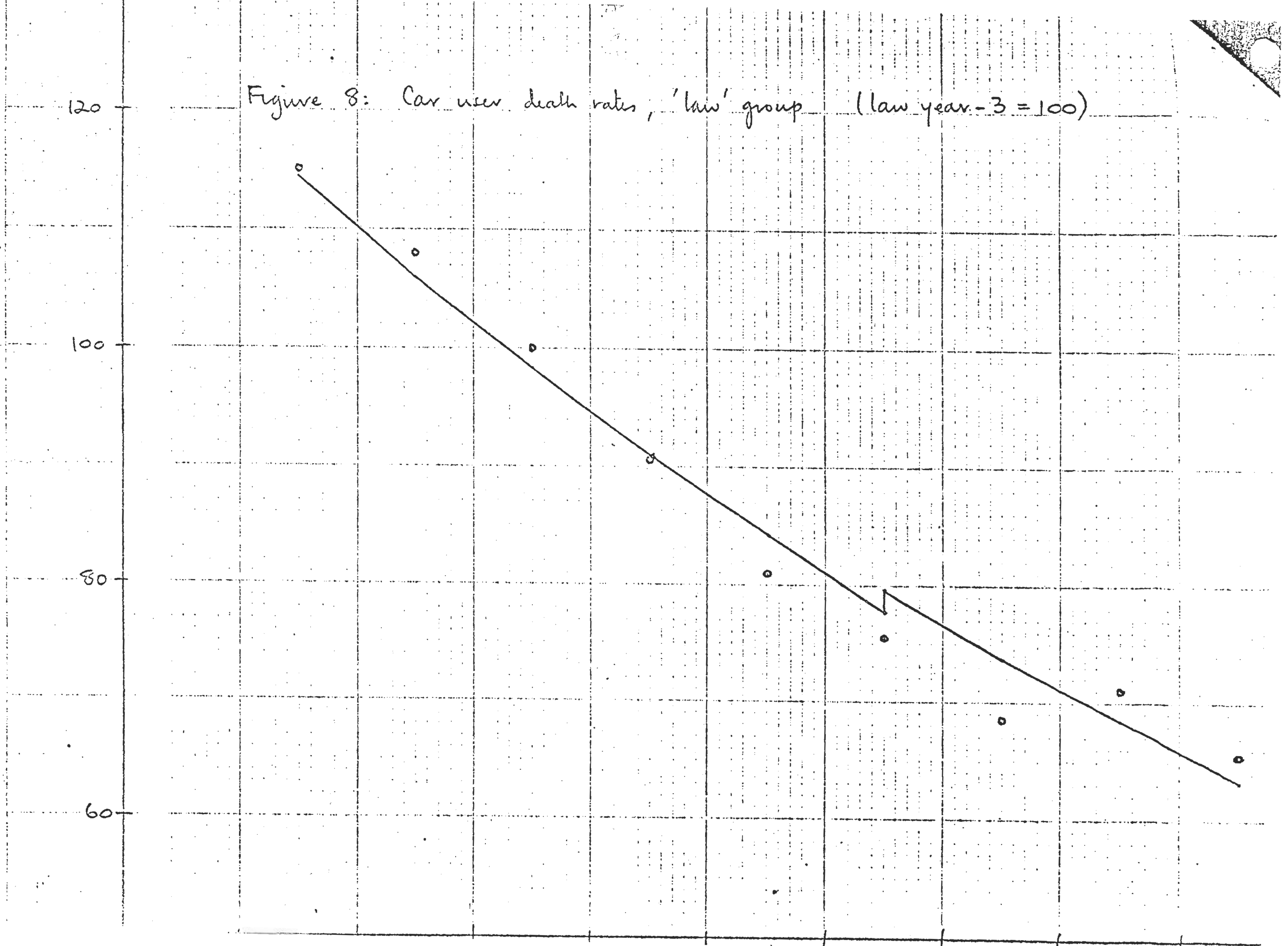


Figure 9: Pedestrian injury rates, 'law' group. (law year - 3 = 100)

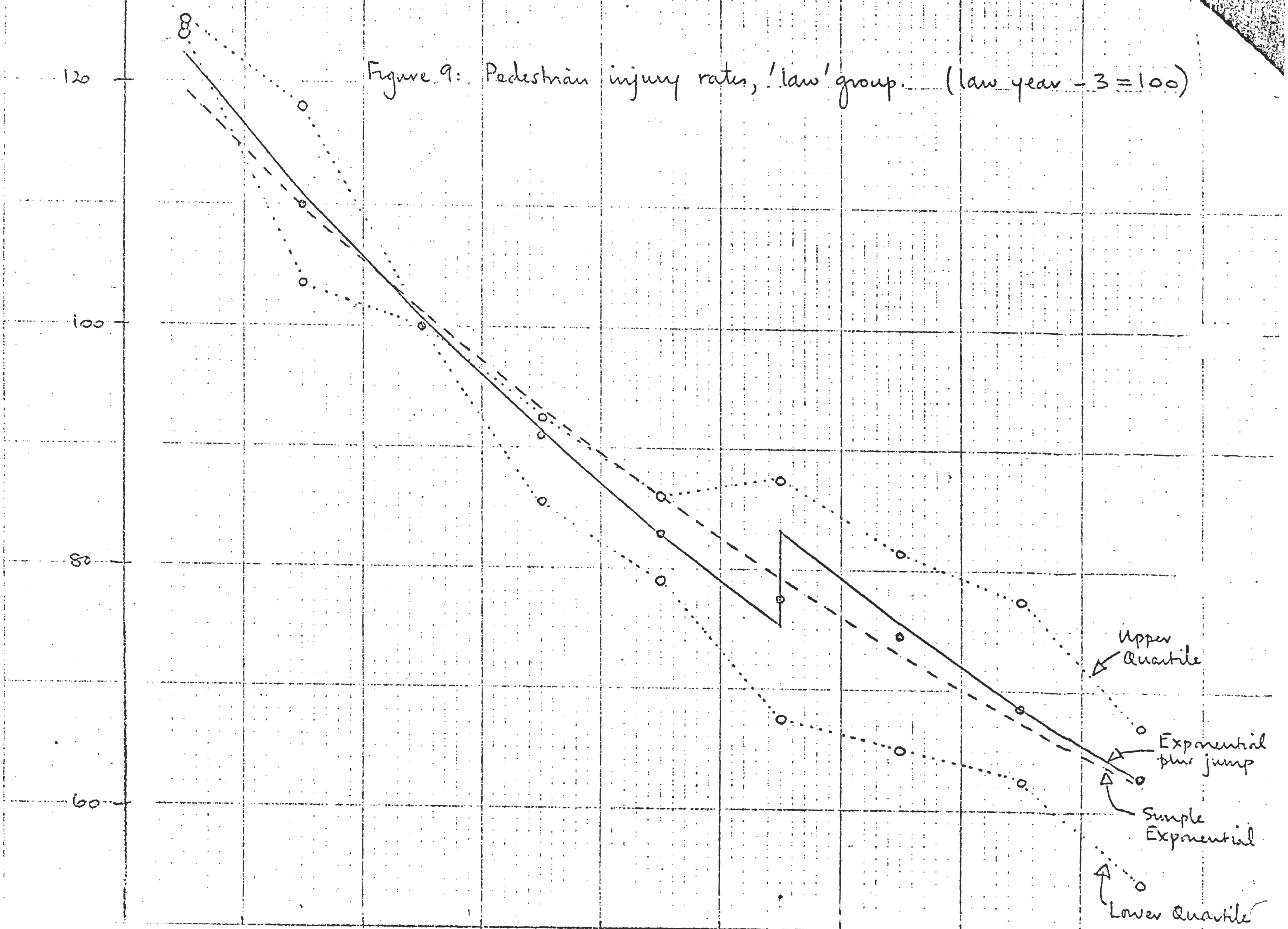


Figure 10: Other road user injury rates group. (law year -3 = 100

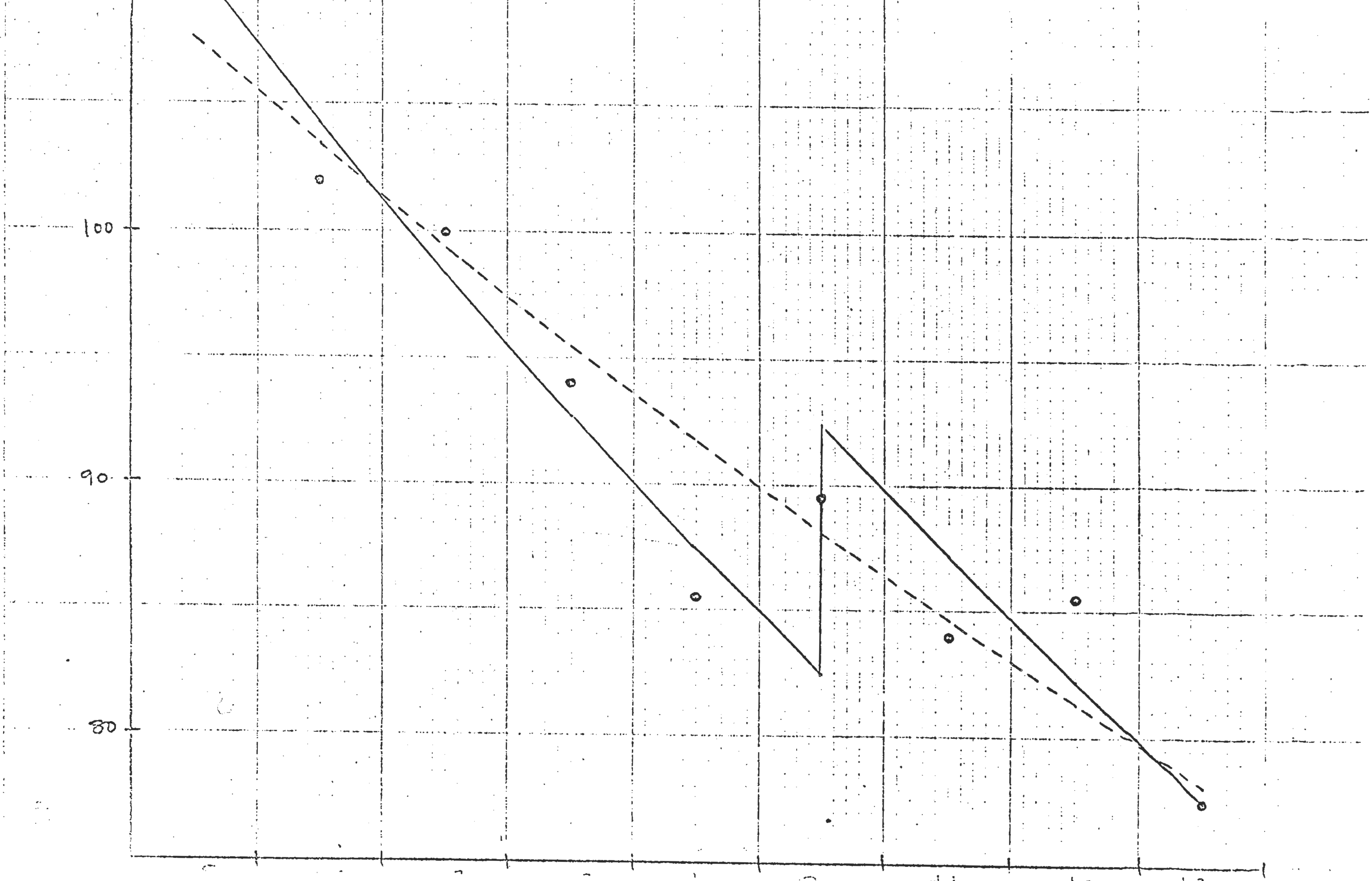


Figure 11: Pedestrian injury rates (1970 = 100), 'law' group

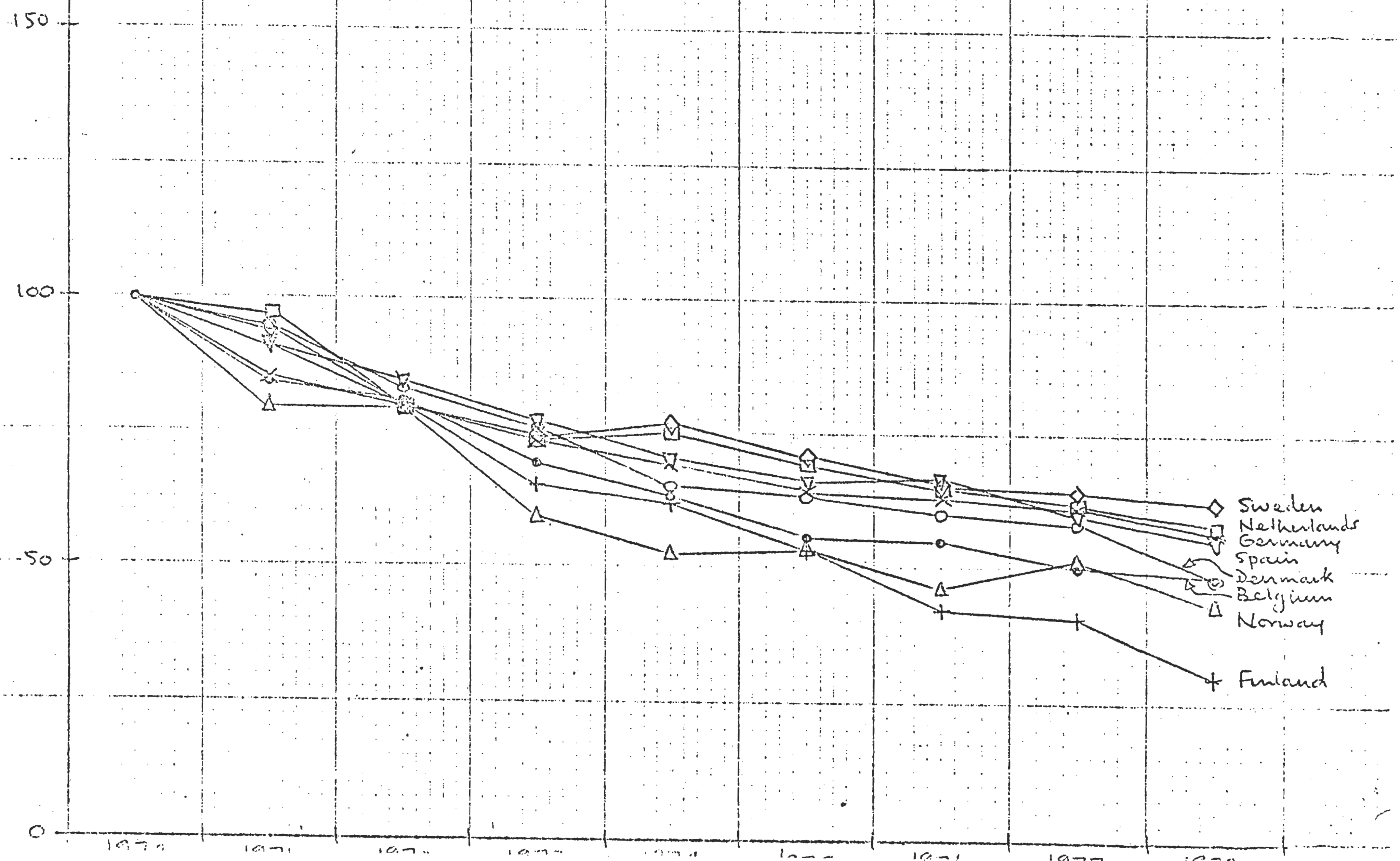
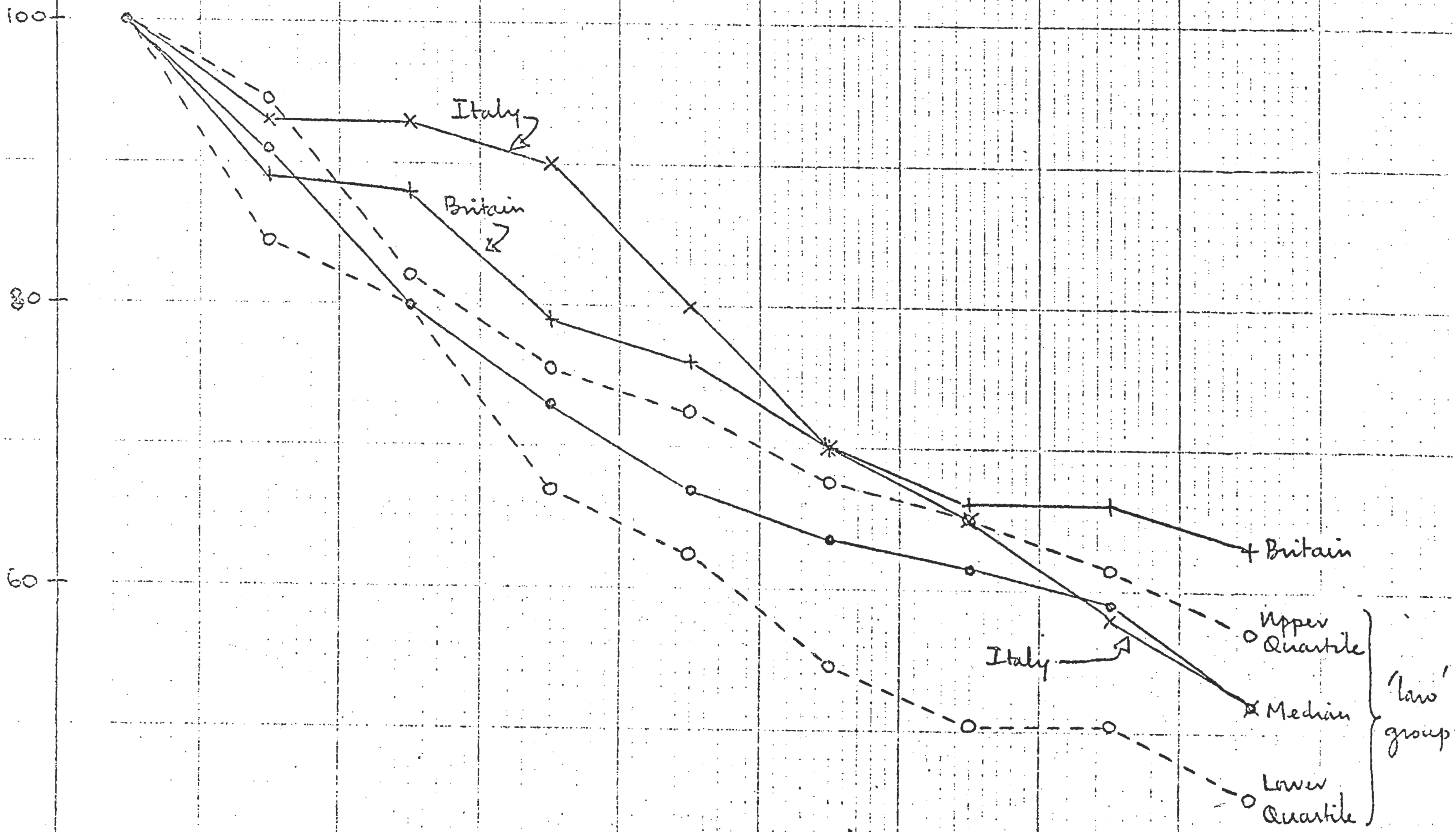


Figure 12: Pedestrian injury rates (1970=100).



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J E ISLES

STG DIVISION (Dept of Transport)

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31. This 'no effect' conclusion appears to be at variance with the Department's estimates (for front seat vehicle occupants only). However, these related to voluntary wearing. It is perhaps conceivable that the estimated savings might be realised if voluntary wearing rose to 100%, but it is hardly possible to verify this.