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Speed and road accidents

an evaluation of the power model

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Preface

The relationship between speed and road safety is a highly controversial and emotionally charged subject. This report investigates the effects of changes in speed on the number of road accidents or road accident victims. It is found that the relationship between speed and accidents or accident victims can be represented by a set of power functions, as postulated in the so called “Power Model” of the relationship between speed and road safety.

The study was funded by the Swedish National Road Administration and the Norwegian Public Roads Administration. Project manager at the Swedish National Road Administration was first Jan Ifver, then Thomas Lekander. Project manager at the Norwegian Public Roads Administration was Finn Harald Amundsen.

The principal author of this report is chief researcher Rune Elvik, who was project manager at the Institute of Transport Economics. Senior research statistician Peter Christensen performed the meta-analyses presented in the report. Research geographer Astrid Amundsen retrieved the studies used in the report and coded the data used in the study.

Comments to drafts of this report have been given by professor (emeritus) Ezra Hauer, University of Toronto, professor Richard Allsop, University College London, professor Christer Hydén, Lund Institute of Technology, and colleagues at the Institute of Transport Economics. Head of department Marika Kolbenstvedt has been responsible for formal quality assurance. Secretary Trude Rømming did the final editing of the manuscript.

Oslo, December 2004
Institute of Transport Economics

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Authors: Rune Elvik, Peter Christensen, Astrid Amundsen
Oslo 2004, 134 pages English language

Summary:

Speed and road accidents: an evaluation of the Power Model

The relationship between speed and road safety is a controversial topic. In this report, the relationship between speed and road safety has been evaluated by means of a meta-analysis of studies that provide estimates of how changes in speed affect the number of road accidents and the number and severity of injuries to road users.

The Power Model

This study was designed to evaluate the Power Model of the relationship between speed and road safety. This model has been proposed by the Swedish road safety researcher Göran Nilsson. According to the Power Model, the effects of changes in speed on the number of accidents and the severity of injuries can be estimated by means of a set of power functions.

A power function is a mathematical function that relates two variables to each other by raising values of one of the variables to a power in order to obtain values for the other variable. Any function in which a variable is raised to a certain exponent is called a power function (not to be mixed up with an exponential function, which is e (e = 2.71828) raised to an exponent). The Power Model describes the relationship between speed and road safety in terms of six equations. As an example, the equation referring to fatal accidents is:

$$\frac{\text{Fatal accidents after}}{\text{Fatal accidents before}} = \left(\frac{\text{Speed after}}{\text{Speed before}}\right)^4$$

If speed is reduced from 100 km/h to 90 km/h, the ratio speed after/speed before equals 90/100 = 0.9. Raising 0.9 to a power of 4 gives (0.9 · 0.9 · 0.9 · 0.9) 0.656. This means that the number of fatal accidents is estimated to go down to 0.656 times the initial number, corresponding to a reduction of 34.4 percent.

The Power Model consists of one equation for fatalities, one for fatal and serious injuries and one for all injured road users. Moreover, there is one equation for fatal accidents, one for accidents involving fatal or serious injury, and one for all injury accidents. An exponent of 4 is proposed for fatal accidents, an exponent of 3 for accidents involving fatal or serious injury, and an exponent of 2 for all injury accidents. For fatalities an exponent larger than 4, but smaller than 8 is proposed.

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For fatal and serious injuries, the exponent is between 3 and 6. For all injured road users, the exponent is between 2 and 4. Changes in the number of accidents or accident victims are modelled as a function of the relative change in the mean speed of traffic.

The Power Model has been widely employed to estimate the expected effects of changes in speed. The objective of the research presented in this report was to evaluate the validity of the model by means of a systematic review and meta-analysis of relevant studies.

Systematic literature search and meta-analysis

A systematic search for relevant studies was made by accessing the TRANSPORT literature database. "Speed and accidents" was used as search terms. A total of 1.469 entries were found. The computer search was supplemented by a manual search of selected scientific journals and previous reviews of the relationship between speed and road safety. A total of 175 studies were identified as relevant. The results of these studies were summarised by means of a meta-analysis. To be included in the meta-analysis, a study had to state the relative change in speed and the relative change in the number of accidents or accident victims. 98 studies, containing 460 estimates of the effects of changes in speed were included in the meta-analysis. 77 studies identified as relevant could not be included in the meta-analysis, mostly because they did not report the information needed.

Summary estimates of exponents were developed by means of meta-analysis. These analyses were performed by means of traditional techniques as well as techniques for meta-regression (multivariate models). Six models were developed. In addition, several versions of these models were employed in sensitivity analyses. The possible presence of publication bias was tested for by means of the trim-and-fill technique.

Results and interpretation of them

The results give clear support to the Power Model. The values of the exponents are not perfectly identical to those proposed by the Power Model, but they are close to them and exhibit a pattern that conforms to the Power Model.

The Power Model, as stated, contains an element of inconsistency. To explain this, consider the following. The exponent for fatal accidents is 4. The exponent for accidents involving fatal or serious injury is 3. The exponent for all injury accidents, including fatal accidents, is 2. Thus, fatal accidents are represented by an exponent of 4 when considered exclusively, but by an exponent of 3 when merged with serious injury accidents, and an exponent of 2 when merged with all injury accidents. The exponents of 4, 3 and 2 cannot all be true at the same time for the same category of accidents. The Power Model was therefore reformulated, so that the various levels of accident- or injury severity do not overlap, but are treated as mutually exclusive categories. The following exponents are the best estimates for the modified version of the Power Model:

	Best estimate of	95% confidence

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Accident or injury severity	exponent	interval
Fatalities	4.5	(4.1 – 4.9)
Seriously injured road user	3.0	(2.2 – 3.8)
Slightly injured road user	1.5	(1.0 – 2.0)
All injured road users (severity not stated)	2.7	(0.9 – 4.5)
Fatal accidents	3.6	(2.4 – 4.8)
Serious injury accidents	2.4	(1.1 – 3.7)
Slight injury accidents	1.2	(0.1 – 2.3)
All injury accidents (severity not stated)	2.0	(1.3 – 2.7)
Property-damage-only accidents	1.0	(0.2 – 1.8)

Source: TØI report 740/2004

These results show that there is a strong statistical association between speed and road safety. As an example, it can be estimated that a 10 percent reduction in the mean speed of traffic will result in a 37.8 reduction of the number of fatalities.

The results show the statistical relationship between speed and road safety. Correlation does not necessarily imply causation. Is there a causal relationship between changes in speed and changes in road safety? The report concludes that the relationship is indeed causal. This is based on the following arguments:

1. There is a very strong statistical relationship between speed and road safety. It is difficult to think of any other risk factor that has a more powerful impact on accidents or injuries than speed.
2. The statistical relationship between speed and road safety is very consistent. When speed goes down, the number of accidents or injured road users also goes down in 95% of the cases. When speed goes up, the number of accidents or injured road users goes up in 71% of the cases. While it may to some extent be possible to offset the impacts of higher speed by introducing other road safety measures, a reduction in speed will almost always improve road safety.
3. The causal direction between speed and road safety is clear. Most of the evidence reviewed in this report comes from before-and-after studies, in which there can be no doubt about the fact that the cause comes before the effect in time.
4. The relationship between speed and road safety holds up when potentially confounding factors are controlled for. There is no evidence of a weaker relationship between speed and road safety in well-controlled studies than in less well-controlled studies.
5. There is a clear dose-response relationship between changes in speed and changes in road safety. The larger the change in speed, the larger the impact on accidents or accident victims.

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6. The relationship between speed and road safety appears to hold universally and is not influenced by, for example, the country in which it has been evaluated, when it was evaluated or the type of traffic environment in which it was evaluated.
7. The relationship between speed and road safety can be explained in terms of elementary laws of physics. These laws of physics determine the stopping distance of a vehicle and the amount of energy released when an impact occurs.

It is concluded that there is a law-like and causal relationship between speed and road safety. This relationship is adequately described by means of the Power Model.

Some limitations of the study

The study has a number of limitations. The most important of these can be summarised as follows:

1. A fairly high proportion of the relevant studies, 77 out of 175, could not be included in the meta-analysis. An assessment has been made of whether exclusion of these studies has influenced the results of the study. It is concluded that exclusion of these studies from the meta-analysis is unlikely to have affected its results materially.
2. There is a possibility of publication bias in the data, meaning that studies that are regarded as useless, or whose findings are difficult to interpret, are less likely to be published than other studies. A formal tests for publication bias was conducted and no evidence of it was found.
3. The results may to some extent reflect the effects of other road safety measures, not just changes in speed. This is true as far as individual studies are concerned. It is, however, not true as far as the results of the meta-analyses are concerned. In these studies, the effects of other road safety measures were controlled for statistically by means of multivariate analyses. Hence, the summary estimates of power show the effects of speed only.
4. Data concerning speed and/or accidents can be unreliable. This is obviously correct. However, the impact of unreliable data is always to attenuate statistical relationships, never to reinforce them. It is therefore highly likely that the true effects of speed on road safety are underestimated in this study.
5. A number of studies contain multiple estimates of effect. If these estimates are statistically dependent on each other, variance is reduced and a spuriously strong relationship between speed and road safety can be found. The variance of study findings was assessed, and no evidence of any within-study statistical dependency was found.

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6. The study does not state what the relationship between speed and accidents is for specific types of accidents or in specific types of traffic environment. Unfortunately, the data did not allow the relationship between speed and accidents to be estimated for specific types of accidents. As far as different types of traffic environment are concerned, the analyses gave no indication of any differences with respect to the impacts of speed on road safety.
7. The study has evaluated the Power Model only. Very many other models can be imagined to summarise the relationship between speed and road safety. Only two alternative models have been examined. One of these is a linear model, according to which it is the absolute change in speed, not the relative change, that produces changes in road safety. The other model is a logistic model, according to which the effects of changes in speed depend on the initial level of speed. The linear model is highly implausible. The logistic model is somewhat more plausible, but the data did not permit it to be tested in a sufficiently stringent manner. It is concluded that the Power Model is to be preferred to other models because of its generality and simplicity.

The overall conclusion is that the limitations of the study are unlikely to have influenced its findings.

Practical implications

Speed has been found to have a very large effect on road safety, probably larger than any other known risk factor. Speed is a risk factor for absolutely all accidents, ranging from the smallest fender-bender to fatal accidents. The effect of speed is greater for serious injury accidents and fatal accidents than for property-damage-only accidents.

If government wants to develop a road transport system in which nobody is killed or permanently injured, speed is the most important factor to regulate. The report argues that driver speed choice may not always be perfectly rational; hence, a legitimate basis exists for limiting the freedom of choice with respect to speed. The need for such regulation is very widely recognised, as nearly all motorised countries have an extensive system of speed limits and a programme of enforcement. Speed limits and their enforcement are very important road safety measures.

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Forfattere: Rune Elvik, Peter Christensen, Astrid Amundsen
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Sammendrag:

Fart og trafikkulykker: evaluering av potensmodellen

Sammenhengen mellom fart og trafikksikkerhet er et omdiskutert tema. I denne rapporten er sammenhengen mellom fart og trafikkulykker studert ved hjelp av en meta-analyse av et stort antall undersøkelser som gir anslag på hvordan endringer i fart virker på antall trafikkulykker og på alvorligheten av personskader i trafikkulykker.

Potensmodellen

Utgangspunktet for undersøkelsen er den såkalte potensmodellen av sammenhengen mellom fart og trafikksikkerhet. Denne modellen er foreslått av den svenske trafikksikkerhetsforskeren Göran Nilsson. Potensmodellen beskriver sammenhengen mellom endringer i fart og endringer i ulykker, eller skadde eller drepte, i form av et sett av potensfunksjoner.

En potensfunksjon er en sammenheng mellom to variabler som fremkommer ved at verdier på den ene variabelen kan beregnes ved å opphøye den andre variabelen i en eksponent. Enhver funksjon der en variabel opphøyes i en eksponent kalles for en potensfunksjon. Potensmodellen for sammenhengen mellom endringer i fart og endringer i ulykker, eller skadde eller drepte, består av seks likninger. For eksempel er ligningen for dødsulykker:

$$\frac{\text{Dødsulykker etter}}{\text{Dødsulykker før}} = \left(\frac{\text{Fart etter}}{\text{Fart før}}\right)^4$$

Dersom farten eksempelvis reduseres fra 100 km/t til 90 km/t, er verdien av fart etter/fart før lik 90/100 = 0,9. Når 0,9 opphøyes i fjerde potens (0,9 · 0,9 · 0,9 · 0,9) får vi 0,656. Det vil si at antallet dødsulykker da forventes å bli redusert til 0,656 av opprinnelig verdi, altså en reduksjon på 34,4 prosent.

I potensmodellen foreslås en ligning for drepte, en for drepte og alvorlig skadde og en for alle skadde eller drepte. Videre foreslås en ligning for dødsulykker, en for ulykker med drepte eller alvorlig skadde og en for alle personskadeulykker. Det foreslås en eksponent på 4 for dødsulykker, 3 for ulykker med drepte eller alvorlig skadde og 2 for alle personskadeulykker. For drepte foreslås en eksponent som er større enn 4, men mindre enn 8. For drepte eller alvorlig skadde foreslås en eksponent mellom 3 og 6 og for alle skadde eller drepte foreslås enn eksponent

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mellom 2 og 4. Endringene i ulykker eller skadde eller drepte beskrives i alle ligninger som en funksjon av den relative endringen i trafikkens gjennomsnittsfart.

Potensmodellen har vært mye brukt til å beregne forventede virkninger av endringer i fart. Formålet med denne studien var å evaluere modellens gyldighet ved hjelp av en systematisk gjennomgang og oppsummering av relevante undersøkelser.

Systematisk litteratursøk og meta-analyse

Det ble gjennomført et litteratursøk i den bibliografiske databasen TRANSPORT, med ”fart og ulykker” som søkeord. Søket ga 1.469 treff. Søket ble supplert med manuell gjennomgang av utvalgte tidsskrifter og tidligere litteraturstudier om fart og ulykker. Undersøkelsene ble sortert etter relevans og 175 studier ble bedømt som relevante. Resultatene av de relevante studiene er sammenfattet ved hjelp av meta-analyse. For å inngå i meta-analysen måtte en undersøkelse gi opplysninger om relativ endring av fart og relativ endring av ulykker eller skadde eller drepte. 98 undersøkelser med til sammen 460 resultater inngikk i meta-analysen. 77 av de relevante undersøkelsene kunne av ulike grunner, primært at de ikke ga alle nødvendige opplysninger, ikke inngå i meta-analysen.

I meta-analysen er resultatene av de ulike undersøkelsene veid sammen til gjennomsnittsresultater. Dette ble gjort både ved å benytte tradisjonelle teknikker for meta-analyse og ved å utføre meta-regresjon (multivariat analyse). I alt ble seks ulike modeller utviklet. I tillegg ble ulike varianter av disse benyttet til følsomhetsanalyser. Det er testet for publikasjonsskjevhet med ”trim-and-fill” metoden.

Resultater og tolkning av resultatene

Resultatene av meta-analysen gir klar støtte til potensmodellen. Verdiene av eksponentene er ikke nøyaktig lik dem som foreslås i potensmodellen, men viser et mønster som er i samsvar med den.

Potensmodellen inneholder, slik den er formulert, et element av inkonsistens. Dette kan forklares slik. Eksponenten for dødsulykker alene er 4. Når dødsulykker slås sammen med ulykker med alvorlige personskader, er eksponenten 3. Når dødsulykker slås sammen med alle personskadeulykker, er eksponenten 2. Dødsulykker er følgelig representert ved en eksponent på enten 4, 3 eller 2, avhengig av om de betraktes isolert eller i sammenheng med andre ulykker. For å unngå denne inkonsistensen, er potensmodellen i rapporten reformulert, slik at de ulike nivåene for ulykkers eller skaders alvorlighetsgrad blir gjensidig utelukkende. Følgende eksponenter sammenfatter resultatene av undersøkelsen med henvisning til den reformulerte versjonen av potensmodellen:

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Skadegrad	Beste anslag på eksponenten	95% konfidens-intervall
Drepte	4,5	(4,1 – 4,9)
Alvorlig skadde	3,0	(2,2 – 3,8)
Lettere skadde	1,5	(1,0 – 2,0)
Alle skadde (uspesifisert skadegrad)	2,7	(0,9 – 4,5)
Dødsulykker	3,6	(2,4 – 4,8)
Ulykker med alvorlig personskade	2,4	(1,1 – 3,7)
Ulykker med lettere personskade	1,2	(0,1 – 2,3)
Alle personskadeulykker (uspesifisert)	2,0	(1,3 – 2,7)
Ulykker med kun materiell skade	1,0	(0,2 – 1,8)

Kilde: TØI rapport 740/2004

Disse resultatene viser at det er en sterk sammenheng mellom fart og trafikksikkerhet. Det kan for eksempel beregnes at en reduksjon av gjennomsnittsfarten med 10 prosent kan forventes å redusere antallet drepte med 37,8 prosent.

Resultatene viser den statistiske sammenhengen mellom fart og trafikksikkerhet. Er det også en årsakssammenheng mellom fart og trafikksikkerhet? I rapporten konkluderes det med at det er en årsakssammenheng mellom fart og ulykker. Denne konklusjonen bygger på følgende argumenter:

1. Det er en meget sterk statistisk sammenheng mellom fart og trafikksikkerhet. Knapt noen annen risikofaktor synes å ha så sterk virkning på ulykker og skader som fart.
2. Den statistiske sammenhengen mellom fart og trafikksikkerhet er helt entydig. Når farten reduseres, går antallet ulykker eller skader ned i 95% av tilfellene. Når farten øker, øker antallet ulykker eller skader i 71% av tilfellene. Økt fart kan med andre ord til en viss grad kompenseres med andre tiltak, mens lavere fart nesten alltid bedrer trafikksikkerheten.
3. Årsaksretningen mellom fart og trafikksikkerhet er entydig. Resultatene bygger i det alt vesentlige på før-og-etterundersøkelser, der det ikke hersker tvil om rekkefølgen i tid mellom endringer i fart og endringer i trafikksikkerhet.
4. Sammenhengen mellom fart og ulykker svekkes ikke når man kontrollerer for andre faktorer som påvirker trafikksikkerheten. Det er ingen tendens til at godt kontrollerte undersøkelser finner en svakere sammenheng mellom fart og ulykker enn mindre godt kontrollerte undersøkelser.
5. Det er en klar dose-responssammenheng mellom fart og trafikksikkerhet. Jo større endringer i fart, desto større endringer i trafikksikkerhet.
6. Sammenhengen mellom fart og trafikksikkerhet synes å gjelde universelt og er uavhengig av hvilket land den er undersøkt i, når undersøkelsene er gjort eller hvilken type trafikkmiljø undersøkelsene gjelder.

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7. Sammenhengen mellom fart og trafikksikkerhet lar seg forklare ved hjelp av elementære fysiske lover som bestemmer stopplengden for kjøretøy og energiutløsningen i ulykkesøyeblikket.

Det kan konkluderes med at det er en lovmessig årsakssammenheng mellom fart og trafikksikkerhet. Denne lovmessige sammenhengen kan beskrives godt ved hjelp av potensmodellen.

Svakheter og begrensninger ved undersøkelsen

Undersøkelsen har en del svakheter og begrensninger. De viktigste av disse kan oppsummeres i følgende punkter:

1. Forholdsvis mange av de relevante undersøkelsene, 77 av 175, kunne ikke inngå i meta-analysen. Det drøftes om dette kan ha påvirket resultatene. Konklusjonen er at resultatene neppe er nevneverdig påvirket av at mange undersøkelser måtte utelates fra meta-analysen.
2. Resultatene kan være påvirket av publikasjonsskjevhet, det vil si en tendens til at noen resultater sjeldnere blir publisert enn andre, for eksempel fordi de er vanskelige å tolke. Det er testet for publikasjonsskjevhet. Det ble ikke funnet tegn til at det finnes slik skjevhet i det materialet som ligger til grunn for meta-analysen.
3. Resultatene kan til en viss grad reflektere virkninger av andre trafikksikkerhetstiltak, ikke bare endringer i fart. Dette er riktig for noen av undersøkelsene. Det er likevel ikke en feilkilde i meta-analysen, fordi resultatene fremkommer ved analyser der det er kontrollert for hvilke tiltak som er iverksatt. Resultatene av meta-analysen viser derfor, ideelt sett, kun virkningene av endringer i fart.
4. Data om fart og ulykker i de enkelte undersøkelser kan være beheftet med feil. Dette er riktig, men i den grad slike feil finnes, vil de svekke sammenhengen mellom fart og trafikksikkerhet. Den sanne sammenhengen, beregnet på grunnlag av ”feilfrie” data, er derfor etter all sannsynlighet sterkere enn den sammenhengen som er funnet i denne undersøkelsen.
5. Mange undersøkelser inneholder mange resultater. Hvis det er en statistisk avhengighet mellom de ulike resultatene, kan det redusere variasjonen i datamaterialet og dermed skape en spuriøs sammenheng mellom fart og trafikksikkerhet. Denne muligheten er undersøkt og det konkluderes med at det ikke finnes noen tegn til avhengighet mellom resultatene av en gitt undersøkelse.
6. Resultatene av undersøkelsene sier ikke noe om hvilke ulykkestyper, eller i hvilke trafikkmiljøer, fart har størst betydning for trafikksikkerheten. Datagrunnlaget gjorde det dessverre ikke mulig å undersøke sammenhengen mellom fart og ulykker for ulike ulykkestyper. Resultatene gjelder alle ulykker. Betydningen av trafikkmiljø er undersøkt i meta-analysen. Resultatet var at trafikkmiljø ikke har noen betydning.

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Sammenhengen mellom fart og trafikksikkerhet synes å være den samme i alle trafikkmiljøer.

7. Undersøkelsen er begrenset til potensmodellen. Mange andre modeller kan tenkes å beskrive sammenhengen mellom fart og trafikksikkerhet. To alternative modeller til potensmodellen ble undersøkt. Den ene er en lineær modell, der det er den absolutte endringen i fart, ikke den relative endringen, som har sammenheng med trafikksikkerhet. Den andre er en logistisk modell, der virkningen av fart avhenger av hvor høy farten er i utgangspunktet. Den lineære modellen er lite plausibel. Den logistiske modellen er noe mer plausibel, men datagrunnlaget gjør det ikke mulig å teste den på en god måte. I kraft av sin generalitet og enkelhet er potensmodellen overlegen.

Den generelle konklusjonen er at svakheter ved undersøkelsen neppe har hatt noen særlig betydning for resultatene.

Praktiske implikasjoner

Undersøkelsen viser at fart har meget stor betydning for trafikksikkerheten, trolig større enn noen annen kjent risikofaktor. Fart er en risikofaktor for absolutt alle trafikkulykker, fra de minste til de mest alvorlige. Betydningen av fart som risikofaktor er større for alvorlige ulykker og personskader enn for rene materiellskadeulykker.

Dersom man ønsker å utvikle et vegtransportsystem der ingen blir drept eller varig skadet (nullvisjonen), er fart den viktigste faktoren som bør reguleres. Det argumenteres i rapporten for at føreres valg av fart ikke alltid kan betraktes som fullstendig rasjonelt. Det foreligger med andre ord et legitimt grunnlag for at myndighetene begrenser trafikantenes frihet når det gjelder valg av fart. Dette gjøres da også i nesten alle land i form av fartsgrenser og håndheving av disse. Fartsgrenser og tiltak for å sikre at disse overholdes er særdeles viktige trafikksikkerhetstiltak.

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1 Background and introduction

1.1 Background

Few topics in transport are subject to more heated discussions than speed and its relationship to road accidents. Most drivers think that they are capable of choosing a safe speed. Speed limits have, however, been introduced in all countries. No country today allows drivers to freely choose their speed wherever they drive.

Speed limits are, however, widely violated. It is not uncommon that 30-50% of all drivers exceed the posted speed limit on a given road. Driving a little faster than the speed limit can be very tempting. In most cases, fast driving does not result in an accident, nor are most speeders caught by the police. Most of the time, the consequences of speeding, as experienced by the driver, are rewarding only. Most of the time, a speeding driver does not experience any unwanted consequences at all of speeding. It is therefore hardly surprising that most drivers think that they can indeed choose the right speed and regard speed limits as a necessary evil at best.

By contrast, most governments regard speeding as a major road safety problem. The government of New Zealand, for example, has published an extensive review of the relationship between speed and accidents, entitled “Down with speed” (Patterson, Frith and Small 2000). The Australian Transport Safety Bureau (2001) likewise identifies speed enforcement as one of the key actions of its national road safety strategy 2001-2010 (Australian Transport Safety Bureau 2001). For Sweden, Elvik and Amundsen (2000) estimated that perfect compliance with speed limits could reduce the number of road accident fatalities by 38% and the number of injured road users by 21%.

That estimate relied on a model of the relationship between speed and accidents developed by Göran Nilsson, referred to as the Power Model (Nilsson 2000; 2004A). According to the Power Model, which is presented in detail in the next chapter, changes in speed are associated with changes in the number of accidents according to a power function. The value of the exponent of this function varies according to accident severity.

The objective of this report is to evaluate the Power Model of the relationship between speed and accidents. This is done by reviewing and summarising a large number of studies that have evaluated the effects of changes in speed on the number and severity of accidents. Before presenting the results of this review, and the main questions it was designed to answer, a summary is given of other recent reviews of the relationship between speed and accidents.

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1.2 Recent reviews of speed and accidents

In the past 10 years, research projects reviewing the relationship between speed and accidents have been reported in Australia, the United States and Europe. The Australian review (Fildes and Lee 1993), concludes as follows with respect to the relationship between speed and road safety (page 10):

"Considerable research has been undertaken into the relationship between speed and crash involvement. Early studies suggested that variance above and below the mean speed of the traffic was the critical factor in causing speed related crashes. While recent studies have confirmed the relationship for vehicles travelling above the mean speed, it is not clear whether slow travel speeds are also crash inducing. Furthermore, many of these studies are flawed for one reason or another because of the lack of objective travel speed data for crash involved vehicles. … There is clearly an urgent need for more definitive research into the relationship between travel speed and crash involvement."

"The relationship between travel speed and injury severity is considerably more convincing than for crash involvement. Indeed, the dissipation of energy resulting from any collision can be expressed by the physical relationship between vehicle mass and speed. … Kinetic energy is generated by the moving vehicle by the square of speed rather than speed itself, defined by the following physical relationship:

$$\text{Kinetic energy} = \tfrac{1}{2} \cdot \text{mass} \cdot (\text{velocity})^2$$

Thus, a 20 percent increase in speed will, for example, result in a 44 percent increase in kinetic energy to be dissipated. This means that the likelihood of injury in a crash increases exponentially with the speed of the collision."

The American review (Transportation Research Board 1998) summarises what is known about the relationship between speed and accidents in the following terms (page 4):

"Drivers' speed choices impose risks that affect both the probability and severity of crashes. Speed is directly related to injury severity in a crash. The probability of severe injury increases sharply with the impact speed of a vehicle in a collision, reflecting the laws of physics. The risk is even greater when a vehicle strikes a pedestrian, the most vulnerable of road users. Although injury to vehicle occupants in a crash can be mitigated by safety belts and airbags, the strength of the relationship between speed and crash severity alone is sufficient reason for managing speed."

"Speed is also linked to the probability of being in a crash, although the evidence is not as compelling because crashes are complex events that seldom can be attributed to a single factor."

Finally, the European review, performed on behalf of the European Commission (the MASTER-project), summarises the relationship between speed and accidents in these terms (European Commission 1999, pages X and XI):

"On roads of a given type, injury accident rate, severe injury (including fatal) accident rate and fatal accident rate increase roughly as the 2^{nd}, 3^{rd} and 4^{th} powers of the mean traffic speed. Studies summarising findings from before-and-after studies of the impacts of speed on accidents have resulted in a rule of

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> *thumb saying that a 1 km/h decrease in mean speed causes a 2 to 3 percent reduction in injury accidents."*

These reviews agree that there is clear relationship between speed and accident severity. The European review is the only one that explicitly refers to the Power Model and endorses it. The other two reviews, in particular the Australian review, are more cautious in claiming that speed influences the probability of accident occurrence.

It should be noted, however, that the Power Model does not address the relationship between speed and the probability of accidents as such. The model refers to injury accidents (of varying levels of severity) only, and not to accidents in general. In principle, it is conceivable (but unlikely) that speed does not influence the total number of accidents, merely whether an accident leads to personal injury or not.

While covering an impressive number of studies, neither of the three major reviews quoted above tried to formally synthesise evidence from these studies by applying techniques of meta-analysis. The reviews were not designed to test the Power Model, but the conclusions drawn in them are consistent with that model.

1.3 Research problems addressed in this report

The main research problem addressed in this report is:

Does the Power Model adequately describe the relationship between speed and accidents?

To answer this question, a systematic review has been made of studies that have evaluated the effects of changes in speed on the number or severity of accidents or accident victims. The studies that have been reviewed were retrieved by means of a systematic literature search. Relevant studies were coded and a meta-analysis of these studies was performed. This analysis was designed to evaluate the Power Model.

In addition to evaluating the Power model, this report will discuss the following questions:

1. Can the relationship between speed and accidents be modelled theoretically, i.e. can the Power Model or related models be deduced from physical laws of motion?
2. Is the Power Model the best model of the relationship between speed and accidents, or do alternative models summarise this relationship more accurately?
3. To what extent is the relationship between speed and accidents modified by other variables, for example the traffic environment?

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2 The Power Model

2.1 The Power Model as presented by Göran Nilsson

The following presentation of the Power Model is to a large extent based on a description of the model given by Göran Nilsson in his doctoral dissertation (Nilsson 2004A).

The empirical background of the model comes from the different changes in the speed limits, which were made at the end of the sixties and in the beginning of the seventies in Sweden. Studies evaluating the effects of these changes found that the percentage change of the number of injury accidents was proportional to the square of the relative speed change. This applied both to increases and decreases in mean speed.

The model can be summarised in terms of six equations that relate changes in the number of accidents or in the number of road users killed or injured in accidents to changes in the mean speed of traffic. Denote speed by V, accidents by Y, and accident victims by Z. Furthermore, subscript by 0 the values observed before a change in mean speed and by 1 the values observed after a change in mean speed. The Power model is then presented in equations 1 to 6 below:

Number of fatal accidents = $\boldsymbol{Y}_1 = \left(\frac{\boldsymbol{V}_1}{\boldsymbol{V}_0}\right)^4 \boldsymbol{Y}_0$ (1)

Number of fatalities = $Z_1 = \left(\frac{V_1}{V_0}\right)^4 Y_0 + \left(\frac{V_1}{V_0}\right)^8 (Z_0 - Y_0)$ (2)

Number of fatal and serious injury accidents = $\boldsymbol{Y}_1 = \left(\frac{\boldsymbol{V}_1}{\boldsymbol{V}_0}\right)^3 \boldsymbol{Y}_0$ (3)

Number of fatal or serious injuries = $Z_1 = \left(\frac{V_1}{V_0}\right)^3 Y_0 + \left(\frac{V_1}{V_0}\right)^6 (Z_0 - Y_0)$ 4)

Number of injury accidents (all) = $\boldsymbol{Y}_1 = \left(\frac{\boldsymbol{V}_1}{\boldsymbol{V}_0}\right)^2 \boldsymbol{Y}_0$ (5)

Number of injured road users (all) = $Z_1 = \left(\frac{V_1}{V_0}\right)^2 Y_0 + \left(\frac{V_1}{V_0}\right)^4 (Z_0 - Y_0)$ (6)

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The Power Model suggests that the number of fatal accidents, serious injury accidents (including fatal accidents), and all police reported injury accidents (including fatal and serious injury accidents) change in proportion to, respectively, the fourth, third and second power of the relative change in the mean speed of traffic. The power of two proposed for injury accidents is derived from the equation for kinetic energy:

$$\text{Kinetic energy} = \tfrac{1}{2} \cdot \boldsymbol{mass} \cdot \boldsymbol{V^2} \qquad (7)$$

The powers proposed for fatal accidents and serious injury accidents are based on best fitting values to data from Sweden, and have no theoretical foundation.

A slightly altered version of the Power Model has been developed for the number of killed or injured road users. This number tends to be more than one per injury accident. As an example, there is, on the average, about 1.13 fatalities per fatal accident in Norway, and about 1.4 injured road users per police reported injury accident. The additional terms raised to the powers of 8, 6 and 4, for fatalities, serious injuries and all injuries, are multiplied by a term indicating the difference between the number of accident victims and the number of accidents.

To show how predictions can be derived from the model, consider the following example:

There are 265 fatal accidents per year in a certain road traffic system. In these accidents, a total of 300 people are killed. Let us assume that the mean speed of traffic is reduced by 10%. The ratio V_1/V_0 is then 0.9. The following number of fatal accidents is predicted following this change in mean speed:

$$\text{Predicted number of fatal accidents} = 0.9^4 \cdot 265 = 0.656 \cdot 265 \approx 174.$$

The number of fatal accidents is predicted to be reduced from 265 to 174 (the nearest whole number), which is a reduction of more than 34%. The number of fatalities is predicted to be:

Predicted number of fatalities = $(0.9^4 \cdot 265) + (0.9^8 \cdot (300 - 265)) \approx 174 + 15 = 189$

The number of fatalities is thus expected to go down from 300 to 189, a reduction of 37%.

In general, the Power Model predicts larger percentage changes in the number of accident victims than in the number of accidents. The Power model does not include a prediction of the number of slight injury accidents or slightly injured accident victims. Logically speaking, however, the exponent applying to slight injury accidents and slightly injured road users must be lower than the exponent applying to all injury accidents or all injured road users. Table 1 presents a set of hypothetical exponents that would be consistent with the Power Model.

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Table 1: Predicted, hypothetical, values of exponents consistent with the Power Model

	Dependent variable		
Accident or injury severity	**Accident victims**	**Accidents**	**Victims per accident**
Fatal	6	4	2
Serious	5	3	2
Slight	2	1	1
All injury	3	2	1
Property damage	-	1	-

Source: TØI report 740/2004

The Power Model has a number of attractive features. It is parsimonious, simple, elegant and general. It yields results that make sense. It is empirically testable. Moreover, since the model relies on a power relationship between variables, it lends itself to reformulation and further development by applying the algebra of powers, the essential elements of which are presented below for reference purposes.

2.2 The algebra of Powers

$$A^m \cdot A^n = A^{m+n} \qquad (8)$$

$$(A \cdot B)^n = A^n \cdot B^n \qquad (9)$$

$$\frac{A^m}{A^n} = A^{m-n} \qquad (10)$$

$$\left(\frac{A}{B}\right)^n = \frac{A^n}{B^n} \qquad (11)$$

$$\left(\frac{A}{B}\right)^n \cdot \left(\frac{B}{C}\right)^n = \left(\frac{A}{C}\right)^n \qquad (12)$$

$$A^{-n} = \frac{1}{A^n} \qquad (13)$$

$$A^n = e^{\ln(a) \cdot n} \qquad (14)$$

$$Y = X^a \Rightarrow a = \frac{ln(Y)}{ln(X)} \qquad (15)$$

By applying the algebra of powers, estimates referring to different levels of accident or injury severity can be compared directly to one another.

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3 Theoretical perspectives on speed and accidents

3.1 An elementary model of the relationship between speed and accidents

A moving body contains kinetic energy, according to equation 7 (see chapter 2). When an accident occurs, kinetic energy is transformed into destructive forces that deform vehicles and may cause injury to vehicle occupants. The energy model is well suited to describing the severity of an accident, once it has occurred. It does not, however, say anything about the probability of accident occurrence. Kinetic energy is harmless by itself, as long as it is controlled. The Power Model has implications both for the probability of accident occurrence and for the consequences of accidents in terms of injuries. It is therefore of some interest to investigate whether the Power Model can be deduced from elementary physical models of the relationship between speed on the one hand, and the probability and likely consequences of accidents on the other hand.

Accidents are complex and multi-causal events. No model has therefore been developed that relates speed directly to accidents in terms of elementary physical laws. The closest analogy to such a law is probably the general equation for the braking distance of a motor vehicle:

$$\text{Braking distance} = S = \frac{v_0^2}{2fg} \qquad (16)$$

in which v_0^2 is initial speed squared (metres per second), f is the friction coefficient and g is the gravitational constant (9.8 metres per square second). Let us, for the purposes of the discussion, disregard any confounding factors for the moment and investigate the implications of this simple model for motor vehicles travelling at initial speeds of 20, 40, 60, 80 or 100 km/h on a plain road surface that has a friction coefficient of 0.8. This corresponds to driving on a dry, hard road surface during summer. Initial speeds of 20, 40, etc km/h correspond to speeds of 20/3.6 = 5.55 m/second, 40/3.6 = 11.11 m/second, and so on.

Figure 1 shows retardation curves for the five initial speeds assumed above. The curves consist of two parts. The first part is a straight line, which is the distance travelled before the driver reacts. A standard reaction time of 1.5 seconds has been assumed. The second part is when braking occurs. The distance covered during braking is estimated by means of equation 16 above.

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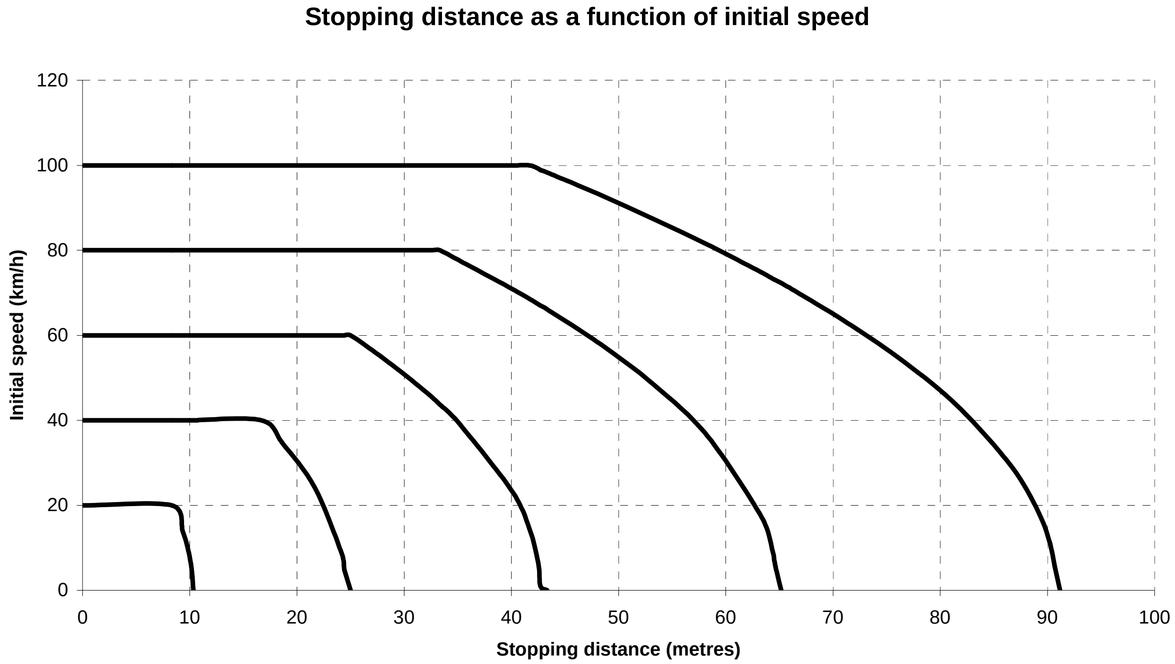

Source: TØI report 740/2004

Figure 1: Stopping distance from various initial speeds

Stopping distance is not proportional with speed. If one takes the length of the stopping distance as an indicator of the probability of accident occurrence, the following information can be extracted from Figure 1:

Initial speed (km/h)	Stopping distance (m)	Relative change in speed	Relative change in stopping distance	Estimate of power
20	10.3			
40	25.0	2.00 (40/20)	2.43 (25/10)	1.28
60	42.8	1.50 (60/40)	1.71 (43/25)	1.33
80	64.7	1.33 (80/60)	1.51 (65/43)	1.44
100	90.8	1.25 (100/80)	1.40 (91/65)	1.52

Source: TØI report 740/2004

Stopping distance is seen to increase more rapidly than speed, implying a power of between 1.28 and 1.52, for the range of changes in speed considered. The estimate of power is not constant, but appears to increase as initial speed increases. This suggests that the probability of accident occurrence increases more than in proportion to speed level, which is consistent with the Power Model.

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3.2 The relationship between speed and injury severity

The Power Model applies to injury accidents only, and not to accidents at large. Most accidents do not result in personal injury, but in property-damage-only. Whether an accident results in personal injury, depends on its severity. Severity is usually stated in terms of the change in speed occurring as a result of the accident (delta V = ΔV). Haddon (1970) states, as a rule of thumb, that decelerations of less than 30g (g is the acceleration of gravity = 9.81 m/sec^2) do not cause injury. Kallberg and Luoma (1996) point out that human vulnerability varies a great deal, and that such rules of thumb may be too simple. They add, however, that decelerations of 40 to 80g usually cause serious injuries.

A comprehensive review of the biomechanics of impacts in road accidents has been given by Mackay (1997). The review makes it clear that there is no simple relationship between impact severity, which is generally measured in terms of the change in velocity (usually referred to as delta-V, symbolised by ΔV) when an impact occurs, and the severity of injuries sustained by road users. A pedestrian, for example, is unprotected and may sustain fatal or serious injuries at impact speeds as low as 30 or 40 km/h. A well-protected occupant of a modern car would in most cases not be injured at all at a similar impact speed in a frontal crash. If, on the other hand, the car is struck from the rear, whiplash injuries leading to long-term impairment may occur even at impact speeds of 15-20 km/h.

Despite the complexity of the relationship between impact speed and the probability of sustaining injuries of a given severity, there is no doubt at all that the probability of sustaining fatal or serious injury increases dramatically as impact speed increases. Figure 2 gives an illustration of this. It has been derived from studies of the relationship between impact speed and the probability of a fatal injury to pedestrians struck by motor vehicles (Ashton 1980, Walz et al 1983, Otte and Suren 1984, Interdisciplinary Group 1986). It is seen that the probability of fatal injury increases rapidly as impact speed increases.

Figure 3 shows how the probability of fatal injury in frontal impacts varies depending on impact speed for unbelted car drivers (Evans 1994). When impact speed is less than 60 km/h, very few drivers are killed. The probability of a fatal injury injured then rises rapidly. At impact speeds above 100 km/h all drivers are injured. Mackay (1997) shows that the probability of getting injured at a given impact speed depends on driver age. It is shifted about 10 km/h down for drivers who are above 60 years, compared to drivers who are less than 30 years.

The Power Model refers to the relationship between the mean speed of traffic and the number of accidents or injured road users. In general, impact speed is lower than the speed of traffic, as many road users are able to slow down before an accident occurs. As is apparent from figures 2 and 3, impact speed has a major effect on the probability of sustaining fatal injury. Impact speed is related to the speed of traffic. Hence, the relationships observed in figures 2 and 3 suggest that reducing the speed of traffic will prevent accidents, as drivers will then be able to stop before the accident occurs.

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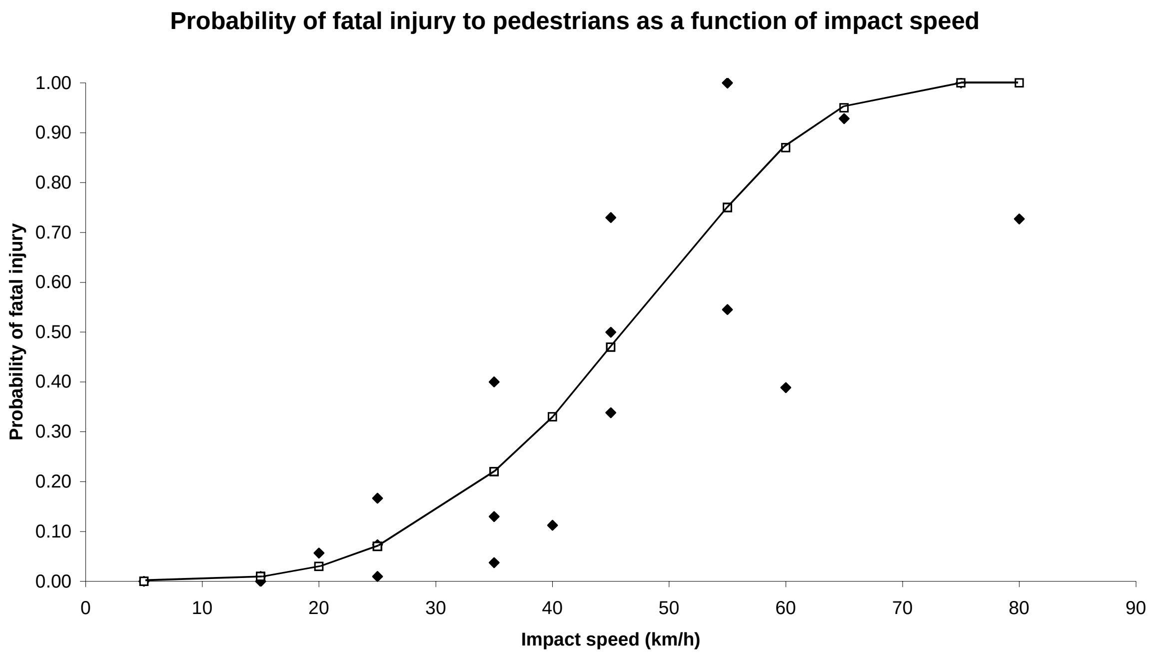

Source: TØI report 740/2004

Figure 2: Relationship between impact speed and probability of fatal injury to pedestrians. Adapted from Ashton 1980, Walz et al 1983, Otte and Suren 1984, Interdisciplinary Group 1986.

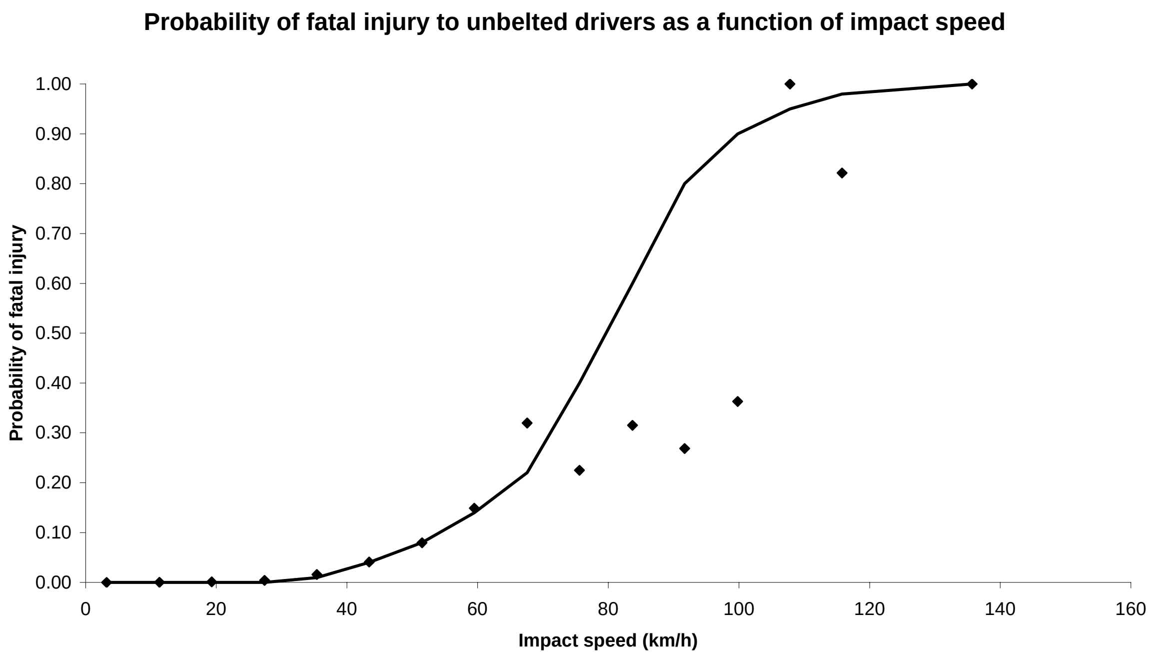

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Figure 3: Relationship between impact speed and probability of fatal injury to unbelted car drivers. Derived from Evans 1994

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3.3 The relationship between speed variance and road safety

It is sometimes claimed that road accidents are more closely related to speed variance than to speed level. One source of this claim is the studies by Solomon (1964), Munden (1967), Cirillo (1968) and others showing a U-shaped relationship between speed and accident rate. According to these studies, cars travelling slower or faster than the mean speed of traffic are more often involved in accidents (per kilometre of driving) than cars travelling at a speed close to the mean speed of traffic. A simple theoretical model can be used in order to assess the plausibility of the hypothesis that accidents are more closely related to speed variance than to speed level. Imagine a set of cars travelling at identical speeds along a two-lane undivided road. Since the speeds do not vary, no car can catch up with another car; cars travelling in the same direction cannot therefore crash with one another. The only possibility for crashes between cars is when a car encounters another car travelling in the opposite direction.

Figure 4 shows a flow of traffic conforming to the above description. Cars travelling south are shown as lines sloping downwards to the right. Cars travelling north are shown as lines sloping upwards to the right. All the lines have the same slope, indicating that there is no variance in speed. Intersections between the lines denote encounters between cars. Each encounter represents an event that could lead to an accident. Each encounter is indicated by a black dot. There are 32 encounters in total in Figure 4.

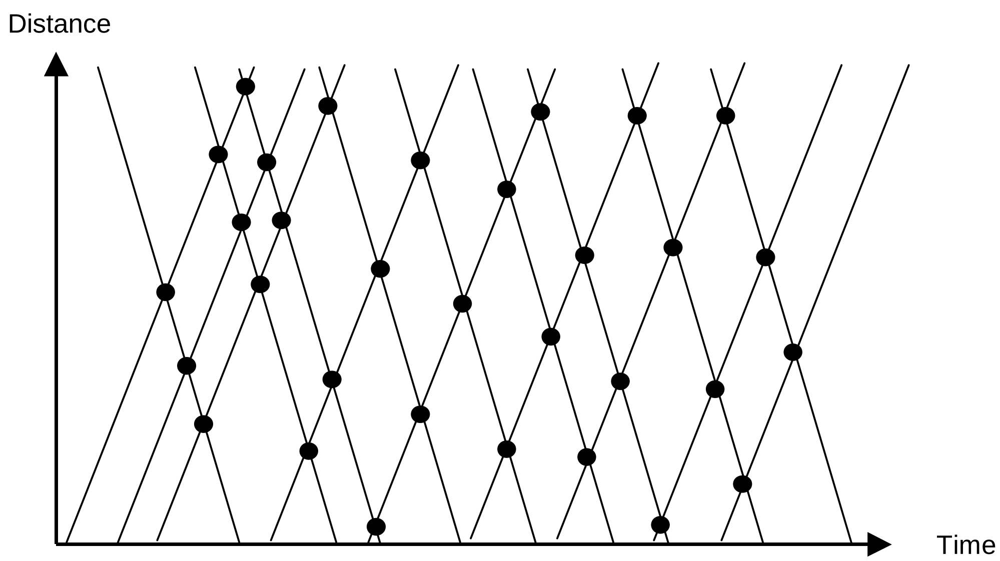

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Figure 4: Number of encounters between cars travelling at identical speed in opposite directions

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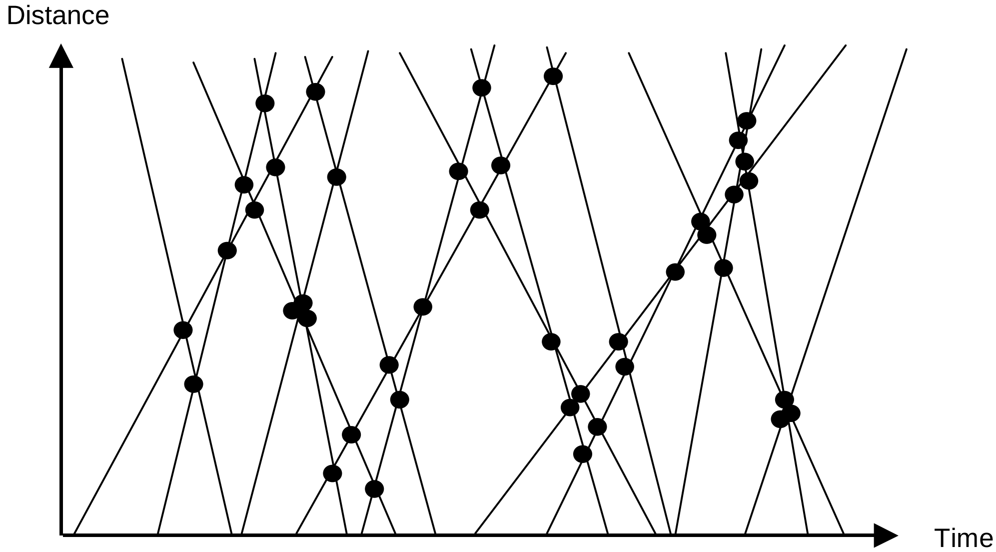
Let us now assume that speed varies. Speed variation can be depicted by varying the slopes of lines. Cars travelling faster than other cars may now catch them up, thus creating encounters even between cars travelling in the same direction. These encounters can be resolved by slowing down or by overtaking. Figure 5 shows the flow of traffic in a situation where there is variation in speed.

Cars travelling slowly are shown by lines with a gentle slope, cars travelling fast are shown by lines with a steep slope. Encounters between cars travelling in the same direction are shown simply as lines crossing, although in actual traffic the lines would not cross in the immediate fashion shown in Figure 5, but run parallel until an opportunity for overtaking became available.

There is a total of 42 encounters in Figure 5, compared to 32 in Figure 4. This is an increase of the number of encounters of almost a third, indicating that speed variance may generate more conflict situations between vehicles.



Source: TØI report 740/2004

Figure 5: Number of encounters between cars travelling at different speeds

Comparing Figures 4 and 5, the distribution of cars by the number of encounters is:

Number of encounters	No speed variance	Speed variance
2	1	2
3	7	2
4	8	3
5	2	7
6		1
7		3
Total number of cars	18	18

Source: TØI report 740/2004

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If the number of encounters is taken as an indicator of the probability of accident occurrence, it would seem that speed variance is indeed associated with an increase in this probability. Moreover, in Figure 5 a tendency can be seen for slow moving cars to have more encounters than faster moving cars, indicating that they could be more often involved in accidents than cars moving faster.

It would, however, be wrong to conclude on the basis of these observations that the presence of a U-shaped relationship between speed and accident involvement shows that speed variance is more important for accidents than speed level. White and Nelson (1970) have shown that a U-shaped relationship between speed and accident involvement rate could arise as an artefact of errors in the measurement of speed. Simulation studies made by Hauer (2003) have confirmed this. Even if the speeds of vehicles involved in accidents are accurately measured, a U-shaped curve for deviation from mean speed can still arise purely as a statistical artefact, as a result of how the crash involvement variable is defined. Consider the following example:

Table 2: Hypothetical data on speed of traffic and accident involvement for 100 cars

Speed of car 1	Speed of car 2 (standard deviations from the mean)					
(standard deviations from the mean)	-2	-1	0	1	2	Total
-2	1	2	4	2	1	10
-1	2	4	8	3	2	20
0	4	8	16	8	4	40
1	2	4	8	4	2	20
2	1	2	4	2	1	10
Total	10	20	40	20	10	100

Source: TØI report 740/2004

Table 2 shows hypothetical data for 200 cars, involved in 100 two-car crashes. There is no relationship between deviation from mean speed, indicated by the number of standard deviations below (minus) or above (plus) the mean, and crash involvement. However, depending on how the number of crash-involved cars is counted, an artificial relationship can arise. Take, for example, crashes involving cars driving more than two standard deviations below the mean. The number of crashes in which such cars is involved is 19. This is derived as the sum of the row entitled “-2” and the column entitled “-2”, minus the crash in the “-2, -2” cell, which is counted twice. In the same manner, the number of crashes involving cars driving one standard deviation below the mean, at the mean speed, etc is derived. The resulting number of crashes and estimated risk of crash involvement is shown below:

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Deviation	Crashes	Traffic	Risk
-2	19	10	1.9
-1	36	20	1.8
0	64	40	1.6
1	36	20	1.8
2	19	10	1.9

It is seen that an apparent relationship between deviation from mean speed of travel and accident involvement arises. But this relationship is not real, it is simply a product of the way crashes are counted.

Finally, as pointed out by Davis (2001), the presence of a correlation between an aggregate measure of speed dispersion and aggregate accident rate has no implications whatsoever for the shape of the relationship between deviation from the mean speed of traffic and accident involvement rate at the individual level.

In real data, there is often a strong correlation between mean and variance: the higher the mean speed, the greater the variance. It may therefore be difficult to separate the effects on accidents of mean speed and speed variance. The Power Model applies to mean speed, and the analysis has focused on the relationship between mean speed and the number of accidents or accident victims.

3.4 Speed in context: the role of confounding factors

Although the Power Model suggests that there is a strong relationship between speed and accidents, it by no means claims that speed is the only factor that influences accident occurrence or injury severity. Speed is just one of very many factors that influence accidents or injuries.

When studying the effects on accidents of speed and changes in speed, it is important to control for other factors that may influence the number of accidents or their severity. To help us to identify such factors, a causal diagram of the main categories of variables influencing accidents will be useful. Figure 6 presents such a diagram.

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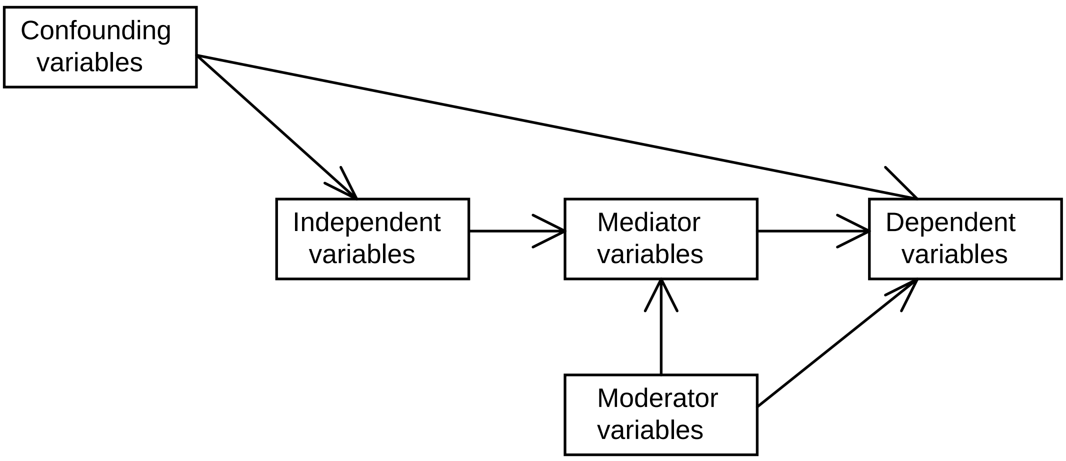

Source: TØI report 740/2004

Figure 6: A typology of the main categories of variables influencing road accidents

Figure 6 identifies five types of variables that are relevant for road safety evaluation studies, including studies of the relationship between speed and accidents. It is convenient to read the figure from right to left.

Dependent variables are variables that describe the outcome of interest in a study. In the present study, two dependent variables are of primary interest (chapter 2):

1. The (expected) number of accidents of a given severity
2. The (expected) number of accident victims of a given injury severity

Independent variables are variables that are intended to influence the dependent variables in a desired direction. In most road safety evaluation studies, the independent variable will be a road safety measure, which is intended to reduce the number of accidents or injury severity. In the present study, independent variables are all variables that are deliberately controlled or introduced in order to influence driving speeds. These variables include speed limits, police enforcement of speed limits, speed humps and a host of other variables.

Most road safety measures exert their influence on accidents by modifying one or more risk factors that are associated with accident occurrence or injury severity. These risk factors are identified as mediating variables in Figure 6. In the case of measures intended to influence speed, the mediating variable is changes in speed.

The size of the effect on accidents of a road safety measure depends, not just on how strongly the measure influences the risk factor or risk factors it is intended to influence, but also on how other risk factors are affected. These other risk factors, which may include factors a safety measure does not intend to influence, are referred to as moderating variables. As an example, the effects on accidents of changes in speed may vary according to road surface condition, which is in turn related to friction.

Confounding variables, in the present context, are all variables in addition to changes in speed that influence the number of accidents or injuries. Hence, any variable that may influence accidents or accident victims independently of speed, is a potentially confounding variable.

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The number of potentially confounding variables in road safety evaluation studies is very large. It is obviously impossible to identify all potentially confounding variables, let alone measure these variables and control for them. In principle, potentially confounding variables include any changes in exposure (traffic volume) and risk factors that could influence the number of accidents or the severity of injuries independently of, or in addition to, driving speed. The next chapter will discuss more in detail some important confounding variables affecting studies of the relationship between speed and road safety.

For the moment, it suffices to note that the Power Model should be seen as a theoretical model of the relationship between speed and road safety, in which no other factors are assumed to influence the number of accidents or accident victims. Thus, the model is an idealised description of the relationship between speed and accidents. In real data, such an ideal relationship will not be observed. Observed relationships between speed and accidents will be more or less confounded or attenuated by the effects of confounding variables and moderating variables whose effects various studies will be more or less successful in identifying or removing. It is therefore essential to describe studies as precisely as possible in terms of the confounding and moderating variables they control for or specify.

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4 Threats to validity in studies of the impacts of speed on road safety

4.1 Types of validity and threats to validity

The validity framework of Shadish, Cook and Campbell (2002) has been applied in order to identify the most important threats to validity in studies evaluating the effects of changes in speed on road safety. Relevant threats fall in four classes:

1. Threats to statistical conclusion validity
2. Threats to internal validity
3. Threats to construct validity
4. Threats to external validity

Statistical conclusion validity refers to the accuracy, reliability and sampling properties of the numerical findings of a study. Internal validity refers to the possibility of inferring a causal relationship between a pair of variables (or within a larger subset of variables). Construct validity refers to the adequacy of operational definitions of theoretical concepts and propositions. External validity refers to the possibility of generalising the findings of a study to other contexts or settings than those in which the study was performed. All these types of validity are relevant in the present study.

Statistical conclusion validity is influenced by, for example, sample size, the reliability of data, the possible presence of systematic errors in data and the appropriateness of the statistical techniques used to analyse data. In this study, the following factors are judged to be most relevant for statistical conclusion validity:

1. Errors in speed measurements
2. Incomplete accident reporting; unreliable accident data

Internal validity is primarily influenced by how well a study controls for potentially confounding variables. As noted in Chapter 3, the number of potentially confounding variables in road safety evaluation studies is infinite. To assure the best possible control for confounding variables, an experimental study design (randomised controlled trial) must be applied. In road safety evaluation studies, experimental designs are rarely found. Therefore, most such studies do not control for all potentially confounding variables. Trying to enumerate all such variables and assessing whether a study has controlled for them or not is an impossible task. It has therefore been decided to focus on the following four potentially confounding variables that are known (Hauer 1997) to be important in road safety evaluation studies:

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1. Regression-to-the-mean, in particular in accident data
2. Long term trends, in particular in accident data
3. Major changes in traffic volume
4. Inadequate control of risk factors associated with accident occurrence

The meaning of these variables will be elaborated below.

The theoretical relationship (construct) of interest in this study is the relationship between speed and road safety. While speed might appear to be a very simple concept – the distance covered per unit of time – it can be measured in a number of ways, not all of which give identical results. At least the following measures of speed are found in the literature:

1. The mean speed of traffic, measured as spot speeds of vehicles passing a certain point on the road.
2. The mean speed of traffic, measured by driving along a road for a certain distance, trying to stay close to mean speed (floating car method).
3. Percentile speeds, of which the most commonly found are the 50th percentile speed (equal to median speed) and the 85th percentile speed.
4. Impact speed, which is the speed of a vehicle or road user when an impact starts.

The Power Model applies to the "mean speed of traffic". Hence impact speed is not relevant. In most studies, speed is probably measured as spot speed. Accident data will usually refer to a certain road or road system, and not just a particular spot. Moreover, accident data in most studies cover a period of at least a few years, whereas speed is often recorded only during a short period, often not more than a few weeks. Spot speeds measured during a short time may not be typical of the speed for the whole road system represented by accident data, nor for the whole period to which these data apply. This can threaten both construct validity and statistical conclusion validity.

Unfortunately, most studies do not describe in very great detail how speed was measured or how the locations and periods for speed measurements were sampled. The studies therefore hardly provide a basis for assessing how well the speed data presented in them represent what these data ought ideally to represent, namely the mean speed for the whole road system (not just a particular spot) to which accident data apply and for the whole period covered by these data (not just a short period).

An assessment of the construct validity of speed data is therefore hardly possible. In this study, the speed data given by each study have been taken at face value. In nearly all studies, the speed given is mean speed; in a few studies various percentile speeds have been used. The most frequently used percentile speeds are the 50th percentile, which is usually close to the mean speed, and the 85th percentile speed, which is usually close to one standard deviation above the mean speed.

The concept of road safety can also be defined in many ways. The theoretically best definition of road safety is the (long-term) expected number of accidents or accident victims. The recorded number of accidents or accident victims may not always be a good estimate of the long term expected number. Unreliability of

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accident data is, however, mainly a problem influencing statistical conclusion validity and internal validity.

External validity is sought in most scientific endeavours. The Power Model is stated in general terms and postulates that the effects of changes in speed on accidents and accident victims are the same everywhere. To test whether this is case, one should examine the relationship between speed and accidents across different countries, different years, different types of traffic environment, and so on. The findings of studies made in different contexts will then have high external validity if: (1) contexts that differ in a number of important respects are represented in the set of studies, and (2) study findings agree across different contexts. If, on the other hand, study findings do not agree across different contexts, external validity is low. Thus, external validity is best assessed as part of a meta-analysis of studies that differ with respect to the contexts in which they were performed.

4.2 Threats to validity in studies evaluating the relationship between speed and road safety

4.2.1 Errors and bias in speed data

It is often believed that it is easy to measure speed. Indeed, many children have done so by marking a start point and an end point on the ground and using a stopwatch to measure the time used to cover the distance between the two points. Measuring the speed of road traffic is, however, not so easy. There are many pitfalls and sources of error in speed data.

An example of the prevalence of errors in speed data based on continuous measurements employing inductive loops buried in the road surface can be found in a recent Norwegian study that evaluated the effects of reduced speed limits on selected sections of road (Ragnøy and Muskaug 2003). Data were collected only from sites where speed had been measured continuously for at least 10-15 weeks. For each site, the data were given in the form of hourly mean speed. There are 8,760 hours in one year, or 1,680 hours in 10 weeks. One would expect estimates of mean speed based on data for such a long period to be almost unaffected by short-term factors such as a rain shower, traffic congestion caused by an accident, abnormally slow-moving vehicles, and so on. An algorithm was developed that removed abnormal hourly mean values, such as zero (most likely due to failure of the equipment), hours that had mean speeds below 60 km/h (indicating congestion; the roads had speed limits of either 80 or 90 km/h; in congested traffic, speed limits are assumed not to influence the mean speed of traffic), or hours that had a standard deviation of more than 20 km/h. Application of this algorithm, as well as additional quality control of speed data resulted in the discarding of 36% of the data.

An error rate of 36% is clearly disturbing, in particular if it were to go undetected. It is, however, surprisingly rarely the case that studies evaluating the relationship between speed and road safety discuss errors in speed data. In most studies, errors

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in speed data are not mentioned. Apparently, the assumption is made that these data are perfectly reliable.

It is not correct to assume that speed data do not contain errors. Unfortunately, so few studies have tried to assess such errors, that for the purpose of the meta-analysis made in this report, no account has been taken of the possible presence of random or systematic errors of measurement in speed data. The assumption has been made that these data are reliable.

Another problem with respect to speed data concerns how the locations and periods for measuring speed were sampled. Again, few studies provide details about how the sample of speed data was obtained. Ideally speaking, speed data ought to be representative for the mean speed of traffic in the whole area and for the whole period to which accident data apply. In most studies, speed data have been collected during a much shorter period than accident data. It is not uncommon that speed data for a single location, or some very locations, are presumed to represent a road system of several hundred kilometres. In practice, this report makes the same assumption, as information to justify a different approach is almost nonexistent.

4.2.2 Incomplete accident reporting and changes in the level of accident reporting

It is well known (Elvik and Mysen 1999) that the reporting of injury accidents in official road accident statistics is incomplete. The fact that reporting is incomplete does, by itself, not introduce any bias in studies evaluating the relationship between speed and accidents. Results can be biased, however, if the level of accident reporting changes over time (relevant to before-and-after studies), or if it varies between locations (relevant to cross-section studies)

Nearly all studies evaluating the relationship between speed and road safety are based on official road accident statistics. An explicit assessment of the level of reporting, and its variation, is almost never done. The reason for this is that in most cases, an accident record known to be complete does not exist. Hence, all that can be said about this matter, is that unknown variation in the level of accident reporting is a potential source of error in most road safety evaluation studies. It is, unfortunately, not possible to remove or control for this potential source of error unless one has access to an accident record known be complete, or at least more complete, than official road accident statistics.

4.2.3 Regression-to-the-mean

Regression-to-the-mean denotes the tendency for an abnormally high number of accidents to return to values closer to the long term mean; conversely abnormally low numbers of accidents tend to be succeeded by higher numbers. Regression-to-the-mean occurs as a result of random fluctuation in the recorded number of accidents around the long-term expected number of accidents. Regression-to-the-mean threatens the validity of before-and-after studies, but is, at least in large samples, perhaps a less serious threat to validity in cross-sectional studies.

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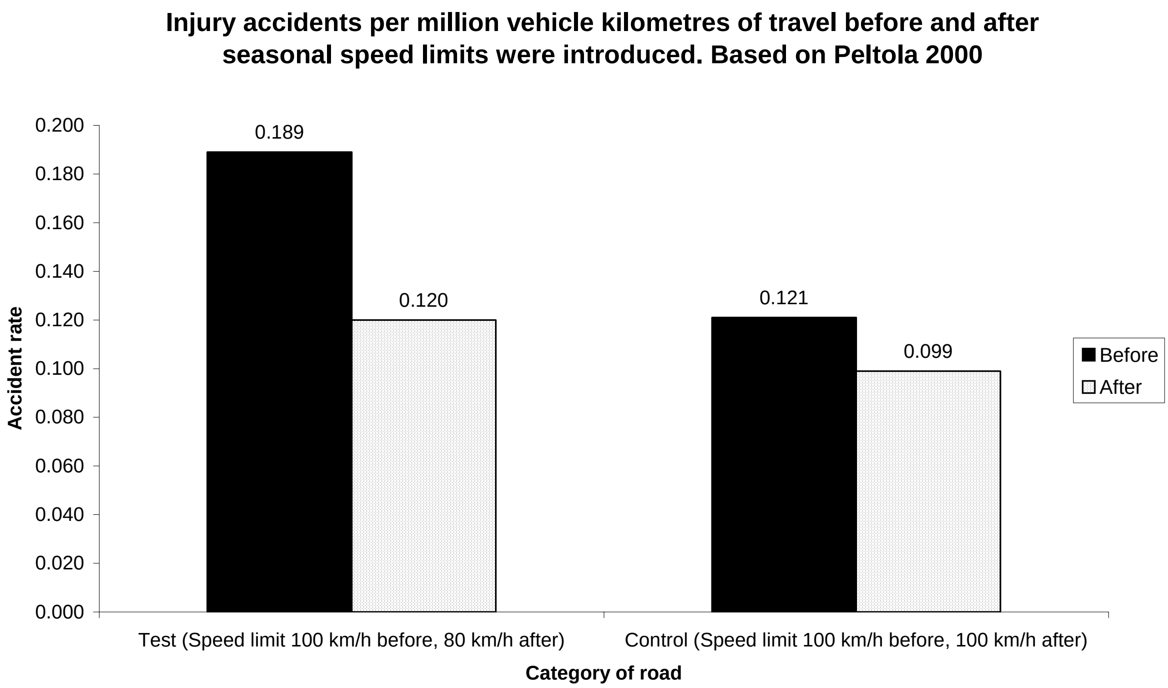
When is regression-to-the-mean most likely to be a confounding factor in before-and-after studies? Regression-to-the-mean is most likely to confound a study when speed limits are changed on relatively short road sections that have been selected for changes in speed limits because of their past accident record. The most typical cases would be for speed limits to be reduced on roads with a bad accident record, and for speed limits to be increased on safer-than-normal roads.

Regression-to-the-mean can influence the results even of experimentally designed studies. An example of this is an evaluation of seasonal speed limits in Finland (Peltola 2000). Matched pairs of roads were formed. In each pair, one road was selected at random for introduction of a seasonal speed limit, the other road retained the original speed limit. Despite the random assignment of roads, their accident rates during the before-period differed, as shown in Figure 7.

The accident rate on road sections where the seasonal speed limit of 80 km/h was introduced was more than 50% higher during the before-period than the accident rate on roads that retained the 100 km/h speed limit. The author notes that part of the observed reduction of the accident rate on roads that got a seasonal speed limit may be attributable to regression-to-the-mean.



Source: TØI report 740/2004

Figure 7: Change in accident rates on roads in Finland where a seasonal winter speed limit was introduced compared to roads where a seasonal winter speed limit was not introduced

In order to assess whether regression-to-the-mean actually is a confounding factor in a study, one needs data that can tell whether the recorded number of accidents was abnormally high or low. Very often, such data are not presented in evaluation studies, nor easily available from other data sources. Hence, in most cases, the best that can be done is to: (1) Check whether a study controlled for regression-to-the-mean or not, and (2) In case a study did not control for regression-to-the-mean, assess if regression-to-the-mean is likely to be a confounding factor or not.

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4.2.4 Long term trends in accident rates

Ideally speaking, studies evaluating the relationship between speed and road safety ought to control for long-term trends in accident rates. Historically speaking, accident rates have been falling in most highly motorised countries during the last 50-60 years. If this trend is not controlled for, there is a risk that studies will overstate the effect on accidents of reductions in speed, by erroneously attributing the whole reduction in accident rate from one year to the next to the reduction in speed. Conversely, the effect of increases in speed may be underestimated, or at worst have the wrong sign, if a long-term trend towards lower accident rates is left uncontrolled.

Controlling for long-term trends in accident rates can be done in various ways. One possibility is to extrapolate past trends. Another possibility is to use a comparison group that in the past was subject to the same long-term trend as the group in which a measure intended to influence speed was introduced.

4.2.5 Major changes in traffic volume

No factor exerts a greater influence on the number of accidents than traffic volume. Studies typically find that traffic volume explains about 67-75% of the systematic variation in the number of accidents (see, for example, the contributions to Gaudry and Lassarre 2000, or Greibe 2003). Moreover, it is typically found that the number of injury accidents increases by about 6-10% when traffic volume increases by 10%.

Traffic volume does not usually change abruptly from one year to the next. In Norway, to give an example, annual change in vehicle kilometres of travel during the years from 1973 to 2002 has varied between a reduction of 0.7% and an increase of 9.1% (Rideng 2003). In most years, traffic grows by between 1% and 3% compared to the previous year. These small changes are normally not associated with major changes in the number of accidents.

In many before-and-after studies, the data refer to a period of 6-10 years. During such a long period, changes in traffic volume are likely to be in the order of 10-20%, which one would expect to have an effect on the number of accidents. This effect can be controlled for by using a large comparison group, in which changes in the number of accidents can reasonably be assumed to reflect the effects of all factors that influence accident occurrence, including changes in traffic volume.

Another approach to controlling for changes in traffic volume is to estimate the relationship between traffic volume and the number of accidents, or to estimate accident rates. The latter approach is not ideal, since it controls for the effect of changes in traffic volume on the number of accidents only if this effect is linear and strictly proportional, i.e. a 10% increase in traffic volume is associated with a 10% increase in the number of accidents. If the relationship between traffic volume and the number of accidents is non-linear, relying on accident rates could give misleading results if there are major changes in traffic volume.

The objective of a study designed to evaluate the effects of changes in speed on road safety is to estimate effects of changes in speed on the expected number of accidents or on expected injury severity. The expected number of accidents is the

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number of accidents expected to occur in the long run at a given traffic volume and if all risk factors remain constant. Hence, one should always control for changes in traffic volume, even if these changes were induced by the measure that was introduced. It has been found, for example, that speed humps discourage traffic and can lead to substantial reductions in traffic volume (Baguley 1982). Even if these reductions in traffic volume are an effect of the measure, and may be expected to reduce the number of accidents, it is *the partial effect of changes in speed only – when all other factors are controlled for* – that we want to estimate. This means that the effects of reductions in traffic volume have to be factored out, so that any remaining changes in the number of accidents can be attributed to changes in speed only.

4.2.6 Inadequate control of risk factors influencing accident occurrence

A very large number of risk factors influence the number and severity of road accidents. Speed is an important risk factor, but it is not the only one, and it is not always dominant.

In some studies, the relationship between speed and road safety is evaluated by comparing speed and accident rates in a sample of roads that differ with respect to speed and accident rates. This type of study is generally referred to as a cross-section study. As an example, consider a study by Harkey, Robertson and Davis (1990). In order to assess criteria for speed zoning, Harkey, Robertson and Davis compared driving speeds and accident rates for six different speed limits. The results of this comparison are shown in Table 3.

Table 3: Comparison of speed and accident rates for six different speed limits. Source: Harkey, Robertson and Davis 1990

Speed limit (miles/h)	Mean speed (miles/h)	Overall accident rate	Injury accident rate
25	31	13.53	4.45
30	36	10.81	2.90
35	39	2.89	1.07
40	41	1.96	0.82
45	49	1.52	0.66
50	52	1.74	0.89

Mean speeds are seen to slightly exceed the speed limit for all speed limits that were considered. Accident rate declines sharply as speed limit increases, both for all accidents and for injury accidents. If this relationship were taken at face value, one should raise speed limits in order to reduce accident rates.

This kind of relationship is not uncommon in cross-sectional studies. The reason for it is that the best roads tend to have the highest speed limits. The relationship, and the misunderstandings it can give rise to, can be further illustrated by means of some hypothetical, but by no means unrealistic data.

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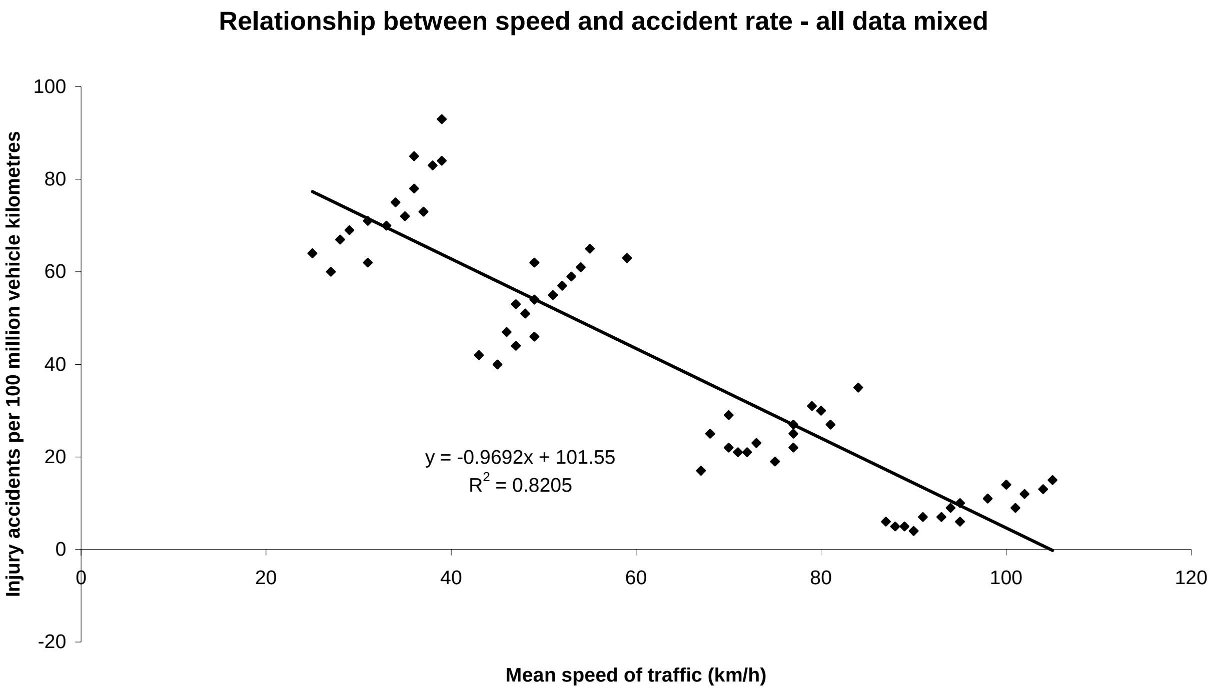
Figure 8 shows the relationship between speed and accident rate for four categories of road that have different speed limits. If all these categories are mixed, and a function fitted to the data, it will take the form shown in Figure 8, indicating that accident rate goes down as speed goes up. The absurdity of the function fitted to the data in Figure 8 is apparent from the fact that when speeds exceed about 100 km/h, the fitted accident rate becomes negative.

The true relationship between speed and accidents is shown in Figure 9. Functions have been fitted to the data within each category of road. In each group, the slope is now positive, indicating that as speed increases, so does the accident rate. A very instructive discussion of how to model the relationship between speed and accidents in cross-sectional data is given by Taylor, Baruya and Kennedy (2002). By applying state-of-the-art multivariate techniques, it is in principle possible to form groups of roads that are sufficiently homogeneous with respect to other risk factors to permit the partial effect of speed on accident rates to be estimated.



Source: TØI report 740/2004

Figure 8: Simple bivariate relationship between speed and accident rates. Hypothetical data

The basic problem in cross-sectional data that employ a heterogeneous sample of road sections, is that the causal direction between speed and accident rates becomes inverted or at least ambiguous: A low speed limit has been introduced on roads that have high accident rates, because these roads have high accident rates. It is not the case that low speed limits lead to high accident rates.

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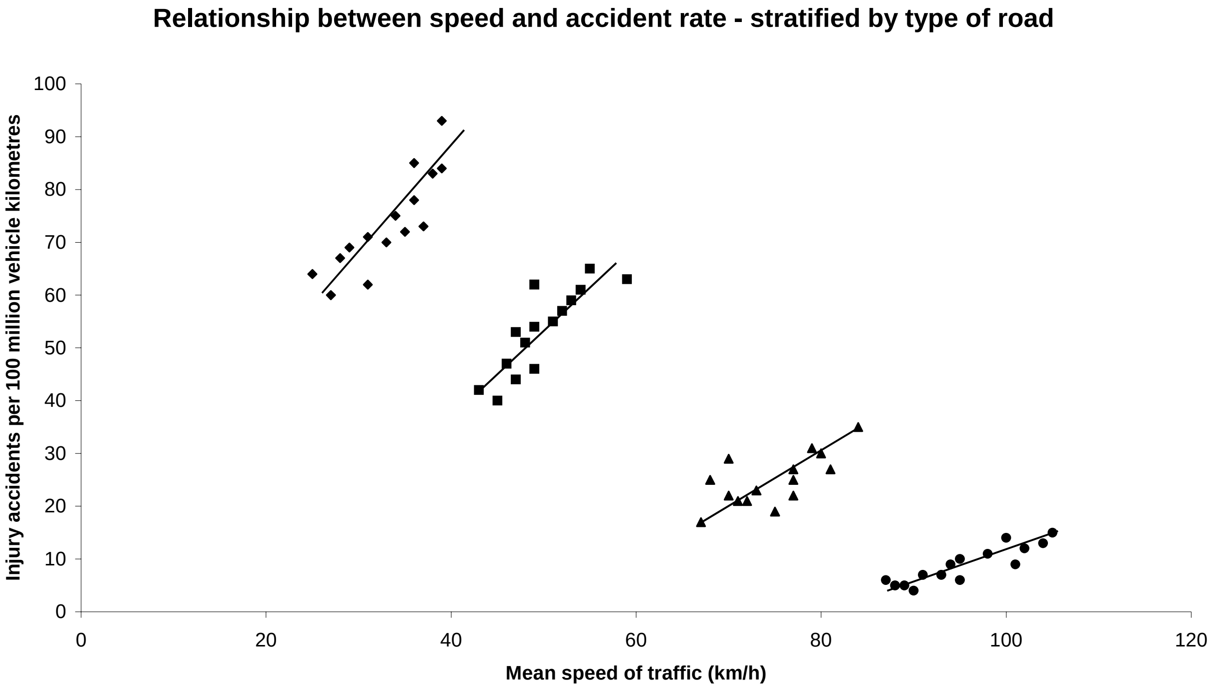

Source: TØI report 740/2004

Figure 9: Relationship between speed and accident rate modelled separately in each group of roads. Hypothetical data

In order to give valid estimates of the relationship between speed and accident rates, cross-sectional data should be homogeneous with respect to at least the following factors (these are judged to be the most important potentially confounding factors):

1. Traffic volume, since it influences both speed and accident rate.
2. The mix of various groups of road users in traffic. A high share of pedestrians, cyclists and users of mopeds or motorcycles is likely to increase the accident rate.
3. Speed limit. Of all factors influencing speed, speed limit is perhaps the most important.
4. Road width. It is known (Glad et al 2002) that road width strongly influences both speed and accident rate. The wider the road, the higher the speed. Wider roads tend to have lower accident rates.
5. Number of lanes. On two-lane roads, oncoming traffic can prevent overtaking, thus affecting speed.
6. Presence of a median. The presence of a median strongly reduces the number of head-on collisions.
7. Road curvature. Speeds are lower, and accident rates higher, on roads that have severe horizontal and vertical curves (bends and hills).
8. Number of access points. The higher the number of access points along a road, the higher the accident rate.

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It is rarely the case that cross-sectional studies have adequately controlled for all these factors. Hence, the results of such studies can be influenced by lack of control for other risk factors (than speed) influencing accident occurrence.

4.3 Control for threats to validity in various study designs

Various study designs are to varying extents capable of controlling for the threats to validity discussed in section 4.2. The degree of control for confounding factors actually obtained by the various research designs depends on two factors:

1. The way the research design is implemented
2. The way measures intended to influence speed are introduced.

4.3.1 Commonly employed study designs

Table 4 lists some of the more commonly found versions of various study designs employed in road safety evaluation research. Randomised controlled trials (experiments) are rarely used in road safety evaluation research. When used, such trials nearly always rely on a matched pair design. In this design, pairs of study units are formed, so that the members of each pair are as similar as possible with respect to all factors affecting road safety. Then one member of the pair is drawn at random for introduction of a measure intended to influence speed. The other member of the pair forms the control group. In general, matched pair experiments in road safety are rarely very rigorous by the standards that have developed in medical research. Features such as double blind analyses of data are rarely implemented. Despite this, randomised controlled trials can be assumed to control for all confounding factors, at least if the number of pairs used is sufficiently large for the law of large numbers in sampling theory to hold.

Table 4: Commonly employed study designs in studies evaluating the relationship between speed and road safety

Study design	Commonly found forms of the study design
A. Randomised controlled trials	A.1 Matched pair design
B. Before-and-after studies	B.1 With a matched comparison group
	B.2 With a non-equivalent comparison group
	B.3 With data on some confounding factors
	B.4 Without comparison groups or data on confounding factors
C. Cross-sectional studies	C.1 Employing multivariate models to form homogeneous groups
	C.2 Stratifying data by confounding factors
	C.3 Simple bivariate studies
D. Case-control studies	D.1 Controlling for confounding factors by multivariate analyses
	D.2 Controlling for confounding factors by stratifying data
	D.3 Not explicitly controlling for confounding factors

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Before-and-after studies come in many different forms. In Table 4, the most commonly found forms of this design have been listed from those that best control for confounding factors to those that basically do not control for any confounding factors at all (simple before-and-after studies with no comparison group and no data on confounding factors).

The various forms of cross-sectional studies and case-control studies have also been listed from those that presumably embody the best control of confounding factors to those that do not control for confounding factors.

4.3.2 Ways of introducing speed-influencing measures

Whether a certain potentially confounding factor is likely to exert an influence on a study or not, depends to some extent on how the measure intended to influence speed has been introduced. A distinction can be made between two broad classes of cases:

1. Cases in which the measure is local, i.e. introduced on certain road sections only.
2. Cases in which the measure is system-wide, i.e. introduced on all roads in a country or state.

In the former case, the road sections where a measure is introduced are always deliberately selected from a larger set of roads. In this process of selection, a bad accident record may be one of the reasons for introducing a safety treatment on a certain road. When the measure is local, one can never rule out the possibility that regression-to-the-mean can (but not necessarily will) influence a study. A similar point of view applies when a speed limit is temporary (seasonal). The period during which it applies will then very often have been identified as a period during which there are more accidents than during the rest of the year.

In the latter case, the measure will usually involve changing speed limits on a system-wide basis. Although even the total number of accidents in a country or state is subject to random fluctuations, it is highly unlikely that these could exert a very great influence on a study. Consider, as an example, a system in which the annual expected number of injury accidents is 1,000. The 95% confidence interval for this expected number of accidents spans 938 to 1,062 accidents. Hence, the maximum conceivable size of regression-to-the-mean in this case would be amount to an accident reduction, or accident increase, of slightly more than 5%. Regression-to-the-mean is, therefore, in general less likely to be a major confounding factor if a measure is system-wide than if it is confined in time or space.

Long-term trends can influence all before-and-after studies, but not cross-sectional studies or case-control studies. This applies irrespective of whether a measure intended to influence speed is introduced locally or on a system-wide basis.

Changes in traffic volume, or differences in it, is a threat to validity in all studies, except those that have been matched with respect to traffic volume. Effects of risk factors other than speed figures prominently as a potential source of error in cross-sectional studies and case-control studies, but is less likely to affect before-and-

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after studies, in particular if these studies have employed a comparison group that reflects the effects of all risk factors influencing accidents.

In chapter 5, the assessment that has been made in this chapter will be employed in order to score studies with respect to control for confounding factors.

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5 Study retrieval, data extraction and study quality assessment

5.1 Literature search

A search for relevant studies was conducted by combining two search strategies. The first strategy was to retrieve all references listed in previous literature surveys. This included the references found in Elvik, Mysen and Vaa (1997), Transportation Research Board (1998), Fildes and Lee (1993), and European Commission (1999).

The second strategy was to perform a computer search in the TRANSPORT literature data base, using “speed and accidents” as search terms. This search identified 1,469 entries. Two researchers independently examined these entries, and selected those that were judged to be relevant on the basis of the abstract (if an abstract was provided). If an abstract was not provided, studies were selected in the basis of their title.

A total of 98 studies were included in the meta-analysis. Far more studies were found, but not all of them could be included in the meta-analysis.

5.2 Study inclusion criteria

To be included in the meta-analysis, a study had to provide the following information:

1. Mean speed before the adoption of a measure affecting speed
2. Mean speed after the adoption of a measure affecting speed
3. The number of accidents, or accident victims, by severity, before the adoption of a measure affecting speed
4. The number of accidents, or accident victims, by severity, after the adoption of a measure affecting speed
5. An identification of the measure which was introduced

The first four of these items are required in order to estimate the relationship between speed and accidents, and the statistical uncertainty of each estimate of this relationship. The fifth item is needed in order to evaluate whether the effects on accidents of changes in speed depend on the measure used to influence speed. Some of the measures that influence speed will also influence other risk factors, and may thus have a different effect on accidents from those measures that influence speed only (chiefly speed limits).

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5.3 Data extracted and coded for meta-analysis

Data for each study were entered on an EXCEL spreadsheet. Table 5 shows the variables that were coded for each study.

Table 5: Data extracted and coded for meta-analysis

Name of variable	Explanation
Study record number	A record number for each study, starting at 1 for the first study. Studies were entered chronologically.
Result record number	A record number for each result. Numbered consecutively. A study may contain several results.
Authors	Authors by family name, listed in order of appearance in the publication.
Publication year	Year of publication (four digits).
Data country	Country in which study obtained data. A three-letter code was used to identify countries.
Publication type	Type of publication. The following main types of publication are used: (a) Scientific dissertation (Ph D or other) (DISS) (b) Paper in scientific journal (ART) (c) Conference proceedings (CONF) (d) Research report or report of public agency (REP)
Study design	A code for study design, with the following main types of design: (a) Randomised controlled trial (EXP) (b) Before-and-after, matched comparison group (BAM) (c) Before-and-after, non-equivalent comparison group (BAC) (d) Before-and-after, no comparison group (BAS) (e) Cross-sectional study (CST) (f) Case-control study (CACO) (g) Time-series analysis (TI-SE)
Main measure	The principal road safety measure introduced. The following classification of relevant principal measures has been used: (a) Speed limits (new or modified) (LIMIT) (b) Environmental streets (ENVST) (c) Traffic calming (CALM) (d) Humps or other physical measures (HUMP) (e) Police enforcement, traditional (POLIS) (f) Speed cameras (CAM) (g) Driver speed choice, i.e. no particular measure (CHOICE)
Accompanying measure	Any accompanying measure introduced in addition to the main measure. As an example, police enforcement may be increased when speed limits are lowered. Coded by letters as stated above.
Traffic environment	Type of road or traffic environment in which the principal measure is introduced. A distinction was made between: (a) Motorways, freeways (FREE) (b) All purpose rural highway (RURAL) (c) All purpose urban highway (URBAN) (d) Residential access road (RESI) (e) All types of environment (ALL)
Road users involved	Types of vehicles or road users involved in the accidents for which effects have been evaluated. The following main categories are used: (a) All vehicles and all road users (ALL) (b) Motor vehicles (almost) exclusively (CAR)

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Table 5: Data extracted and coded for meta-analysis, continued

Name of variable	Explanation
Accident or injury severity	The severity of accidents or of injuries to road users. The following main categories are used: (a) Fatal accident or injury (FAT) (b) Serious accident or injury (SER) (c) Slight accident or injury (SLI) (d) All injury accidents or injuries (INJ) (e) Property damage only (PDO)
Accidents or victims	Whether effects observed refer to accidents or accident victims, coded as follows: (a) Accidents (ACC) (b) Victims (VIC)
Speed limit – before	Speed limit before the main measure was introduced. Stated in kilometres per hour. If no speed limit was in force, the code NONE is used.
Speed limit – after	Speed limit after the main measure was introduced. Stated in kilometres per hour. If no speed limit was in force, the code NONE is used.
Mean speed – before	Mean speed of traffic before the main measure was introduced. Stated in kilometres per hour.
Mean speed – after	Mean speed of traffic after the main measure was introduced. Stated in kilometres per hour.
Vehicle kilometres in treated group before	Vehicle kilometres of travel in treated (case) group before main measure was introduced (if available; if not left blank)
Vehicle kilometres in treated group after	Vehicle kilometres of travel in treated (case) group after main measure was introduced (if available; if not left blank)
Vehicle kilometres in comparison group before	Vehicle kilometres of travel in comparison (control) group before main measure was introduced (if available; if not left blank)
Vehicle kilometres in comparison group after	Vehicle kilometres of travel in comparison (control) group after main measure was introduced (if available; if not left blank)
Accidents or victims in treated group before	Number of accidents in treated (case) group before the main measure was introduced.
Accidents or victims in treated group after	Number of accidents treated (case) group after the main measure was introduced.
Accidents or victims in comparison group before	Number of accidents in comparison (control) group before the main measure was introduced (if available; otherwise left blank).
Accidents or victims in comparison group after	Number of accidents in comparison (control) group after the main measure was introduced (if available; otherwise left blank).
Relative change in speed	The relative change in mean speed, expressed in terms of the ratio: speed after/speed before.
Relative change in accidents or victims	The relative change in the number of accidents or accident victims, expressed in terms of the simple odds (after/before) or the odds ratio, adjusted for changes in traffic volume when possible.
RTM bias	An assessment of whether the study can be affected by uncontrolled regression-to-the-mean (YES or NO).
Trend bias	An assessment of whether the study can be affected by uncontrolled long-term trends in the number of accidents, victims or accident rates (YES or NO).
Volume bias	An assessment of whether the study can be affected by changes in traffic volume that were not controlled for (YES or NO).
Risk factor bias	An assessment of whether the study can be affected by other risk factors than speed, not controlled for by the study (YES or NO).

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The odds ratio measure of effect is defined as follows:

$$\text{Odds ratio} = \frac{\left(\dfrac{\text{Number of accidents or victims in treated group after}}{\text{Number of accidents or victims in treated group before}}\right)}{\left(\dfrac{\text{Number of accidents or victims in comparison group after}}{\text{Number of accidents or victims in comparison group before}}\right)}$$

The data extracted from all studies is listed in Appendix 1 in the report.

5.4 Case illustrations of data extraction

In order to give the reader an impression of how data were extracted from each study, two examples of data extraction will be given. The examples concern studies made by Upchurch (1989) and Nilsson (2004A).

5.4.1 Case 1: Upchurch 1989

This study presents an evaluation of the effects of the 65 miles per hour speed limit on rural Interstate roads in the state of Arizona in the United States. It was published in Transportation Research Record number 1244, coded as a paper in a scientific journal.

Upchurch first presents data on the effects on speed. Based on Figure 1 in the paper, the mean speed of traffic (employing the 50th percentile of speed as an indicator of mean speed) can be estimated to 95.7 km/h before the speed limit was raised (from 88.5 to 104.6 km/h) and 104.6 km/h after the speed limit was raised. The relative change in speed is 104.6/95.7 = 1.093. For urban Interstates, where the 55 miles per hour speed limit was retained, the paper states that no changes in speed were observed.

The following data on accidents and vehicle miles of travel are presented (see Table 6).

Table 6: Data on accidents and miles of travel presented in Upchurch 1989

	Before speed limit was raised on rural interstates				**After**
Data	**1983**	**1984**	**1985**	**1986**	**1988**
Rural Interstates					
Fatal accidents	71	82	92	97	117
Injury accidents	978	1052	1015	1047	1322
PDO-accidents	1428	1654	1757	1669	1969
Million vehicle miles	3745	3992	4129	4620	4966
Urban Interstates					
Fatal accidents	10	16	13	13	15
Injury accidents	609	750	815	803	737
PDO-accidents	1717	2092	2124	2105	2217
Million vehicle miles	1360	1470	1577	1791	1907

Source: TØI report 740/2004

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This Table presents a wealth of information. Based on the data given, accident rates were estimated for fatal accidents, injury accidents and property-damage-only (PDO) accidents. An examination of these rates for rural interstates showed that the rates did not display any consistent trend during the before period. Moreover, the year-to-year changes in accident rates were quite similar for urban and rural interstates, meaning that urban interstates could serve as a comparison group for rural interstates.

On the basis of these findings, it was decided to add data for all years before. The data that were entered on the EXCEL spreadsheet and how these data were used to extract estimates of effect are shown in Table 7. Effects were estimated as accident rate ratios. To give an example, the estimate of the effect on fatal accidents was defined as follows:

Effect on fatal accidents = [(117/4966)/(342/16486)]/[(15/1907)/(52/6198)]

Table 7: Extraction of data and estimates of effect from Upchurch 1989

	Rural interstates		Urban interstates		
Data	**Before**	**After**	**Before**	**After**	**Effect**
Fatal accidents	342	117	52	15	1.211
Injury accidents	4092	1322	2977	737	1.333
PDO-accidents	6508	1969	8038	2217	1.120
Million vehicle miles	16486	4966	6198	1907	

Estimates of the effects on injury accidents and on PDO-accidents were extracted the same way.

5.4.2 Case 2: Nilsson 2004A

Nilsson (2004A) presents estimates of the relationship between speed and accident rate based on a cross section of roads in Sweden. Figure 10 presents a sample of his findings.

Each data point is a combination of mean speed and fatality rate. A function has been fitted to the data points. According to this function, fatality rate increases in proportion to mean speed (x) raised to a power of 3.89. The squared correlation coefficient is 0.42. As is apparent from Figure 10, the data points are rather widely dispersed around the function fitted to them.

In figure 10, each data point carries the same statistical weight, which is not correct, as the number of accidents underlying each data point varies. The functions fitted by Nilsson have been re-analysed applying weighted regression. The results have been entered as a set of estimates (six in total: fatal accidents, fatalities, serious injury accidents, seriously injured road users; slight injury accidents, slightly injured road users) for the exponent and its standard error. Thus, a cross-sectional data set, from a well-controlled study, like the one shown in Figure 10 is represented by a summary estimate of the exponent, and not by the individual data points.

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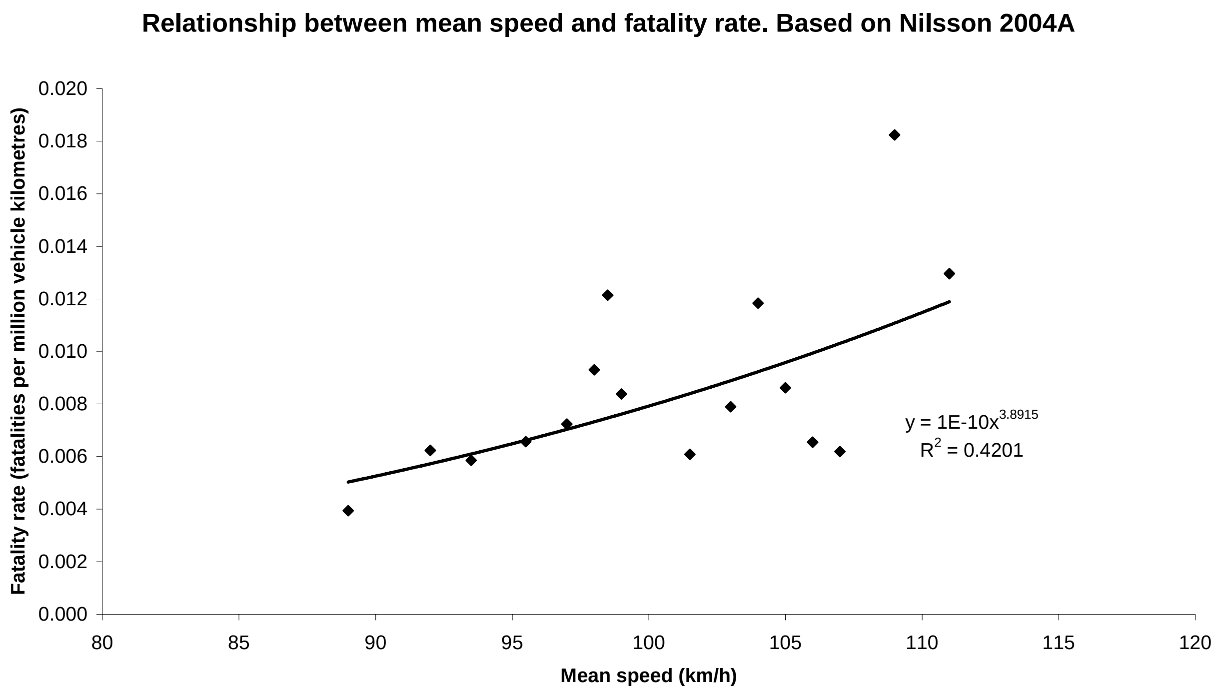

Source: TØI report 740/2004

Figure 10: Fatality rate by mean speed. Derived from Nilsson 2004A

5.5 Study quality assessment

Study quality was assessed by checking for each study whether the following four confounding factors could potentially threaten study validity:

1. Regression-to-the-mean
2. Long-term trends in accidents or accident rates
3. Changes in traffic volume
4. Other risk factors (than speed) that could affect accident occurrence

It was not possible to assess the other threats to validity discussed in Chapter 4, viz. unreliable speed data and incomplete and variable accident reporting. The reason these threats to validity could not be assessed was simply that virtually no study provide any data that can be used as a basis for an assessment. The reliability of speed data, for example, is almost never discussed in studies evaluating the relationship between speed and road safety.

The assessment was basically made by trying to answer the following question: Is lack of control for this confounding factor likely to be a threat to the validity of study findings? The answer was given as yes or no.

The assessment was, to the maximum extent possible, based on data provided by each study. This means that the assessment of study quality is, for the most part, based on how studies have been reported. If, as an example, a study did not report that it controlled for regression-to-the-mean, it was assumed that the study did not control for this factor. Likewise, if a study did not report any data on traffic

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volume, it was assumed not to have controlled for this factor. No study was given the benefit of doubt.

5.5.1 Control for regression-to-the-mean

As noted in Chapter 4, regression-to-the-mean is most likely to threaten the validity of study findings if a measure intended to influence speed is introduced on specific road sections or if it is limited in time. If the measure is system-wide, like changing all speed limits in a country, regression-to-the-mean is less likely to confound a study.

The presence of a potential bias in a study due to regression-to-the-mean was assessed by applying the following rules:

1. Regression to the mean was considered to be a potential source of bias in a study if:

(a) The study was a before-and-after study of a locally applied measure and the study did not explicitly state that it controlled for regression-to-the-mean.

(b) The study was a before-and-after study providing data showing that the treated locations had a (substantially) higher accident rate in the before-period than the comparison locations.

2. Regression-to-the-mean was not considered to be a potential source of bias in a study if:

(a) The study employed a cross-sectional or case-control design.

(b) The study evaluated a system-wide change in speed limits and provided data for the whole system, not just a selected part of it.

These rules do of course not guarantee perfectly correct results, but are the most advanced rules that can be applied, given the quality of the information presented in many reports. Ideally speaking, a more detailed assessment is desirable. In most cases, however, one cannot positively know whether regression-to-the-mean did actually bias a study or not. The closest one can get is to assess whether regression-to-the-mean is likely to have been a source of bias in a study.

5.5.2 Long term trends in accident rates

The assessment of whether long term trends could be a source of bias in a study was based on these rules:

1. Long term trends were considered to be a potential source of bias in a study if:

(a) The study was a simple before-and-after study (no comparison group) relying on data for just one or a few years or other periods (less than four) before the measure was introduced.

(b) The study was a before-and-after study with a comparison group, but relied on just one year of data before and one year of data after.

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2. Long term trends were not considered to be a likely source of bias in a study if:

(a) The study employed a cross-sectional or case-control design.

(b) The study was a before-and-after study with a comparison group relying on at least six years of data for the whole period (before plus after), enabling a test for the presence of a trend to be made.

The rules, again, represent an approximation to an ideal assessment only. Applying these rules do not ascertain whether long term trends that were not controlled for did actually bias a study, only that such trends could have introduced bias into a study.

5.5.3 Changes in traffic volume

The assessment of whether or not a study has controlled for changes in traffic volume was very simple: If the study did provide data on traffic volume (or some indicator of it), enabling accident rates to be calculated, it was assessed as having controlled for changes in traffic volume. In all other cases, a study was assessed as not having controlled for changes in traffic volume, thus making this a potential source of bias in the study.

This simple assessment is not very satisfactory. As noted in Chapter 4, the relationship between traffic volume and the number of accidents is not always linear, as the use of accident rates to control for differences in traffic volume assumes. Relying on accident rates is a very crude way of controlling for traffic volume. It has a long tradition in road safety studies, but is now rapidly being replaced by the use of mathematical functions that do not necessarily assume a linear relationship between traffic volume and the number of accidents.

Unfortunately, the data presented in virtually all the studies that have been reviewed here do not permit the estimation of any mathematical functions that allow for a non-linear relationship between traffic volume and the number of accidents. A second-best solution therefore had to be chosen.

5.5.4 Other risk factors influencing accident occurrence or injury severity

The assessment of whether other risk factors influencing accident occurrence or injury severity have been controlled for was based on study design. If a study employed a cross-sectional or case-control design, it was as a rule considered as not having fully controlled for other risk factors that influence accident occurrence or injury severity. All other study designs were considered as controlling for this potential source of bias.

Adequate control of the very many risk factors that, in addition to speed, influence accident occurrence or injury severity is very difficult to attain in cross-sectional or case-control studies. This is a serious source of bias in cross-sectional or case-control studies. None of the cross-section or case-control studies included in the meta-analysis did control for all the risk factors listed in Chapter 4. However a few studies were judged to be sufficiently well-controlled to be included. These

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were studies that either employed a multivariate technique of analysis in order to form homogeneous groups of road sections (e.g. Taylor Baruya and Kennedy 2002) or that were restricted to a sample of road sections that were homogeneous with respect to important confounding variables (e.g. Nilsson 2004A).

In principle, changes in other risk factors over time could bias before-and-after studies as well, not just cross-sectional or case-control studies. However, many important risk factors tend to change slowly. The distribution of the population by age and gender is a case in point. Many of the risk factors related to road design and traffic control also remain fairly stable over time, such as road alignment. However, these risk factors can vary enormously in space. If you compare speed and accident rate on a hilly, narrow and bending road to speed and accident rate on a flat, wide and straight road, you are likely to find that speed is lower and accident rate higher on the hilly and bending road than on the flat and straight road. This does not mean that lower speeds lead to more accidents. It just means that drivers do not slow down enough to fully compensate for the effects on accident rates of hills, bends and lack of space. Indeed, changes in speed can, in theory, compensate for the effects of very many other risk factors. If such behavioural adaptations were perfect, all roads would have the same accident rate, but very different mean speeds. If this were the case (which it is patently not), it would still be very wrong to conclude that speed does not matter for road safety. In fact, *exactly the opposite conclusion would be correct, namely that speed is such a powerful risk factor that by manipulating it one can virtually remove the effects of any other risk factor*. Thus, the need to control for other risk factors in cross-sectional and case-control studies can hardly be stressed enough.

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6 Meta-analysis

This chapter presents the results of the meta-analysis that has been made of studies evaluating the relationship between speed and road safety. The studies that were identified in the literature search have been sorted into three groups:

1. Studies that were included in the meta-analysis.
2. Studies that did not provide sufficient information to be included in the meta-analysis.
3. Studies that could in principle have been included, but were excluded from the meta-analysis because they were judged not to control adequately for confounding factors or relied on data that may contain systematic biases.

Studies in each of these groups are listed below. Following the list of studies, an overview of evidence is given, before the meta-analysis is presented.

6.1 Studies included in meta-analysis

A total of 98 studies providing a total of 460 estimates of effect have been included in the meta-analysis. These studies, including some characteristics of each study, are listed in Table 8. The studies are listed chronologically. Studies published the same year are listed alphabetically within that year.

The mean number of estimates extracted from each study was 460/98 = 4.69. The number of estimates per study varied from 1 to 29. The publication years of the studies varied from 1966 to 2004.

When extracting multiple estimates of effect from the same study, estimates for which the count of accidents or injuries was zero in one of the cells of the data table were not included. Results based on zero counts were omitted to avoid the problem of having to adjust for zero counts as part of meta-analysis. The conventional way of adjusting for zero counts, by adding 0.5 to each cell in the data table, has been found to give biased summary estimates of effect in meta-analysis (Sweeting, Sutton and Lambert 2004).

451 out of the 460 estimates provided data on both changes in speed and changes in the number of accidents or accident victims. 9 estimates were summary estimates of the exponent of the Power Model, extracted either directly from a study or by means of a re-analysis, explained in section 5.4.2 for the study of Nilsson (2004A).

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Table 8: Studies included in meta-analysis

Authors	Year	Country	Measure evaluated	Number of estimates of effect
Munden	1966	Great Britain	Speed enforcement	12
Ekström et al	1967	Sweden	Speed enforcement	2
Hall et al	1970	Ireland	General speed limits	2
Jönrup and Svensson	1971	Sweden	Local speed limits	20
Rutley	1972	Great Britain	Recommended speed	7
Wahlgren	1972	Finland	Seasonal speed limits	3
Andersson and Nilsson	1974	Sweden	Local speed limits	3
Brodersen et al	1975	Denmark	General speed limits	3
Brodin and Ringhagen	1975	Sweden	Local speed limits	1
Burritt et al	1976	United States	General speed limit	1
Nilsson	1976	Sweden	Local speed limits	12
Scott and Barton	1976	Great Britain	General speed limits	2
Kemper and Byington	1977	United States	General speed limit	6
Brackett and Beecher	1980	United States	Speed enforcement	3
Daltrey and Healy	1980	Australia	General speed limit	4
Nilsson	1980	Sweden	Seasonal speed limits	6
Roop and Brackett	1980	United States	Speed enforcement	18
Amundsen	1981	Norway	Local speed limits	2
Christensen	1981	Denmark	General speed limits	2
Koshi and Kashima	1981	Japan	Local speed limit	3
Salusjärvi	1981	Finland	Local speed limits	24
Baguley	1982	Great Britain	Speed humps	7
Frith and Toomath	1982	New Zealand	General speed limit	4
Salusjärvi	1982	Norway	Local speed limits	2
Amundsen	1983	Norway	Local speed limits	1
Borges et al	1985	Denmark	Environmental streets	5
Jørgensen et al	1985	Nordic countries	General speed limits	4
Sakshaug	1986	Norway	Local speed limits	4
Engel	1987	Denmark	General speed limit	1
Ullman and Dudek	1987	United States	Local speed limits	12
Dietrich et al	1988	Switzerland	General speed limits	8
Engel and Thomsen	1988	Denmark	General speed limit	3
Salusjärvi and Mäkinen	1988	Finland	Speed enforcement	4
Stølan	1988	Norway	Environmental streets	2

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Table 8: Studies included in meta-analysis, continued

Authors	Year	Country	Measure evaluated	Number of estimates of effect
Gallaher et al	1989	United States	General speed limit	1
McCartt and Rood	1989	United States	Speed enforcement	6
Pigman et al	1989	United States	Unmanned radar	4
Rijkswaterstaat	1989	Netherlands	General speed limit	2
Upchurch	1989	United States	General speed limit	3
US Dept of Transportation	1989	United States	General speed limit	2
Brown et al	1990	United States	General speed limit	6
Engel and Thomsen	1990	Denmark	Speed humps	2
Giæver and Meland	1990	Norway	Speed humps	3
Nilsson	1990	Sweden	Seasonal speed limits	4
Roszbach	1990	Netherlands	General speed limit	1
Sidhu	1990	United States	General speed limit	6
Smith	1990	United States	General speed limit	2
Andersson	1991	Sweden	Speed enforcement	6
Angenendt	1991	Germany	Environmental streets	2
Freiholtz	1991	Sweden	Environmental streets	2
Jernigan and Lynn	1991	United States	General speed limit	1
Baier	1992	Germany	Speed limit zones	2
Baier et al	1992	Germany	Environmental streets	2
Godwin	1992	United States	General speed limit	2
Oei and Polak	1992	Netherlands	Speed cameras	4
Nilsson	1992	Sweden	Speed cameras	4
Schnüll and Lange	1992	Germany	Environmental streets	3
Sliogeris	1992	Australia	General speed limit	2
Aakjer-Nielsen and Herrstedt	1993	Denmark	Environmental streets	6
Herrstedt et al	1993	Denmark	Environmental streets	29
Engel and Andersen	1994	Denmark	Environmental streets	1
Sammer	1994	Austria	Local speed limits	2
Rock	1995	United States	General speed limit	3
Wheeler and Taylor	1995	Great Britain	Environmental streets	6
ETSC	1996	Denmark	Speed humps	10
Griborn	1996	Sweden	Environmental streets	1
Webster and Mackie	1996	Great Britain	Speed humps	3
Liu and Popoff	1997	Canada	Driver speed choice	1
Parker	1997	United States	Local speed limits	28

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Table 8: Studies included in meta-analysis, continued

Authors	Year	Country	Measure evaluated	Number of estimates of effect
Aljanahi et al	1999	Bahrain	Driver speed choice	1
Antov and Roivas	1999	Estonia	Seasonal speed limits	2
Buss	1999	Germany	Temporary lane	1
Eriksson and Agustsson	1999	Denmark	Environmental streets	1
Farmer et al	1999	United States	General speed limit	1
Lamm et al	1999	Germany	Speed cameras	3
Wheeler and Taylor	1999	Great Britain	Environmental streets	6
Andersson	2000	Sweden	Speed enforcement	2
Andersson	2000	Sweden	Local speed limits	4
Kronberg and Nilsson	2000	Sweden	Speed cameras	3
Peltola	2000	Finland	Seasonal speed limits	4
Wretling	2000	Sweden	Local speed limits	4
Abel and Matthes	2001	Germany	Local speed limits	17
Agustsson	2001	Denmark	Environmental streets	1
Burns et al	2001	Great Britain	Local speed limits	2
Keall et al	2001	New Zealand	Speed cameras	2
Ossiander and Cummings	2002	United States	General speed limit	1
Pez	2002	Germany	Speed enforcement	4
Taylor et al	2002	Great Britain	Driver speed choice	1
Andersson	2003	Sweden	Speed cameras	6
Goldenbeld et al	2003	Netherlands	Speed enforcement	1
Grendstad et al	2003	Norway	Environmental streets	6
Myrup and Agustsson	2003	Denmark	Speed cameras	6
Varhelyi et al	2003	Sweden	ISA trial	1
Nilsson	2004	Sweden	Driver speed choice	6
Ragnøy	2004	Norway	Speed limits	4
Richter et al	2004	Israel	Speed limits	1
Stuster	2004	United States	Speed enforcement	14
Vernon et al	2004	United States	Speed limits	3

6.2 Studies not providing information for inclusion in meta-analysis

Studies that could not be included in the meta-analysis because they did not provide the data needed are listed chronologically in Table 9. For each study, the main reason (there may be more than one reason) it was not included in the meta-analysis is stated.

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Table 9: Studies excluded from meta-analysis due to incomplete information

Author	Year	Country	Reason for exclusion
California Highway Patrol	1966	United States	No data on changes in speed
Wehner	1966	Germany	No data on changes in speed
Campbell and Ross	1968	United States	No data on changes in speed
Newby	1970	Great Britain	No data on changes in speed
Arbeitsgruppe Tempo 100	1975	Switzerland	No data on changes in speed
Salusjärvi	1975	Finland	No data on changes in speed
Salusjärvi	1977	Finland	No data on changes in speed
Zaremba and Ginsburg	1977	United States	Accident severity not stated
Carr, Schnelle, Kirchner	1980	United States	No data on changes in speed
Sali	1983	United States	No data on changes in speed
Marburger and Ernst	1986	Germany	No data on changes in speed
Smith	1986	Great Britain	No data on changes in speed
Kearns and Webster	1988	Australia	No data on changes in speed
Müller	1989	Germany	No data on changes in speed
Baum et al	1990	United States	No data on changes in speed
Carlsen and Svendsen	1990	Norway	No data on changes in speed
Chang and Paniati	1990	United States	No data on changes in speed
Harkey et al	1990	United States	Number of accidents not stated
Helfenstein	1990	Switzerland	No data on changes in speed
McKnight and Klein	1990	United States	Imprecise data on the number of accidents
Wagenaar et al	1990	United States	No data on changes in speed
Baum et al	1991	United States	No data on changes in speed
Fildes et al	1991	Australia	Accident severity not stated
Peltola	1991	Finland	Accident severity not stated
Pfefer et al	1991	United States	Imprecise data on the number of accidents
Faure and de Neuville	1992	France	No data on changes in speed
Garber and Gadiraju	1992	United States	Number of accidents not stated
Pant et al	1992	United States	No data on changes in speed
Schnüll and Haller	1992	Germany	No data on changes in speed
Vis, Dijkstra and Slop	1992	Netherlands	Imprecise data on the number of accidents
Chang, Chen and Carter	1993	United States	No data on changes in speed
Joksch	1993	United States	Number of accidents not stated
Kallberg	1993	Finland	Incomplete data on changes in speed
Mackie et al	1993	Great Britain	No data on changes in speed
Pasanen and Salmivaara	1993	Finland	Number of accidents not stated
Webster	1993	Great Britain	No data on changes in speed
Baruya and Finch	1994	Great Britain	Number of accidents not stated

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Table 9: Studies excluded from meta-analysis due to incomplete information, continued

Author	Year	Country	Reason for exclusion
McCarthy	1994	United States	No data on changes in speed
Blackburn and Gilbert	1995	United States	Speed data and accident data incompatible
Gledec	1995	Croatia	Number of accidents not stated
Hantula	1995	Finland	No data on changes in speed
Newstead et al	1995	Australia	No data on changes in speed
Sharif, Al-Sharif	1995	Jordan	No data on changes in speed
Holland and Conner	1996	Great Britain	Number of accidents not stated
Johansson	1996	Sweden	No data on changes in speed
Schmidt	1996	Germany	Number of accidents not stated
Statens vegvesen	1996	Norway	No data on changes in speed
Anderson et al	1997	Australia	Number of accidents not stated
Svenska kommunförbundet	1997	Austria	No data on changes in speed
Agent et al	1998	United States	Imprecise data on the number of accidents
Maycock et al	1998	Great Britain	Accident severity not stated
US Dept of Transportation	1998	United States	No data on changes in speed
Quimby et al	1999	Great Britain	Accident severity not stated
Renski et al	1999	United States	No data on changes in speed
Chen et al	2000	Canada	Accident severity not stated
Garber and Ehrhart	2000	United States	Number of accidents not stated
Nilsson and Obrenovic	2000	Sweden	Number of accidents not stated
Wheeler and Taylor	2000	Great Britain	Incomplete data on changes in speed
Balkin and Ord	2001	United States	No data on changes in speed
Ewing	2001	United States	Accident severity not stated
Woolley et al	2001	Australia	Number of accidents not stated
Chen et al	2002	Canada	Accident severity not stated
Kang	2002	South Korea	Speed data and accident data incompatible
Newstead et al	2002	Australia	No data on changes in speed

A total of 64 studies that have been retrieved, but were not included in the meta-analysis, are listed in Table 9. This is a substantial number of studies. It is therefore necessary to discuss whether the exclusion of these studies can introduce bias in the analysis. This will be discussed in Chapter 7.

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6.3 Studies that have been excluded from meta-analysis

A few studies, listed in Table 10, were omitted from the meta-analysis, although they could have been included, since the relevant data were provided. The reasons for omitting these studies will be described.

Table 10: Studies excluded from meta-analysis due to definition of dependent variable, measurement error or inadequate control of confounding factors

Author	Year	Country	Reason for exclusion
Solomon	1964	United States	Potentially systematic errors in measurement
Munden	1967	Great Britain	Potentially systematic errors in measurement
Cirillo	1968	United States	Potentially systematic errors in measurement
West and Dunn	1971	United States	Potentially systematic errors in measurement
Nilsson	1984	Sweden	Not relevant definition of dependent variable
Evans	1994	United States	Not relevant definition of dependent variable
Moore et al	1995	Australia	Potentially systematic errors in measurement
Kloeden et al	1997	Australia	Potentially systematic errors in measurement
Baruya	1998	Europe	Inadequate control for confounding factors
Taylor et al	2000	Great Britain	Inadequate control for confounding factors
Kloeden et al	2001	Australia	Potentially systematic errors in measurement
Keall, Povey and Frith	2002	New Zealand	Duplicates study from 2001 already included
Carlsson	2003	Sweden	Not relevant definition of dependent variable

A series of studies (Solomon 1964; Munden 1967; Cirillo 1968; West and Dunn 1971; Moore et al 1995; Kloeden et al 1997; Kloeden et al 2001) have employed a case-control design to study the relationship between driver speed choice and accident involvement. Most of these studies have found a U-shaped relationship between speed and accident involvement: drivers who drive slower than the mean speed of traffic, and drivers who drive faster than the mean speed of traffic are more often involved in accidents than drivers who drive close to the mean speed of traffic.

As pointed out by White and Nelson (1970), Hauer (2003) and others, there is a very real possibility that the findings of these studies are more or less artefacts, created by error in the estimate of speed for cars involved in accidents (the case group). In view of the serious doubts that can be raised about the validity of these studies, it was decided not to include them in the meta-analysis.

A second group of studies (Nilsson 1984; Evans 1994; Carlsson 2003) study the relationship between either the mean speed of traffic (Nilsson 1984; Carlsson 2003) or impact speed (Evans 1994) and the severity of injuries sustained in accidents. As noted in the discussion of problems related to the construct validity of testing the Power Model, impact speed is not relevant. In studies using the mean speed of traffic, the problem is that the dependent variable has not been defined the same way as in the Power Model. The Power Model refers to, as an example, fatal accidents, the number of fatalities or the number of fatalities per

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fatal accident. However, in both the studies of Nilsson (1984) and Carlsson (2003), the dependent variable is fatalities per injury accident. This definition of the dependent variable is inconsistent with the Power Model. Hence, these studies were omitted from the meta-analysis.

Finally a third group of studies (Baruya 1998; Taylor et al 2000; Keall, Povey and Frith 2002) have been omitted either because the studies were judged not to control adequately for confounding variables (see the discussion of this with respect to Baruya 1998 and Taylor et al 2000 in Taylor et al 2002) or because they duplicated another study (Keall, Povey and Frith 2002).

In total, 13 studies have been excluded from the meta-analysis.

6.4 Overview of evidence from studies included in meta-analysis

Before presenting the results of the meta-analysis, an overview will be given of evidence contained in the studies that were included in this analysis. Table 11 gives an overview of the distribution of estimates of effect with respect to some selected descriptive variables. The total is 460 (equal to the number of results) for all these variables.

Data from 20 countries or groups of countries were included. The largest number of estimates of the relationship between speed and road safety came from the United States. Other countries contributing substantially to the evidence are Sweden, Denmark and Great Britain.

Studies from the years 1966 to 2004 were included. Studies made after 1990 represent more than 50% of the evidence. The mean number of results found per year has grown from 1.4 in the nineteen sixties to 20.7 after 2000. Nearly all studies have employed a before-and-after design. There are few experimental studies (randomised controlled trials).

The variable labelled “sources of bias” refers to whether studies can reasonably be assumed to have controlled for regression-to-the-mean, long-term trends, changes in traffic volume or other risk factors (than speed) affecting accident occurrence or injury severity. A total of 157 estimates were classified as being free of all these sources of bias, i.e. as controlling for all of them. Studies that did not control for any of the four potential sources of bias have not been included.

The most commonly used measure to influence speed in this data set is speed limits.

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Table 11: Descriptive statistics for studies included in meta-analysis. Number of results

Variable	Values of variable	Number	Percentage
Country	Australia	6	1.3
	Austria	2	0.4
	Bahrain	1	0.2
	Canada	1	0.2
	Denmark	68	14.8
	Estonia	2	0.4
	Finland	32	7.0
	France	2	0.4
	Germany	31	6.7
	Great Britain	46	10.0
	Ireland	2	0.4
	Israel	1	0.2
	Japan	3	0.7
	Netherlands	8	1.7
	New Zealand	6	1.3
	Nordic countries (mixed)	4	0.9
	Norway	24	5.2
	Sweden	87	18.9
	Switzerland	8	1.7
	United States	126	27.4
Decade of publication	1960-1969	14	3.0
	1970-1979	60	13.1
	1980-1989	134	29.1
	1990-1999	159	34.6
	2000-2004	93	20.2
Study design	Experimental designs	6	1.3
	Before-and-after designs	439	95.4
	Cross-sectional designs	13	2.8
	Time-series analyses	2	0.4
Sources of bias	No source present	157	34.1
	One source present	92	20.0
	Two sources present	144	31.3
	Three sources present	67	14.6
Main measure	Speed limits	245	53.3
	Traffic engineering	102	22.2
	Police enforcement	104	22.6
	Driver speed choice	9	2.0

Source: TØI report 740/2004

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6.5 Exploratory meta-analysis

There are three stages of meta-analysis: (1) Exploratory analysis, (2) Main analysis, and (3) Sensitivity analysis. The purpose of an exploratory meta-analysis is to explore distributions and patterns in the data for the purpose of determining whether a meta-analysis makes sense. The purpose of a meta-analysis is to estimate one or more summary estimates of effect based on a set of estimates provided by the studies included in the meta-analysis. For a summary estimate of effect to be informative, the distribution of the individual estimates of effect around the summary estimate should ideally be:

1. Unimodal, that is have a peak close to the summary estimate
2. Symmetric, that is not have a longer tail in one direction that in the other
3. Unaffected by clearly outlying data points, which can unduly influence the estimate of the summary mean

6.5.1 Two ways of representing data concerning speed and road safety

There are two ways of summarising the relationship between changes in speed and changes in road safety. One of them is to plot the results of all studies in an X-Y diagram and estimate a line summarising the statistical relationship between X (changes in speed) and Y (changes in the number of accidents or accident victims). This will be referred to as the regression approach.

Another way of representing the results of studies evaluating the relationship between speed and road safety, is to estimate the power of the effect of speed on road safety for each data point. Each estimate of the relationship between speed and accidents is now represented by a single figure: the estimate of power. Estimates of power can be combined by applying standard techniques of meta-analysis. This will be referred to as the meta-analysis approach.

Mathematically speaking, these ways of representing the data are identical, but they do not lend themselves equally easily to the use of formal tests based on the so called funnel plot, which is an important aid for exploratory meta-analysis (Elvik 1998). A funnel plot is a plot of estimates of effect (X-axis) versus the statistical precision (Y-axis) of those estimates. If the distribution of estimates is "well-behaved" (unimodal, symmetric and without outlying data points), it should resemble a funnel turned upside down. By analysing the funnel plot, one can detect the presence of publication bias in the data. Publication bias denotes a tendency not to publish results that are believed to be uninformative or hard to explain. In road safety evaluation research, publication bias could materialise in the form of not publishing results that are not statistically significant or not publishing results that go against conventional wisdom, showing, for example, that an increase in speed is associated with a reduction in the number and severity of accidents.

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Funnel plots are most easily produced from estimates of power. Hence, this way of representing the data will be used to probe for the possible presence of publication bias. The main approach to analysis is therefore the meta-analysis approach.

6.5.2 The consistency of the relationship between speed and road safety

The first test to be made concerns the consistency of the relationship between speed and road safety. If the Power Model is correct, one would expect that increases in speed are always associated with an increase in the number of accidents and injury severity, everything else remaining equal. Conversely, a reduction in speed should always be associated with a reduction in the number of accident and injury severity. Table 12 presents data that are relevant to these predictions.

Table 12: The consistency of the relationship between speed and road safety

	Change in accidents or accident victims		
Change in speed	**Down**	**No change**	**Up**
Down	*267*	2	84
Up	36	0	*71*

Source: TØI report 740/2004

All 460 estimates are included in Table 12. In 353 cases, speed went down. In 267 of these cases, the number of accidents or accident victims was also reduced. Thus, 75.6% of the observations are consistent with the Power Model as far as the direction of impact is concerned. Speed went up in 107 cases. In 71 of these cases, corresponding to 66.1%, did the number of accidents or accident victims also increase. Overall, 338 of 460 estimates (73.5%) indicated a direction of the relationship between speed and road safety which is consistent with the Power Model.

This test is, however, quite weak. It relies on simply counting the number of estimates (vote counting), and disregards the fact that some estimates are more precise than others, being based on larger accident samples and larger changes in speed. In Table 13, estimates of the relationship between changes in speed and changes in road safety have been classified according to the relative contributions of the statistical weights of the estimates.

Table 13: Consistency of relationship between speed and road safety indicated by the distribution of statistical weights of estimates – percentages

	Change in accidents or accident victims		
Change in speed	**Down**	**No change**	**Up**
Down	*95.0%*	0.0%	5.0%
Up	29.5%	0.0%	*70.5%*

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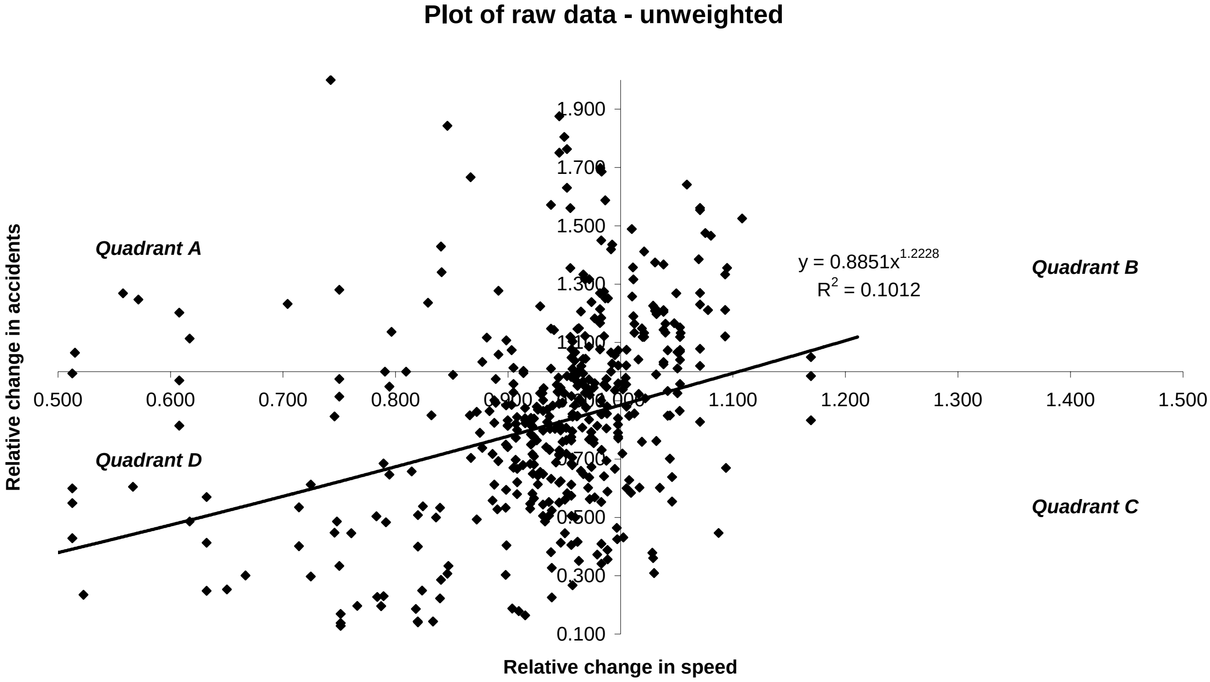
Source: TØI report 740/2004

For reductions in speed, 95% of the statistical weights of the estimates are consistent with the Power Model, by showing a reduction in the number of accidents or accident victims. For increases in speed, the corresponding percentage of the statistical weights of estimates consistent with the Power Model is 70.5%. Considering all estimates of effect, 94.1% of the statistical weights are consistent with the Power Model as far as the direction of impact is concerned.

In other words: *When speed increases, it is very often the case that the number and severity of accidents also increases. When speed is reduced, it is nearly always the case that the number of accidents and the severity of injuries are also reduced.* There is a very systematic pattern in the results indicating that a meta-analysis can be informative.

The effects if increases in speed are not as consistent as the effects of reductions in speed. This shows that it is, to some extent, possible to compensate for increases in speed by adopting other safety measures. Such measures have been introduced in some of the studies that have evaluated, e.g. raised speed limits. For a recent example of this, see Amundsen, Roald and Engebretsen (2004).

To shed further light on the consistency of the direction of the effect of changes in speed on accidents or accident victims, Figure 11 has been produced. Figure 11 shows the simple bivariate relationship between changes in speed and changes in road safety (termed "accidents" in Figure 11). Relative changes in speed are plotted on the X-axis, and have been truncated at the values of 0.5 and 1.5 (for ease in reading the figure). Relative changes in accidents are plotted on the Y-axis, and have been truncated at 0.1 (90% reduction) and 1.9 (90% increase), to make the figure more readable. A line has been fitted to the data points.



Source: TØI report 740/2004

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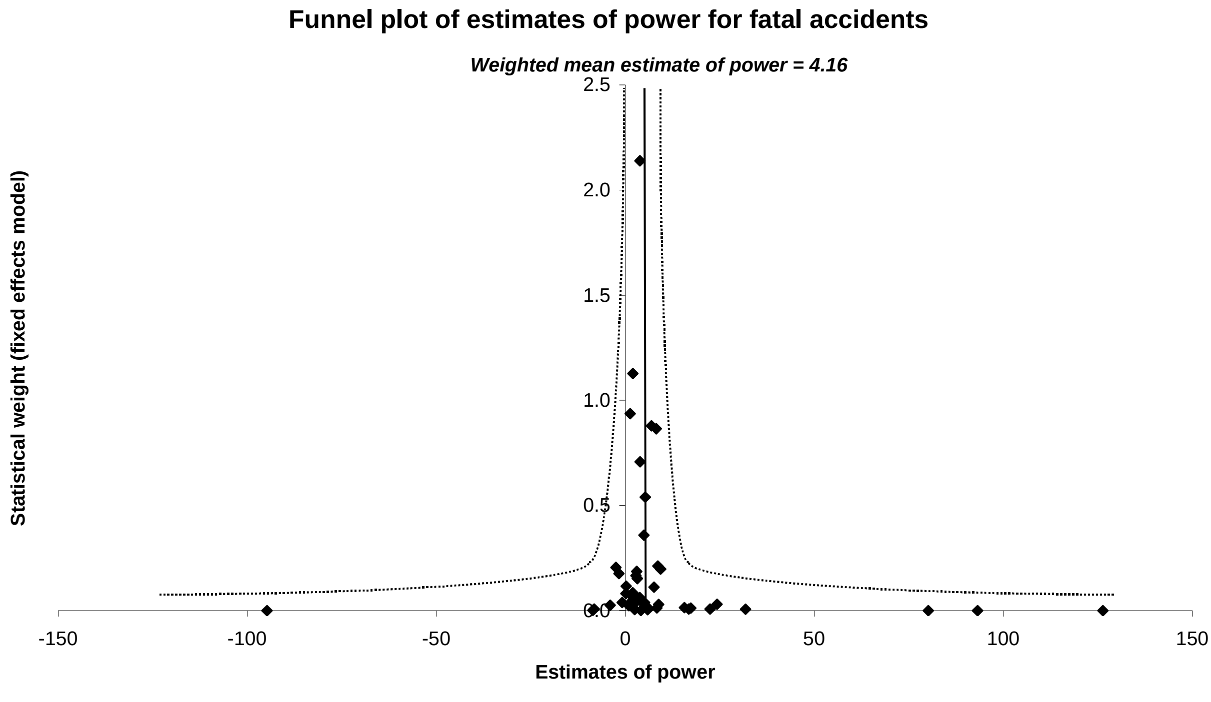
Figure 11: Simple bivariate relationship between changes in speed and changes in accidents

As can be seen from Figure 11, there is a clear relationship between changes in speed and changes in accidents. The function fitted to the data points has a power of about 1.22. This is illustrative only. In Figure 11, accidents of all levels of severity have been mixed up. Each data point carries the same weight, which is obviously wrong. Despite these limitations, the figure does indicate that it is fruitful to probe the relationship between changes in speed and changes in road safety more carefully.

6.5.3 Analysis of funnel plots

The Power Model postulates different values for the exponent according to accident severity and according to whether data refer to accidents or accident victims. In keeping with this, separate funnel plots have been prepared for each of the categories the Power Model identifies. The first funnel plot, presented in Figure 12, refers to changes in fatal accidents.



Source: TØI report 740/2004

Figure 12: Funnel plot of estimates of power for fatal accidents

The funnel plot shown in Figure 12 contains 47 data points. 7 of these indicate a negative power, i.e. an effect in the opposite direction of that predicted by the Power Model. The estimates of power range from a low of about –95 to a high of about +126. The weighted mean value of the exponent is 4.16. This value is close to the values that have the greatest statistical weights. An explanation of how the statistical weights have been estimated is given in the next section of the report.

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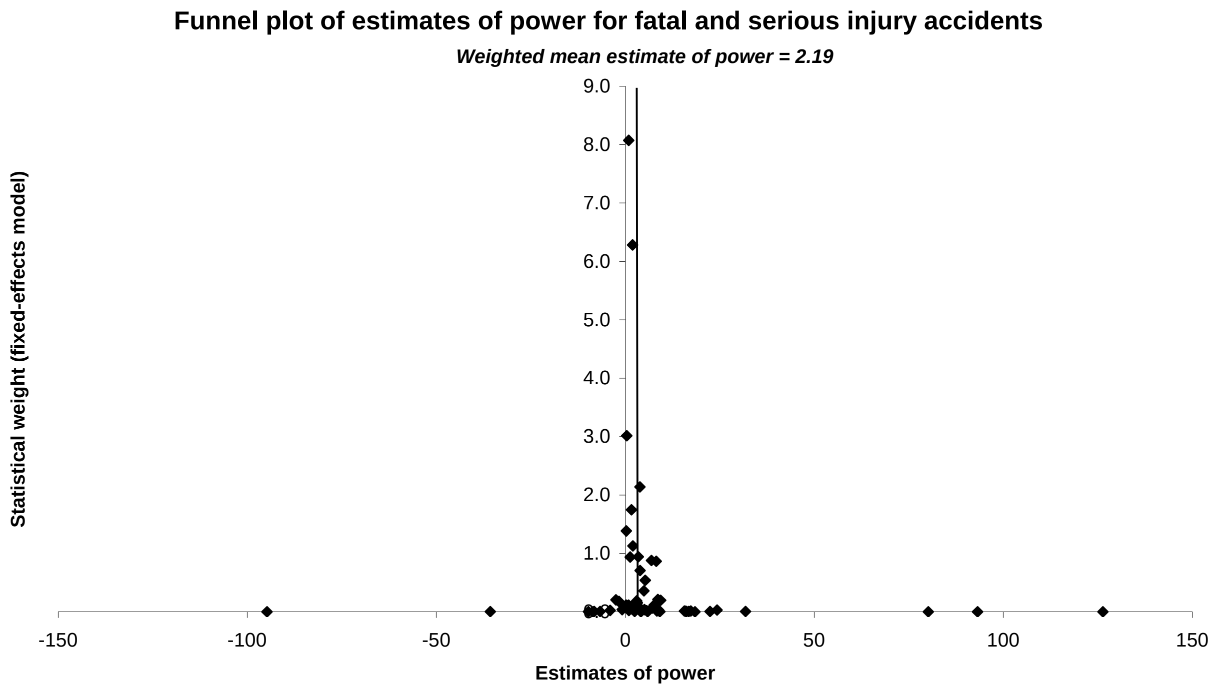
The distribution is clearly unimodal. The distribution of the individual estimates of power around the weighted mean is symmetrical; 25 estimates are below the summary mean estimate, 22 estimates are above it. There are no clearly outlying data points. Hence, this funnel plot indicates that a summary estimate of power is informative.

The potential presence of publication bias is discussed in a later section, once the funnel plots for all the categories of the Power Model have been presented.

The next funnel plot, presented in figure 13, refers to fatal and serious injury accidents. Recall that the Power Model is cumulative: it proposes exponents for fatal, fatal and serious injury and all injury accidents (including fatal and serious).

There are 64 data points in Figure 13. The summary estimate of power is 2.19. The funnel plot is symmetrical, unimodal and does not contain any clearly outlying data points.



Source: TØI report 740/2004

Figure 13: Funnel plot of estimates of power for fatal and serious injury accidents

Figure 14 presents a funnel plot for all injury accidents.

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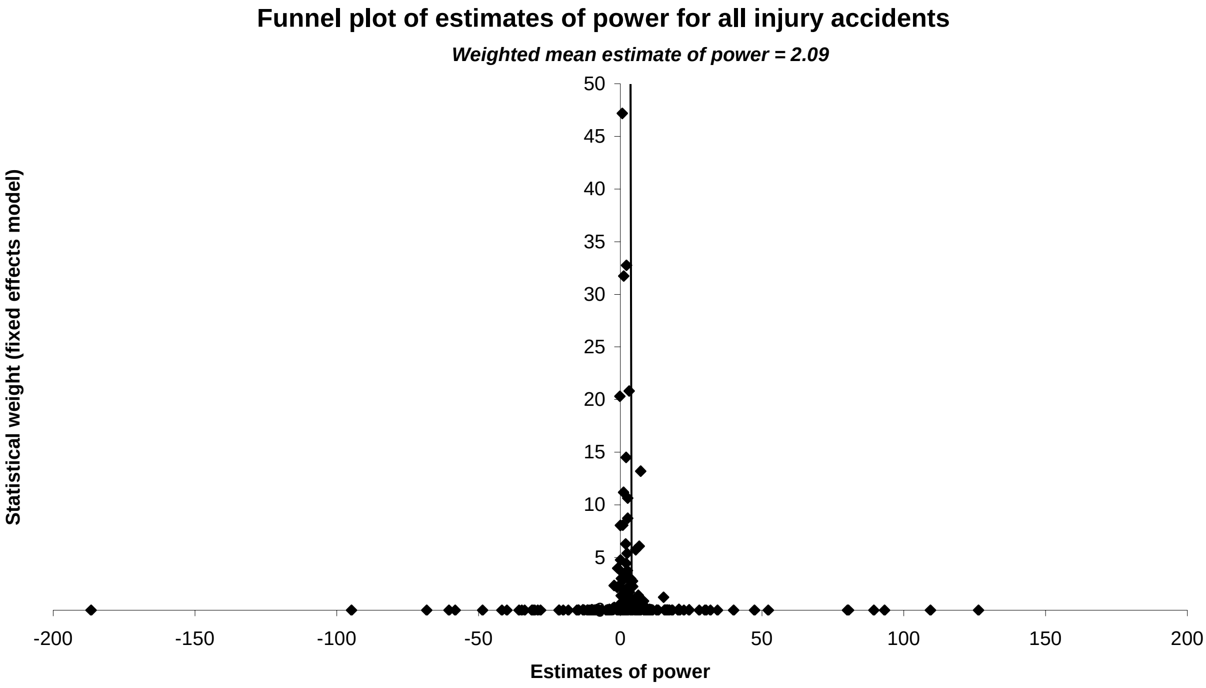
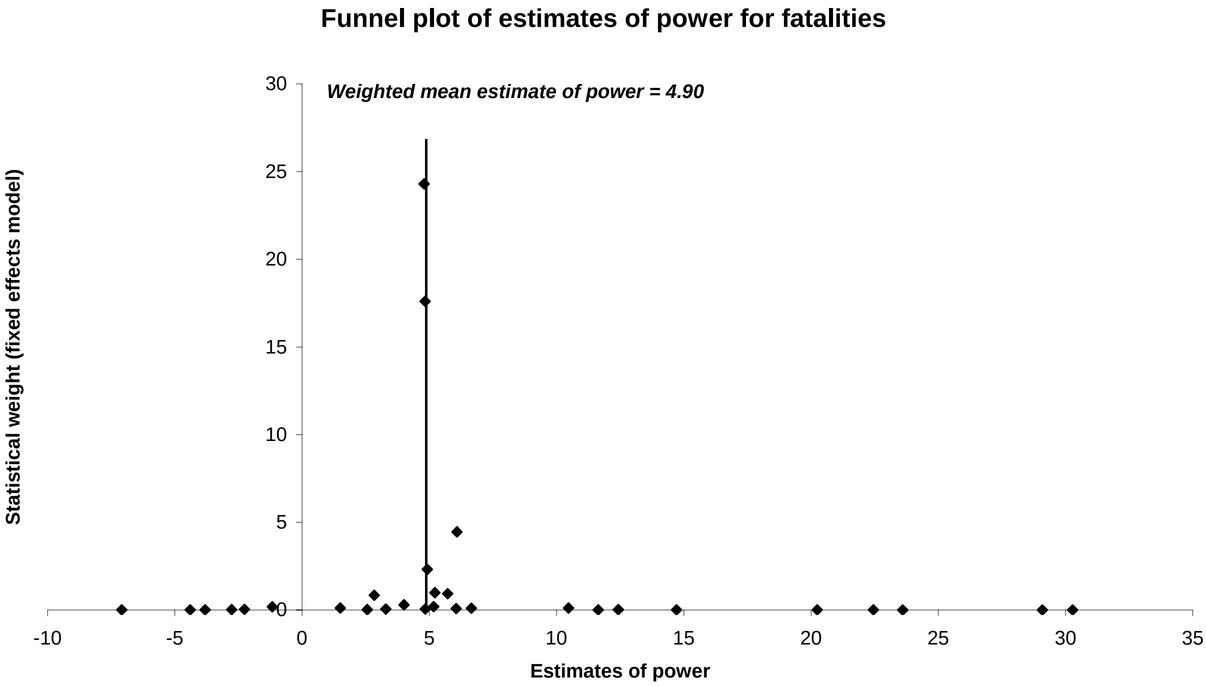


Source: TØI report 740/2004

Figure 14: Funnel plot of estimates of power for all injury accidents

Figure 15 is a funnel plot for fatalities.



Source: TØI report 740/2004

Figure 15: Funnel plot of estimates of power for fatalities

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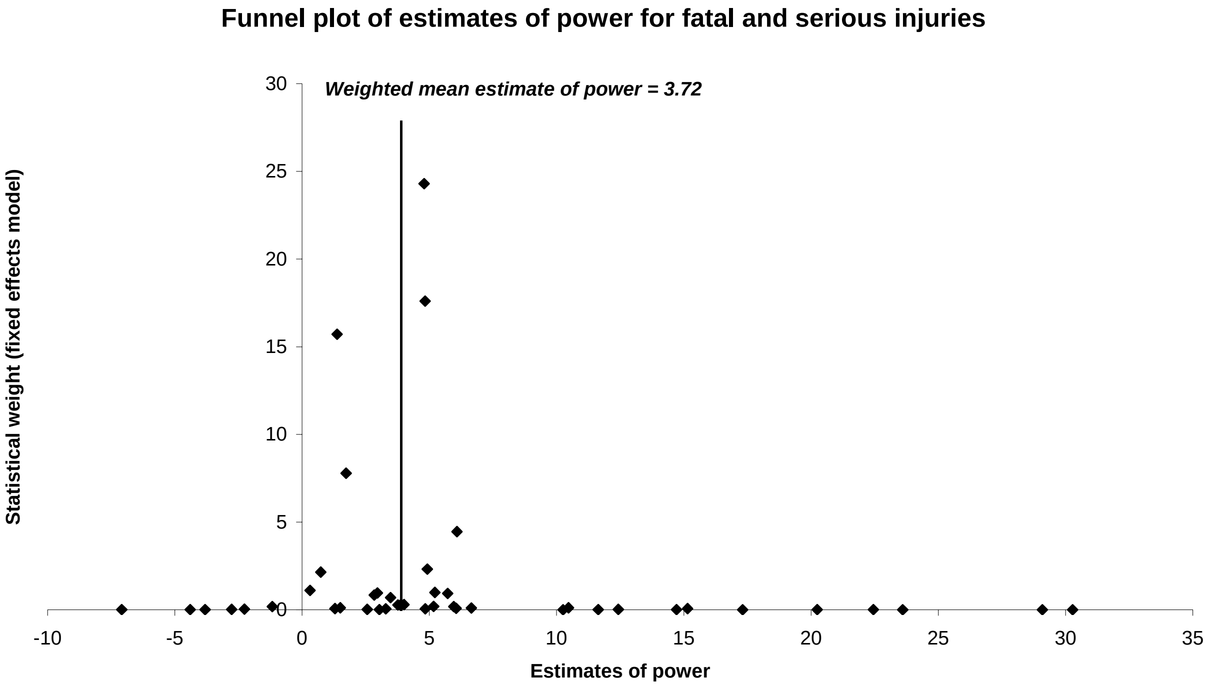
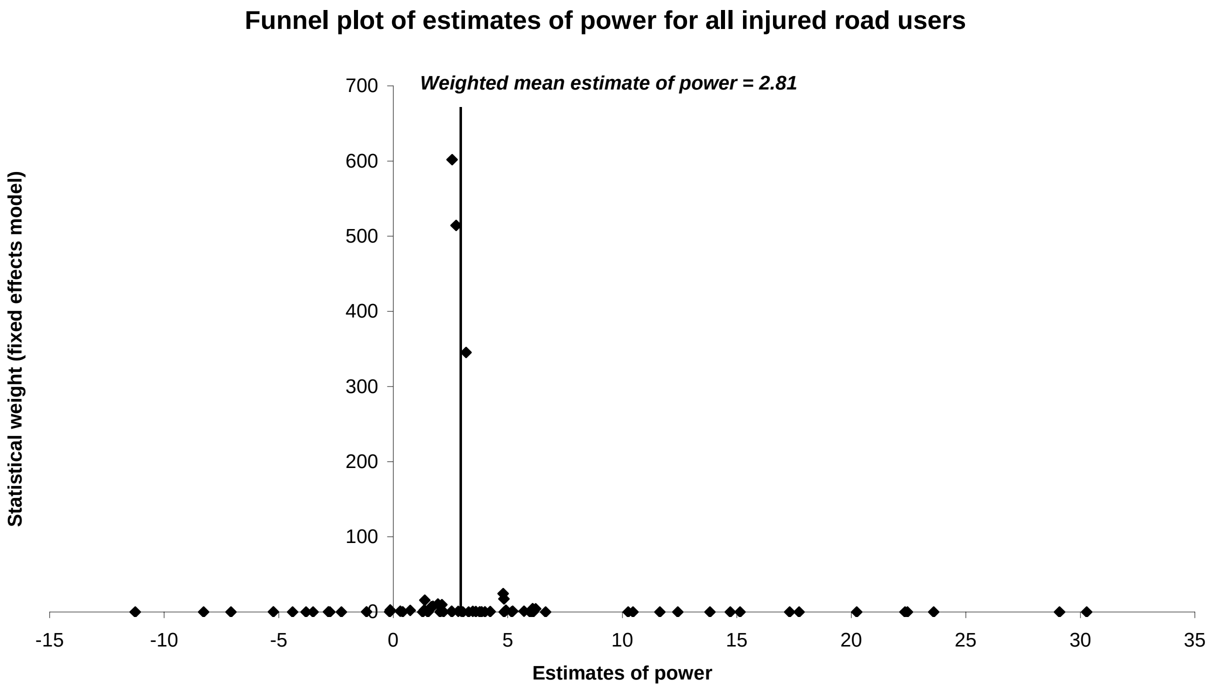


Source: TØI report 740/2004

Figure 16: Funnel plot of estimates of power for fatal and serious injuries



Source: TØI report 740/2004

Figure 17: Funnel plot of estimates of power for all injured road users

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By visual inspection, all these funnel plots appear to be well-behaved and indicate that a meta-analysis makes sense. Formal tests for publication bias have been performed and are reported below.

6.5.4 Testing for publication bias

The possible presence of publication was tested for by means of the trim-and-fill technique (Duval and Tweedie 2000A, 2000B). This is a simple method, which is based on the funnel plot. The method relies on the assumption that if there is no publication bias, the data points in a funnel plot ought to be symmetrically distributed around the summary mean. If a funnel plot is missing one of its tails, this indicates publication bias. The trim-and-fill method then first “cuts off” the one remaining tail of the distribution and then re-estimates the summary mean estimate of effect. Then, the cut-off tail is put back in place, along with “filled in” data points that are the mirror image of the data points that were cut off in the trim part of the analysis. A trim-and-fill analysis both diagnoses publication bias and corrects for it.

A trim-and-fill analysis was applied to the following categories of results:

1. Fatal accidents
2. Serious injury accidents
3. Slight injury accidents
4. Injury accidents, severity not stated
5. Property-damage-only accidents
6. Fatalities
7. Seriously injured road users
8. Slightly injured road users
9. Injured road users, severity not stated

Evidence for publication bias was found in only two of these groups, injury accidents (group 4) and injured road users (group 5). There are 222 estimates of power for injury accidents. The summary estimate of power, based on a fixed-effects model (see below), is 2.586. The trim-and-fill analysis added 8 data points. The adjusted summary estimate of power was 2.587, which is identical to the unadjusted summary estimate to the third decimal place. The random-effects unadjusted summary estimate of power was 2.761. This changed to 2.779 when adjusted for publication bias.

There are 15 estimates of power for injured road users. The fixed-effects summary estimate, not adjusted for publication bias, is 2.780. The trim-and-fill analysis resulted in the addition of 1 data point. The adjusted fixed-effects summary estimate of power was 2.781. The unadjusted and adjusted random-effects summary estimates of power were, respectively, 2.971 and 3.026.

Based on this analysis, it is concluded that there is little evidence of publication bias in the data. The effects of publication bias in the two groups for which there was an indication of such bias were very small.

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6.6 Main analysis

A distinction can be made between a conventional meta-analysis and a meta-regression analysis. A conventional meta-analysis does not rely on multivariate techniques of analysis, but simply combines a set of estimates of effect by weighting each estimate of effect in inverse proportion to its sampling variance. The results of the conventional meta-analyses will be reported first, then the results of the meta-regression analysis.

6.6.1 Statistical weighting of estimates of effect

According to the inverse-variance method of meta-analysis, the findings of a set of studies are combined by applying statistical weights that are inversely proportional to the sampling variance of each estimate of effect (Fleiss 1993; Shadish and Haddock 1994). Let G denote the total number of studies to be combined, with g representing a typical study. Let Y denote the estimate of effect in any study. The summary estimate of effect based on G studies is then:

$$\bar{\boldsymbol{Y}} = \frac{\sum \boldsymbol{W_g Y_g}}{\sum \boldsymbol{W_g}} \qquad (17)$$

in which the statistical weight assigned to each study is:

$$\boldsymbol{W} = \frac{1}{\boldsymbol{SE}^2} \qquad (18)$$

SE^2 is equal to the variance of each estimate of effect. Hence, in order to include an estimate of effect from a study in a meta-analysis, one needs to know the standard error of that estimate.

In the meta-analysis approach, each result is represented as an estimate for the exponent α in the power function calculated as:

$$\alpha = \frac{\ln\left(\frac{Y_1}{Y_0}\right)}{\ln\left(\frac{V_1}{V_0}\right)} \qquad (19)$$

where Y_0 is the number of accidents before a change in speed, Y_1 is the number of accidents after a change in speed, V_0 is speed before the change and V_1 is speed after the change.

The individual estimates of α serve as the basis for an overall estimate for the exponent, obtained by conventional meta-analysis or random effects meta-regression. Each estimate of α is assigned a weight proportional to the inverse of the variance of the estimate.

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Assuming that there is no measurement error in mean speed, the denominator in equation 19 is a constant and the variance of α can be calculated by the relation $Var(kx) = k^2 Var(x)$. The variance of α is given by:

$$\mathrm{Var}(\alpha) = \frac{\frac{1}{\mathrm{Y}_0} + \frac{1}{\mathrm{Y}_1}}{\left(\ln\left(\frac{\mathrm{V}_1}{\mathrm{V}_0}\right)\right)^2} \tag{20}$$

The statistical weight assigned to each estimate of power in the fixed-effects model of analysis is 1/Var(α). In some cases a comparison group has been applied to estimate the change in the number of accidents or accident victims. In that case there are four terms in the numerator, but the principle is the same.

In the regression approach the weights are given by:

$$\mathrm{w} = \frac{1}{\frac{1}{\mathrm{Y}_0} + \frac{1}{\mathrm{Y}_1}} \tag{21}$$

There are two methods of combining estimates of effect in meta-analysis. These are referred to as the fixed-effects model and the random-effects model. The fixed-effects model relies on the assumption that the between-study variance of estimates of effect is random only. The random-effects model is based on the assumption that there is a systematic between-study variance in estimates of effect. The choice between a fixed-effects and random-effects model of analysis can be made on the basis of a statistical test, the homogeneity test. If the homogeneity test statistic is significant, this indicates that there is systematic between-study variance of estimates of effect, and a random-effects model should be preferred. The statistical weight assigned to each estimate of effect is then re-estimated by including a variance component, the size of which reflects the amount of between-study variance in estimates of effect. The statistical weight of each estimate of effect in the fixed effects model of analysis is:

$$w_i = \frac{1}{v_i} \tag{22}$$

To test the validity of the assumption made in the fixed-effects model of no systematic between-study variance in estimates of effect, the following test statistic, Q, is estimated:

$$Q = \sum_{i=1}^{g} w_i y_i^2 - \frac{\left(\sum_{i=1}^{g} w_i y_i\right)^2}{\sum_{i=1}^{g} w_i} \tag{23}$$

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This test statistic has a Chi-square distribution with g – 1 degrees of freedom, where g is the number of estimates of effect that have been combined. If this test statistic is statistically significant, a random-effects model of analysis would normally be preferred. In a random effects model, the statistical weight assigned to each result is modified to include a component reflecting the systematic variation of estimated effects between cases. This component, often referred to as the variance component, is estimated as follows (Shadish and Haddock 1994):

$$\sigma_\theta^2 = \left[Q - (g-1)\right]/c \qquad (24)$$

Q is the test statistic described above, g is the number of estimates and c is the following estimator:

$$c = \sum_{i=1}^{g} w_i - \left[\sum_{i=1}^{g} w_i^2 \Big/ \sum_{i=1}^{g} w_i\right] \qquad (25)$$

The variance of each result now becomes:

$$v_i^* = \sigma_\theta^2 + v_i \qquad (26)$$

The corresponding statistical weight becomes:

$$w_i^* = \frac{1}{v_i^*} \qquad (27)$$

6.6.2 Models estimated

The Power Model, as presented in chapter 2, applies to the following categories of accidents or injured road users:

1. Fatal accidents or fatally injured road users
2. Fatal and serious injury accidents or fatally and seriously injured road users
3. All injury accidents or all injured road users

This formulation is cumulative, in the sense that fatal accidents or fatally injured road users are included in all categories, whereas serious injury accidents or seriously injured road users are also included in all injury accidents or all injured road users. This introduces an element of inconsistency in the model. The effect of changes in speed on fatal accidents, for example, are represented by a power of 4 when only fatal accidents are studied, a power of 3 when serious injury accidents are included as well, and a power of 2 when all injury accidents are included. Despite this inconsistency, the Power Model was tested in the form it has been stated. If, for example, there are far more serious injury accidents than

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fatal accidents, the exponent estimated for fatal and serious accidents will be dominated by the serious accidents.

The Power Model for killed or injured road users consists of two terms: one for the number of accidents, another for the number of victims per accident. Thus, for fatalities an exponent of 4 is postulated for fatal accidents, and an exponent of 8 for the number of fatalities per fatal accident. The data available does not make it possible to test each of these exponents in the Power Model. The data available in most cases show only fatal accidents or fatalities, not the number of fatalities per fatal accident. The exponent for fatalities postulated by the Power Model will, however, be between 4 and 8. It will be close to 4 if the number of fatalities per fatal accident is close 1, closer to 8 the higher the number of fatalities per fatal accident become. In most fatal road accidents, the number of fatalities is 1 or 2. Hence, an exponent for fatalities close to 5 would be consistent with the Power Model.

The following models have been estimated:

1. A conventional meta-analysis, based on all data and applying to the cumulative groups as defined in the Power Model (model 1)
2. A conventional meta-analysis, based on well-controlled studies and applying to the cumulative groups defined in the Power Model (model 2).
3. A conventional meta-analysis, based on all data and applying to mutually exclusive groups of accidents or accident victims (model 3).
4. A conventional meta-analysis, based on well-controlled studies and applying to mutually exclusive groups of accidents or accident victims (model 4).
5. A meta-regression analysis based on the meta-analysis approach, based on all data and applying to mutually exclusive groups of accidents or accident victims (model 5).
6. A meta-regression analysis based on the regression approach, based on all data and applying to mutually exclusive groups of accidents or accident victims (model 6).

The next sections presents the results of the analyses based on these models.

6.6.3 Results of conventional analyses

Table 14 presents results of the analysis based on models 1 and 2, as described in section 6.6.2. The results are based on a random-effects model. Summary estimates of power are presented both for all studies and for well-controlled studies, i.e. studies that have controlled for regression-to-the-mean, long-term trends, changes in traffic volume and changes in other risk factors influencing accidents or injuries.

The general impression is that the Power Model is supported. Based on the well-controlled studies, the summary estimate of power is 3.65 for fatal accidents, 3.29 for fatal and serious accidents, and 2.67 for all injury accidents. The latter two estimates are slightly higher than those postulated by the Power Model (3 and 2, respectively). The Power Model postulates an exponent greater than 4 for fatalities, greater than 3 for fatalities and serious injuries, and greater than 2 for all

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injured road users. Based on well-controlled studies, the summary estimates of the exponents are 4.90 for fatalities, 3.99 for fatalities and serious injuries, and 3.19 for all injured road users.

The Power Model does not consider property-damage-only accidents. A summary estimate of power has been produced for all accidents, which includes fatal accidents, injury accidents and property-damage-only accidents. Based on the well-controlled studies, the summary estimate of the exponent is 2.15.

Table 14: Results of test of the Power Model of the relationship between changes in speed and changes in the number of accidents or accident victims

Category	**Source of evidence**	**Estimate of power**	**Standard error**	**Number of results**
Fatal accidents	All studies	4.21	0.68	47
Fatal accidents	Well-controlled studies	3.65	0.83	23
Fatalities	All studies	4.90	0.16	30
Fatalities	Well-controlled studies	4.90	0.17	21
Fatal and serious injury accidents	All studies	3.41	0.54	64
Fatal and serious injury accidents	Well-controlled studies	3.29	0.72	26
Fatalities and serious injuries	All studies	3.84	0.47	44
Fatalities and serious injuries	Well-controlled studies	3.99	0.50	30
All injury accidents	All studies	2.78	0.25	303
All injury accidents	Well-controlled studies	2.67	0.43	96
All injured road users	All studies	2.86	0.44	71
All injured road users	Well-controlled studies	3.19	0.43	44
All accidents (including PDO)	All studies	2.50	0.23	389
All accidents (including PDO)	Well-controlled studies	2.15	0.39	113

Source: TØI report 740/2004

Table 15 presents results for models 3 (all studies) and 4 (well-controlled studies), as described in section 6.6.2. In these models, the various levels of accident or injury severity are treated as mutually exclusive categories, and not interpreted cumulatively as in the Power Model.

For fatal accidents and fatalities, summary estimates are identical to those presented in Table 14; for the other categories, they differ.

The overall pattern of the findings presented in Table 15 support the following observations: Changes in speed have a strong effect on accident severity. The effect of changes in speed tends to be stronger for accident victims than for accidents. Changes in speed appear to have an effect not just on accident- or injury severity, but also on the probability of accident occurrence (as indicated by the positive power for all accidents). Broadly speaking, the summary estimates of power are consistent with the Power Model. There are, however, a couple of exceptions. In particular, the powers estimated for serious injury accidents and seriously injured accident victims are lower than one would expect, compared to the powers estimated for the other categories. This is probably due to the low number of results, which introduces substantial uncertainty in the summary

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estimates. Most of the summary estimates of power are statistically significant at conventional levels.

Table 15: Estimates of power for the effects on accidents or accident victims of changes in speed – mutually exclusive categories

Category	Source of evidence	Estimate of power	Standard error	Number of results
Fatal accidents	All studies	4.21	0.68	47
Fatal accidents	Well-controlled studies	3.65	0.83	23
Fatalities	All studies	4.90	0.16	30
Fatalities	Well-controlled studies	4.90	0.17	21
Serious injury accidents	All studies	1.35	0.34	17
Serious injury accidents	Well-controlled studies	1.59	0.84	3
Seriously injured road users	All studies	1.59	0.27	14
Seriously injured road users	Well-controlled studies	1.76	0.42	9
Slight injury accidents	All studies	0.90	0.31	17
Slight injury accidents	Well-controlled studies	1.05	0.84	3
Slightly injured road users	All studies	1.64	0.30	12
Slightly injured road users	Well-controlled studies	1.56	0.26	7
Injury accidents (unspecified)	All studies	2.76	0.30	222
Injury accidents (unspecified)	Well-controlled studies	2.61	0.55	67
Injured road users (unspecified)	All studies	1.78	1.60	15
Injured road users (unspecified)	Well-controlled studies	2.40	2.24	7
Property-damage-only	All studies	1.70	0.54	86
Property-damage-only	Well-controlled studies	0.73	0.97	17

Source: TØI report 740/2004

6.6.4 Results of meta-regression analyses

In the meta-regression analyses, it was not possible to treat the various levels of accident or injury severity as cumulative groups, since that would have meant inclusion of the same data point more than once. Fatal accidents, for example, would have to be included four times: once for fatal accidents, once for fatal and serious accidents, once for all injury accidents, and once for all accidents. Hence, in the meta-regression analyses, the categories are mutually exclusive.

Two models have been estimated; one based on the meta-analysis approach, using estimates of power as data, and one based on the regression analysis approach, using estimates of changes in speed and changes in accidents or victims as data. Independent variables in the analyses included study quality, described in terms of control for confounding factors. Results of a random-effects analysis based on these models are presented in Table 16.

There are 460 data points in the meta-analysis approach, 451 data points in the regression approach. Nine data points could not be used in the regression approach, as they are represented only by an estimate of power and its standard error, and does not include data on changes in speed.

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The results are close to the Power Model for fatal accidents and fatalities. For the other groups, some of the results are surprising and highly uncertain.

Table 16: Results of meta-regression analyses – mutually exclusive groups

	Meta-analysis approach (N = 460) (model 5)		**Regression approach (N = 451) (model 6)**	
Category	**Estimate**	**Standard error**	**Estimate**	**Standard error**
Fatal accidents	4.02	0.84	3.82	0.64
Fatalities	4.66	0.96	4.04	0.93
Serious injury accidents	1.45	1.14	1.10	0.40
Serious injured road users	2.78	1.08	2.39	0.95
Slight injury accidents	0.88	1.06	0.72	0.34
Slightly injured road users	1.35	1.06	1.42	1.10
Injury accidents (unspecified)	2.49	0.53	2.28	0.32
Injured road users (unspecified)	1.75	1.02	1.73	1.12
Property damage only	1.44	0.53	0.83	0.22

Source: TØI report 740/2004

In the first place, it is surprising that the exponent for serious injury accidents is lower than for all injury accidents (severity not specified). In the second place, the exponent for injured road users is lower than the exponent for injury accidents. These findings are not consistent with the Power Model. It should be noted, however, that the standard errors of some of the coefficients are very large.

6.6.5 Assessing the consistency of the findings with the Power Model

The Power Model postulates the following exponents for the six categories of accidents or accident victims it identifies:

Fatalities:	4-8, closer to 4 than to 8 (e.g. 4.5)
Fatalities and serious injuries:	3-6, closer to 3 than to 6 (e.g. 3.6)
All injuries:	2-4, closer to 2 than to 4 (e.g. 2.7)
Fatal accidents:	4
Fatal and serious accidents:	3
All injury accidents:	2

The consistency of the findings with the Power Model can, strictly speaking, only be assessed for the analyses in which the categories of accidents or accident victims are cumulative. The methodologically strongest of these analyses is the one based on the well-controlled studies. The coefficients estimated in this analysis were:

Fatalities:	4.90
Fatalities and serious injuries:	3.99
All injuries:	3.19
Fatal accidents:	3.65
Fatal and serious accidents:	3.29

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All injury accidents: 2.67

These values are all close to those postulated by the Power Model. All models are, by and large, consistent with the Power Model. The analyses therefore suggest that relying on the Power Model is certainly a plausible way of describing the relationship between speed and road safety. *In no way do the analyses suggest that the Power Model is gravely wrong. On the contrary, the Power Model is largely supported by the analyses*.

The qualifier “largely” has been inserted, because some of the estimates of the exponents depart so much from the values hypothesised by the Power Model as to suggest a revision of the exponents. To justify a revision, it is necessary to identify the best model, or at least a subset of the models that are better than another subset.

6.7 Sensitivity analysis

A sensitivity analysis is designed to assess the effects on the results of a meta-analysis of analytic choices made as part of the analysis. Elvik (2004A) suggests that a sensitivity analysis should always include the following items:

1. The possible presence of publication bias
2. Choice of estimator of effect (if there is a choice)
3. The possible presence of outlier bias
4. Statistical weighting of studies included in a meta-analysis
5. Assessment of study quality

The first of these items has already been discussed. There were few indications of publication bias in the data set. An indication of publication bias was found only in two of nine groups that were tested. In neither of these groups did the possible presence of publication bias have any effect on summary estimates of power. In fact, in one of the groups the unadjusted and adjusted summary estimates were identical to the third decimal point. It is therefore concluded that there are no indications that publication bias has influenced study findings.

As far as the second item is concerned, the estimator of effect in the current study is the summary estimates of power. This estimator comes in two versions: (1) The cumulative version, consistent with the way the Power Model is formulated, and (2) The mutually exclusive groups version, in which summary estimates are developed for each of the various categories of accident or injury severity, treated as non-cumulative.

With respect to the third item, outlying data points, the funnel plots gave no indication of any clearly outlying data points. In some subsets of the data, however, a few data points contribute substantially to the statistical weights. When one, or a few, data points dominate, the summary estimate is really determined by these data points. In such cases, the summary estimate in effect only represents one or a few data points. Hence, a sensitivity analysis has been made in which data points contributing very large statistical weights have been omitted.

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Statistical weighting in meta-analysis generally refers to the choice between a fixed-effects model and a random-effects model. In the analyses reported above, a random-effects model has been used throughout, as all data sets contained systematic variation in estimates of power. However, six different models of analysis have been developed. A comparison of findings across these models will be made, with the aim of trying to select the best model or a way of combining the findings of the different models.

An assessment of study quality (item 5) has already been made, in that studies controlling for regression-to-the-mean, long-term trends, changes in traffic volume and effects of other risk factors (than speed) have been identified as well-controlled. A variable identifying well-controlled studies has been used in the multivariate models.

External validity is also an aspect of study quality. As noted in chapter 4, external validity can be assessed as part of meta-analysis. External validity is high if the findings of the studies are the same in different contexts. If, on the other hand, the findings vary across study contexts, then one cannot generalise the findings of this study from, for example, one country to another. Since the effects of speed are closely related to the physical laws of motion, one would expect their effects to be more or less the same everywhere. It would, in other words, be surprising to find that the relationship between speed and road safety was different in different countries or in different decades. As part of the sensitivity analysis, the effects of various variables describing study context have been tested.

6.7.1 The cumulative and non-cumulative formulations of the Power Model

As noted previously, the cumulative formulation of the Power Model introduces an element of inconsistency in it, in that the coefficient for, say, fatal accidents is postulated to be 4 when considering fatal accidents alone, 3 when fatal and serious injury accidents are combined, and 2 when fatal accidents are combined with all other injury accidents. This inconsistency can be avoided by treating the various levels of accident or injury severity as mutually exclusive categories.

Table 17 gives a comparison of the coefficients in the six models that have been estimated. Two of the models are cumulative, as formulated in the Power Model, the other four are non-cumulative.

For fatalities and fatal accidents, the choice between the cumulative and non-cumulative formulations of the Power Model does not arise, as fatalities and fatal accidents are at the top of the hierarchy. Table 17 shows that the estimated exponents are highly consistent in the different models. All estimated exponents are close to the values postulated by the Power Model. The choice of a best estimate will be discussed later.

For serious injury accidents and seriously injured road users, the choice between a cumulative and non-cumulative model formulation has a large effect on the estimated exponents. Even within the four non-cumulative models, the estimated exponents vary substantially, in particular for seriously injured road users.

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Slight injury accidents and slightly injured road users are not explicitly mentioned in the Power Model. The exponents estimated are fairly stable across the four models that included slight injury accidents and slightly injured road users.

Table 17: A comparison of estimates of power in six models of analysis

	Cumulative		**Non-cumulative**			
Category	**Model 1**	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**
Fatalities	4.90	4.90	4.90	4.90	4.66	4.04
Fatal accidents	4.21	3.65	4.21	3.65	4.02	3.82
Seriously injured road users (1)	3.84	3.99	1.59	1.76	2.78	2.39
Serious injury accidents (2)	3.41	3.29	1.35	1.59	1.45	1.10
Slightly injured road users			1.64	1.56	1.35	1.42
Slight injury accidents			0.90	1.05	0.88	0.72
All injured road users (3)	2.86	3.19	1.78	2.40	1.75	1.73
All injury accidents (4)	2.78	2.67	2.76	2.61	2.49	2.28
Property-damage-only accidents (5)	2.50	2.15	1.70	0.73	1.44	0.83

Source: TØI report 740/2004

(1) Includes fatalities in the cumulative models

(2) Includes fatal accidents in the cumulative models

(3) Includes fatalities, serious injuries, slight injuries and injuries of unspecified severity in the cumulative models

(4) Includes fatal accidents, serious injury accidents, slight injury accidents and injury accidents of unspecified severity in the cumulative models

(5) Includes all fatal and injury accidents in the cumulative models

With respect to all injury accidents and all injured road users, the choice between a cumulative and non-cumulative model does seem to be of some importance as far as injured road users are concerned, but matters little as far as injury accidents are concerned. Finally, the exponents estimated for property-damage-only vary substantially across models. All these estimates are highly uncertain, in part, presumably, because the reporting of property-damage-only accidents is likely to be incomplete and variable.

In addition to the element of inconsistency introduced by the cumulative formulation of the Power Model, another undesirable property of this formulation, is that it makes the exponents dependent on the degree of reporting of accidents or injuries at various levels of severity. To see this, consider the following hypothetical example. In traffic system A, the reporting of injury accidents is very high. For each fatal accident, 10 serious injury accidents and 100 slight injury accidents are reported. In traffic system B, reporting is very low. For each fatal accident, 2 serious injury accidents and 8 slight injury accidents are reported. In system A, the exponent for fatal and serious accidents will be dominated by the serious accidents. The exponent for all injury accidents will be dominated by slight injury accidents. In system B, on the other hand, the exponent for fatal and serious accidents will, roughly speaking, consist of 1/3 contribution from fatal accidents and 2/3 contribution from serious injury accidents. If the exponents for fatal and serious injury accidents are estimated empirically in systems A and B, their values will differ as a result of accident reporting alone, even if their true values are presumed to be identical.

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To help shed further light on this problem, it is useful to present some accident statistics showing the mean number of injured road users per injury accident of a given severity. Table 18 provides such figures for national roads in Norway, covering the period 1993-2000 (Ragnøy, Christensen and Elvik 2002).

Table 18: Number of injured road users per injury accident. Norway 1993-2000. Source: Ragnøy, Christensen and Elvik 2002

		Number of injured road users			
Accident severity	**Number of accidents**	**Fatalities**	**Seriously injured**	**Slightly injured**	**All injured**
Fatal	1437	1633	420	901	2954
Serious	4604		5388	2794	8182
Slight	27648			38506	38506
All injury	33691	1633	5808	42201	49642

Table 18 shows that the number of fatalities per fatal accident is 1633/1437 = 1.136. Similarly, the number of seriously injured road users per serious accident is 5388/4604 = 1.170. The number of fatal and serious injuries per fatal and serious accident is 7441/6041 = 1.232. The number of injured road users per injury accident is 49642/33691 = 1.473.

The number of serious injury accidents per fatal accident was 4604/1437 = 3.2. The corresponding ratio for slight injury accidents was 19.1.

Similarly, for Sweden in 2001, the mean number of fatalities per fatal accident was 1.141, the mean number of fatal and serious injuries per fatal and serious accident was 1.285, and the number of injured road users per injury accident was 1.451. The ratios of the number of accidents was 6.1 serious injury accidents to each fatal accident, and 23.8 slight injury accidents to each fatal accident.

In Great Britain in 2001, the mean number of fatalities per fatal accident was 1.086. The mean number of fatal and serious injuries per fatal and serious accident was 1.167, and the mean number of injured road users per injury accident was 1.368. There were 9.9 serious injury accidents per fatal accident, and 61.6 slight injury accidents per fatal accident.

For the state of Victoria, Australia, 1981, Andreassen (1988) found the number of fatalities per fatal accident to be 1.131. The mean number of fatal and serious injuries per fatal and serious accident was 1.273. The mean number of injured road users per injury accident was 1.437. There were 9.5 serious injury accidents per fatal accident, and 15.3 slight injury accidents per fatal accident.

Höhnscheid (2003) provides similar figures for Germany. In 1998 there was 1.105 fatalities per fatal accident in Germany, 1.195 fatal and serious injuries per fatal and serious injury accident, and 1.455 injured road users per injury accident. There were 12.9 serious injury accidents per fatal accident, and 39.7 slight injury accidents per fatal accident.

Table 19 summarises these data. There is a remarkable consistency with respect to the number of victims per accident. The number of serious or slight injury accidents per fatal accident varies substantially. This means that an application of

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the Power Model, in its cumulative form, could produce different results in different countries.

Table 19: Mean number of injured road users per accident and number of accidents per fatal accident. Data for different countries

	Country					
Category	**Norway**	**Sweden**	**Great Britain**	**Germany**	**Victoria**	**Mean**
Fatalities per fatal accident	1.136	1.141	1.086	1.105	1.131	1.120
Fatal and serious injuries per fatal and serious injury accident	1.232	1.285	1.167	1.195	1.273	1.230
Injured road users per injury accident	1.473	1.451	1.368	1.455	1.437	1.437
Serious injury accidents per fatal accident	3.2	6.1	9.9	12.9	9.5	8.3
Slight injury accidents per fatal accident	19.1	23.8	61.6	39.7	15.3	31.9

Source: TØI report 740/2004

The implications of a cumulative and non-cumulative formulation of the Power Model can be explored by means of a simple simulation. The following assumptions have been made:

Accident severity	Relative numbers, low reporting	Relative numbers, high reporting	Victims per accident
Fatal	1	1	1.1
Serious	2	15	1.2 (fatal and serious)
Slight	10	70	1.4 (all injury accidents)

It is assumed that the true values of the exponents are 4.5 for fatalities, 3.0 for serious injuries, 1.5 for slight injuries, 4.0 for fatal accidents, 2.5 for serious injury accidents, and 1.0 for slight injury accidents.

The exponent for fatalities and serious injuries (cumulated) will then be estimated to 3.46 when reporting is low and 3.08 when reporting is high. The exponent for all injured road users will be 1.89 when reporting is low and 1.79 when reporting is high. Even when reporting is low, slight injuries tend to dominate the data and exert the greatest influence on the exponent.

For fatal and serious accidents, the exponent becomes 3.00 when reporting is low, 2.59 when reporting is high. For all injury accidents, the exponent is 1.79, both when reporting is high and when it is low.

Thus, it is seen that the cumulative formulation of the Power Model is sensitive to the level of accident reporting, particularly for serious injuries. Part of the difference found in the values of the exponents between the cumulative and non-cumulative models is probably due to incomplete accident reporting. The Power Model is less sensitive to incomplete accident reporting, and does not contain

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logical inconsistencies, if revised to a non-cumulative form, in which the various levels of accident or injury severity are treated as mutually exclusive categories.

6.7.2 Choosing the best estimates of the exponents

Six models have been developed to estimate the exponents of the Power Model. In view of the discussion above, best estimates will be developed for the four models that evaluate the non-cumulative formulation of the Power Model only. This means that models 1 and 2 are not relevant. Models 3 and 4 are both based on conventional meta-analysis. The difference between these models is that model 4 is based only on well-controlled studies, whereas model 3 is based on all studies. Models 5 and 6 are both based on multivariate meta-regression. Model 5 is based on the meta-analysis approach (using estimates of power as data), model 6 is based on the regression approach (using data on changes in speed to fit a model that describes changes in accidents or accident victims). In model 5, summary estimates have been adjusted for study quality, in model 6, summary estimates have not been adjusted for study quality. Model 5 is therefore preferred to model 6. This means that the best summary estimates will be derived from models 4 and 5. This can be done in two ways.

A simple way of deriving best estimates based on models 4 and 5 is to combine the coefficients estimated in these models the same way results are combined in meta-analysis, i.e. by using inverse variance weights. Each coefficient is then assigned a weight equal to $1/SE^2$, in which SE is the estimated standard error of the coefficient.

Another way of deriving best estimates, is to apply smoothing functions to the estimates of power, before combining by means of the inverse variance method. Figure 18 shows an example of the application of a smoothing function to summary estimates of power. The smoothed values were assigned the same standard errors as the crude summary estimates of power and then combined for models 4 and 5. Table 20 shows the two sets of best estimates for power.

The estimates derived without smoothing are consistent with the Power Model with two exceptions: (1) The power for seriously injury accidents is lower than the power for injury accidents of unspecified severity, the majority of which are likely to be slight injury accidents. (2) The power for injured road users is lower than the power for injury accidents.

The smoothed best estimates are perfectly consistent with Power Model. The estimates for seriously injured road users and serious injury accidents are adjusted upward. The estimate for injury accidents is adjusted downward.

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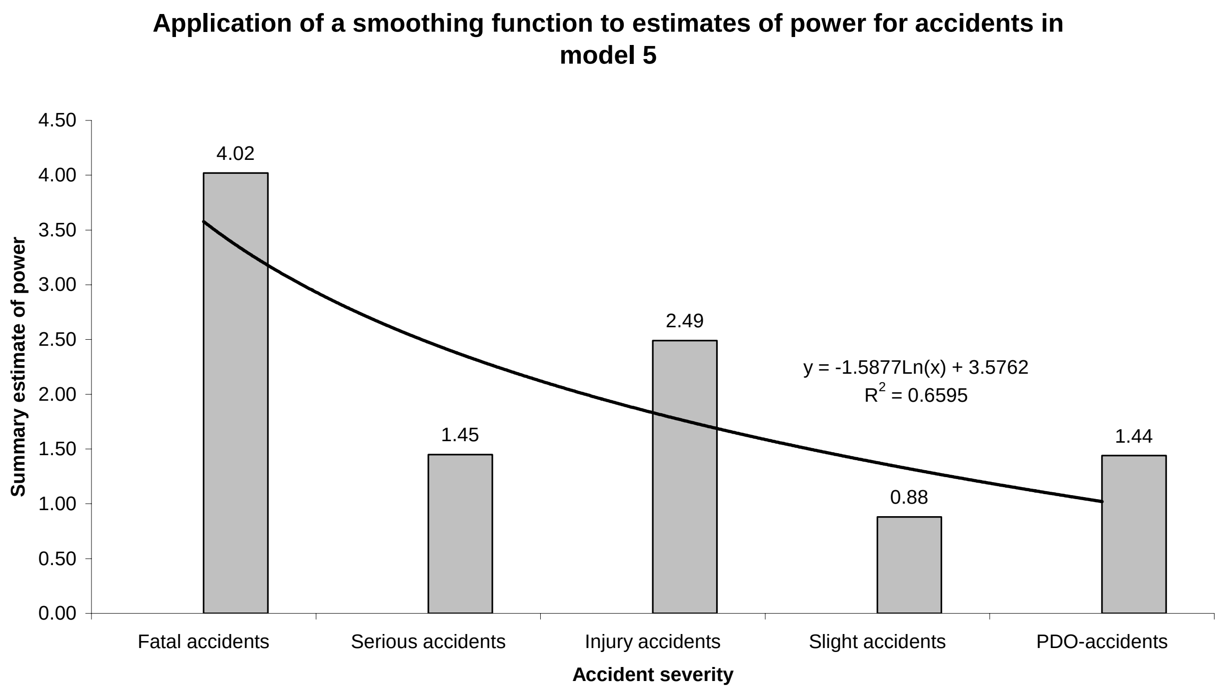

Source: TØI report 740/2004

Figure 18: Application of a smoothing function to summary estimates of power

Table 19 indicated that the number of victims per accidents is remarkably consistent in different countries. Typically, the mean number of fatalities per fatal accident is about 1.1. The mean number of fatal and serious injuries per fatal and serious accident is typically around 1.2. The mean number of injured road users per injury accident is typically around 1.4.

Table 20: Best summary estimates of power based on models 4 and 5 in crude and smoothed form

	Crude summary estimates		Smoothed summary estimates	
Category	**Estimate**	**Standard error**	**Estimate**	**Standard error**
Fatalities	4.89	0.17	4.34	0.17
Serious injuries	1.89	0.39	2.79	0.39
Injuries (unspecified)	1.86	0.93	1.91	0.93
Slight injuries	1.55	0.25	1.32	0.25
Fatal accidents	3.83	0.59	3.48	0.59
Serious accidents	1.54	0.68	2.40	0.68
Injury accidents (unspecified)	2.55	0.38	1.73	0.38
Slight accidents	0.98	0.66	1.23	0.66
Property-damage accidents	1.28	0.47	0.97	0.47

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These facts impose some restrictions on the values that are empirically plausible for the exponents constituting the Power Model. Suppose, as an example, that the power for injury accidents is 2. The power for injured road users cannot then be so much greater than 2 that the expected number of injured road users per injury accident is estimated to drop below 1. Suppose, as an example, that there are 100 injury accidents in which 140 people are injured. If speed is reduced by 10%, the number of injury accidents is expected to go down to 81 (assuming a power of 2). If the power for injured road users is 6, then their number is estimated to go down to 74. This outcome is not logically possible, since there cannot be fewer than 1 injured person per injury accident.

Based on the analyses presented above, best estimates of power have been developed on the basis of the following criteria:

1. Summary estimates of power should be consistent with the evidence from the six models tested, including the smoothed estimates developed on the basis of models 4 and 5.
2. Summary estimates of power should be internally consistent in the sense that: (a) all estimates applying to accidents victims should be greater than those applying to accidents, and (b) estimates should fall uniformly as accident or injury severity is reduced.
3. The residual terms for summary estimates of power should be as small as possible and conform to the normal distribution.

The first of these criteria means that one should not propose summary estimates of power that are outside those found in the analyses reported, including the smoothed estimates presented in Table 20. The second criterion is proposed in order to ensure that summary estimates of power are not internally inconsistent, as would be the case, for example, if the power applying to serious injuries is lower than the power applying to slight injuries. While the analyses might produce summary estimates indicating a lower power for serious injuries than for slight injuries, such a finding must be considered as rather implausible in view of the laws of physics and well-established knowledge in the field of biomechanics.

Basically, criterion 2 means that a theoretically plausible structure is imposed on the set of summary estimates of power – not by disregarding or discounting the empirical summary estimates, but by giving little weight to those empirical estimates that are clearly inconsistent with the majority of these estimates.

To apply criterion 2, the following rank order of outcomes by severity is used (from the most severe to the least severe):

Fatal accidents or fatalities > Serious accidents or injuries > Injury accidents or injuries (severity not stated) > Slight accidents or injuries > Property-damage-only

The third criterion is proposed in order to ensure that predictions made on the basis of summary estimates of power will not be systematically wrong and that the error involved in such predictions will in most cases not be greater than randomness accounts for.

Table 21 presents summary estimates of power derived on the basis of the three criteria listed above. The ranges of empirical summary estimates given in Table

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21 include only estimates that refer to the non-cumulative form of the Power Model.

Table 21: Best summary estimates of power

	Range of empirical summary estimates of power		Proposed best summary estimates of power	
Category	**Crude**	**Smoothed**	**Estimate**	**95% CI**
Fatalities	4.04 – 4.90	4.33 – 4.58	4.5	4.1 – 4.9
Serious injuries	1.59 – 2.78	2.78 – 2.88	3.0	2.2 – 3.8
Injuries (unspecified)	1.73 – 2.40	1.89 – 1.98	2.7	0.9 – 4.5
Slight injuries	1.35 – 1.64	1.19 – 1.33	1.5	1.0 – 2.0
Fatal accidents	3.82 – 4.21	3.38 – 3.58	3.6	2.4 – 4.8
Serious accidents	1.10 – 1.45	2.35 – 2.48	2.4	1.1 – 3.3
Injury accidents (unspecified)	2.28 – 2.76	1.63 – 1.83	2.0	1.3 – 2.7
Slight accidents	0.72 – 1.05	1.14 – 1.38	1.2	0.1 – 2.3
Property-damage accidents	0.73 – 1.70	0.79 – 1.02	1.0	0.2 – 1.8

Source: TØI report 740/2004

The summary estimates of power proposed lie within the range of empirical summary estimates in all cases except two: serious injuries and injuries of unspecified severity. For both these categories a higher estimate of power than found in the analyses is proposed, mainly to ensure that the overall pattern of estimates of power makes sense from a theoretical point of view.

An analysis of residual has been performed. For each estimate of power, its standardised residual is estimated by:

$$\text{Standardised residual} = \frac{(Y_i - \overline{Y})}{SE_i} \qquad (28)$$

Y_i is each estimate of power. $\overline{Y}$ is the summary estimate of power applying to the categories used (fatal accidents, fatalities, and so on). These are the proposed best summary estimates listed in Table 21. SE_i is the standard error of each estimate of power. The standardised residual for an estimate of power indicates the number of standard deviations it departs from the summary (mean) estimate.

According to statistical theory, residuals should be normally distributed around the mean value and should have a mean of zero. Ideally speaking, 95% of the residuals should lie within plus or minus 2 (strictly speaking 1.96) standard deviations of the mean.

Figure 19 shows an example of a residual plot. The plot refers to fatalities. There are a total of 47 estimates of power. The residual is positive for 26 of these estimates, indicating that they are above the summary estimate of 4.5. A negative residual is found in 21 cases. 45 of the 47 estimates lie within plus or minus two standard deviations of the summary estimate of power.

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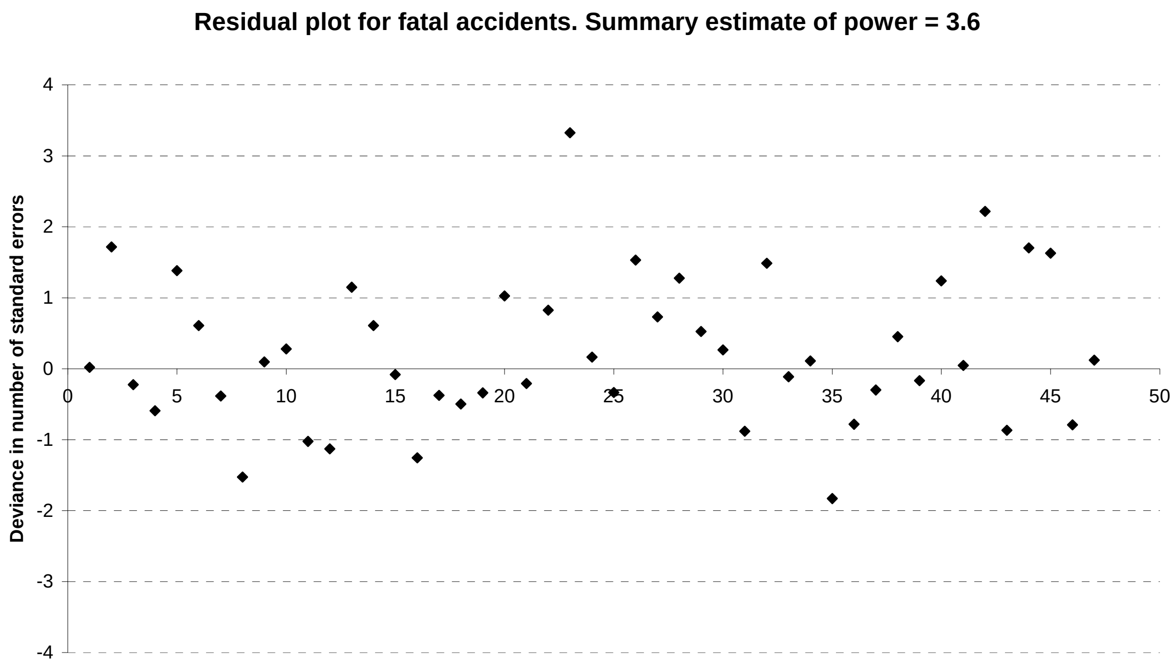

Source: TØI report 740/2004

Figure 19: Residual plot for estimates of power for fatalities

Similar residual plots have been prepared for all categories. These plots are printed in Appendix 2 to the report. Table 22 summarises the results of the analysis of residuals. The table reports the percentage of residuals falling within plus or minus two standard deviations of the summary estimate, the sign of the residuals and tests for normality.

Table 22: Summary of results of analysis of residual for best summary estimates of power

Category	Number of estimates	Within two standard deviations (%)	Positive residuals (%)	Negative residuals (%)	Chi-square for test of normality	P-value for Chi-square
Fatalities	30	93.3%	63.3%	36.7%	4.75	0.447
Serious injuries	14	85.7%	57.1%	42.9%	6.26	0.282
Injuries (unspecified)	15	53.3%	53.3%	46.7%	73.21	0.000
Slight injuries	12	91.7%	66.7%	33.3%	6.84	0.233
Fatal accidents	47	95.7%	55.3%	44.7%	5.47	0.361
Serious accidents	17	88.2%	41.1%	58.9%	6.17	0.290
Injury accidents	222	93.2%	55.0%	45.0%	7.00	0.221
Slight accidents	17	94.1%	52.9%	47.1%	9.67	0.085
PDO-accidents	86	84.9%	59.3%	40.7%	26.71	0.000
All categories	460	90.2%	56.1%	43.9%	40.67	0.000

Source: TØI report 740/2004

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By and large, the residuals are well-behaved, indicating that the proposed summary estimates of power are close to those that minimise the residuals. There is, however, a slight tendency for the number of positive residuals to exceed the number of negative residuals. This suggests that the summary estimates of power are conservative. In view of the fact that no study has controlled perfectly for confounding variables, it is prudent to adopt a conservative interpretation of the evidence, in order not to overestimate the effects on road safety of changes in speed.

6.7.3 The effects of contextual variables

The Power Model is stated in general terms and can be interpreted as a statement of physical laws describing the relationship between speed and road safety. If the Power Model is valid, one would, ideally speaking, expect it to be valid everywhere. The relationship between speed and road safety should not be strongly influenced by contextual variables. The following variables can be regarded as contextual for the purposes of this study: country, study decade, type of publication, traffic environment and measure influencing speed.

Various multivariate models were run to determine if the contextual variables were significant when included in models that also included the variables of primary interest in this study. Due to multi-collinearity, it was not possible to test all the contextual variables in the same model. Country was represented by 11 dummy variables. None of these were significant at the 5% level in the random-effects model of analysis. Publication type (3 dummy variables), study design (5 dummy variables), type of measure influencing speed (5 dummy variables), and study decade (3 dummy variables) were tested in the same model. Three coefficients were statistically significant at the 5% level. Considering the fact that 16 coefficients were estimated, one would expect 1 of them to be significant by chance. None of the contextual variables appeared to have any systematic effect. It is therefore concluded that the relationship between speed and road safety does not depend on the contextual variables, but can be generalised across these variables.

6.7.4 Other analytic choices

Sensitivity analyses were made with respect to the following analytic choices:

1. Omission of results based on very small changes in speed.
2. Omission of results based on studies contributing a large proportion of the statistical weights.

If one observes a very small change in speed, but a large change in accidents, it is unlikely that the change in accidents can be attributed to the change in speed alone. It was therefore decided to omit relative changes in speed smaller than 2.5%. This left 368 results for analysis. Table 23 shows the results of the analysis.

There are only small differences in the estimated values of the exponents in the Power Model, depending on whether the analysis is based on all speed changes or only those greater than 2.5%. Table 23 also shows the results of analyses in which the largest studies (with statistical weights greater than 50) were omitted. By and

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large, the results are close to those obtained for all studies using the meta-analysis approach. It would therefore appear that the results are not sensitive to the size of the change in speed or study size.

Table 23: Results of sensitivity analyses of the Power Model of the relationship between speed and road safety – estimates of power

	Size of speed change		Study size	
Categories (mutually exclusive)	**All studies (model 5)**	**Speed change >2.5%**	**All studies (model 5)**	**Small studies**
Fatal accidents	4.02	3.96	4.02	3.96
Fatalities	4.66	4.76	4.66	4.65
Serious injury accidents	1.45	1.64	1.45	1.35
Seriously injured road users	2.78	2.79	2.78	2.77
Slight injury accidents	0.88	1.08	0.88	0.78
Slightly injured road users	1.35	1.47	1.35	1.34
Injury accidents (unspecified)	2.48	2.77	2.48	2.41
Injured road users (unspecified)	1.75	1.78	1.75	1.17
Property-damage-only accidents	1.44	1.99	1.44	1.50

Source: TØI report 740/2004

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7 Discussion of findings

7.1 A general note on the interpretation of the results of research

There are, in general, two ways of interpreting the results of research:

1. A methodological interpretation, which usually takes the form of a critical examination of a study according to methodological criteria. A study employing a weak design and relying on poor data will often be regarded as inconclusive, based on a methodological interpretation. A methodological interpretation often argues for rejecting the findings of a study.
2. A substantive interpretation, which tries to account for the findings of a study in terms of known causal processes or mechanisms. A substantive interpretation usually argues for taking the findings of a study seriously.

Ideally speaking, one would like to rule out methodological interpretations of a study. In non-experimental accident research, this is not possible. In this chapter, the findings reported in Chapter 6 will be discussed from both a methodological and a substantive point of view.

7.2 Limitations of the analyses

The analyses presented in chapter 6 are not ideal. The following limitations of these analyses are important to notice:

1. The analyses omitted a large number of potentially relevant studies. They may therefore suffer from bias generated by study inclusion criteria.
2. The results of the analyses may to some extent reflect the impacts of other road safety measures, not primarily changes in speed.
3. The analyses did not consider whether the impacts of speed vary according to types of accidents or groups of road users.
4. The analyses rely on speed data that may be of questionable reliability and validity.
5. The analyses are limited to the Power Model and did not consider other models that can describe the relationship between changes in speed and changes in road safety.
6. There may be statistical dependency between multiple results of the same study

Each of these points will be discussed.

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7.2.1 The possibility of study inclusion bias

A total of 175 studies were considered as relevant. 98 studies were included in the meta-analysis, 64 were omitted because they did not report enough information to be included in the meta-analysis, and 13 studies that could in principle have been included were omitted mostly because they were regarded as methodologically too weak. This mean that a substantial proportion of the relevant studies, 77 of 175 (44 percent) were excluded from the meta-analysis.

It is not uncommon that meta-analyses include just a small number of the studies that were regarded as relevant. As an example, consider a meta-analysis by Wagenaar et al (1995) of studies that have evaluated measures to reduce drinking and driving. A literature search identified 6,500 studies dealing with the subject of drinking and driving. Only 815 of these, however, were evaluation studies. Efforts were made to obtain these studies, but only 777 were obtained. These 777 studies were then screened on the basis of three criteria for methodological quality. Only 291 studies passed this screening. 157 of these were omitted because they were judged to be too old or had been summarised previously. This left 134 studies for analysis, of which 9 were omitted because they used very atypical research designs. This left 125 studies for inclusion in the meta-analysis. When the pruning of studies is as drastic as it was in this case, one may wonder how representative the studies were that were included in the meta-analysis.

In this study, the pruning of studies was far less drastic. It is nevertheless important to try to assess whether the exclusion of a large number of studies may have biased the analysis. The studies that were excluded have been classified according to two criteria:

1. What effects can be expected of the measure that was evaluated?
2. Did the study find the expected effects or not?

As an example, if a study evaluated a raised speed limit, one would expect both speed and the number of accidents to increase. If the study found this, i.e. if it found an increase in accidents (speed data would in most cases not be available for the omitted studies), the findings were consistent with what was to be expected. The point of classifying studies this way, is to compare the consistency of their findings with those of the studies that were included. If the excluded studies did not show as consistent findings as the studies that were included, there is a possibility of study inclusion bias. If, on the other hand, findings were equally consistent, the possibility of study inclusion bias is reduced.

The results of this assessment are presented in Table 24. The upper part of the Table is based on the classification of findings in the meta-analyses, reported in Table 12, which found that 338 of the 460 estimates of effect included in the meta-analysis indicated the speed and accidents or victims changed in the same direction (both down or both up). The middle part of Table 24 shows a similar distribution for the 64 studies that did not report enough data to be included in the meta-analysis. The bottom part of Table 24 shows a classification of the findings if the 13 studies that were excluded from the meta-analysis.

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Table 24: Assessment of potential study inclusion bias. Consistency in findings of studies included in meta-analysis and studies not included in meta-analysis

Consistent findings (speed and accidents change in same direction)	Inconsistent findings (speed and accidents change in opposite directions)	Total number of findings
Studies included in meta-analysis		
338 (73.5%)	122 (23.5%)	460 (100.0%)
Studies omitted from meta-analysis because of incomplete data		
56 (87.5%)	8 (12.5%)	64 (100.0%)
Studies excluded from meta-analysis on account of method		
63 (49.2%)	65 (50.8%)	128 (100.0%)

Source: TØI report 740/2004

Some of the 64 studies that were omitted because they did not report all the data required for a meta-analysis contained multiple estimates of effect. These estimates were averaged into an overall estimate for each study. Nearly 88 percent of the studies that were omitted produced consistent findings, that is they found that accidents or the number of victims was reduced when a measure that would normally be expected to reduce speed was introduced, or increased when a measure that would normally be expected to increase speed was introduced.

It is unlikely that inclusion of the 64 studies that could not be included in the meta-analysis would have changed the results very much. If anything, inclusion of these studies might have reinforced the relationship between speed and accidents or accident victims, as the findings appear to be more consistent than for the studies that were included in the meta-analysis.

A total of 13 studies were omitted, mainly for methodological reasons. A total of 128 estimates of effect have been extracted from these studies. These estimates appear to be much less consistent than for other studies. In fact, about half of the estimates of effect indicate that safety is improved (accidents reduced) when speed increases. It cannot be ruled out that inclusion of these studies in the meta-analysis would have weakened the relationship between speed and accidents or accident victims. It should be noted, however, that some of the inconsistent findings are based on very small accident samples, and would therefore not have contributed much to the data set.

On the whole, it is very unlikely that study inclusion bias can explain the findings of the meta-analysis.

7.2.2 Effects of other road safety measures – residual confounding

Ideally speaking, the results of the meta-analysis should show the effects on accidents or accident victims of changes in speed only, and not the effects of other road safety measures. In some of the studies included, however, measures were taken that might have an effect on the number of accidents or victims even if speed remained unchanged. Reconstructing urban streets to environmentally adapted streets is a case in point. Such reconstruction often involves new parking regulations or new pedestrian crossing facilities that might affect safety even if speed does not change.

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In the multivariate meta-regression analyses, the effects of variables indicating the measure taken were tested. Speed limits was used as the reference category, meaning that the results presented in Chapter 6 show the effects of changes in speed on accidents or accident victims *when changes in speed limit is the only measure introduced to change speed*. This was chosen as the reference category, because a change in the speed limit is, in a manner of speaking, the "purest" measure that can be taken to influence speed, since it does not affect road layout or the level of enforcement.

The meta-regression analyses found that the variables indicating the measure taken were mostly non-significant at conventional levels. The only exception was police enforcement. Studies in which the primary measure taken was increased police enforcement find a much greater effect on accidents of changes in speed than studies in which other measures were introduced to change speed. Police enforcement may perhaps influence other aspects of road user behaviour in addition to speed, for example, by increasing the alertness of road users.

The results thus show the relationship between speed and road safety given that road layout and police enforcement remain unchanged. There is still a possibility that the results contain residual confounding, attributable to, for example, changes in the weather or changes in the level of accident reporting. It is, however, very unlikely that these potentially confounding factors could have such a great effect on accidents or accident victims as to eliminate or greatly attenuate the effect attributed to speed in the meta-analysis.

7.2.3 Effects of speed on various types of accident or groups of road users

At the start of the study, the aim was to include variables describing the type of accident affected and the groups of road users affected, in order to study whether the relationship between speed and road safety differed according to these characteristics. It soon became apparent that most studies did not include any information on these characteristics; hence they were not included among the variables coded in the meta-analysis.

It is known (see, e.g. Anderson et al 1997) that impact speed has a major effect on the probability of sustaining a fatal injury when a pedestrian is struck by a car. Whether the effect of impact speed on the probability of fatality can be described in terms of a power function or some other mathematical function has not been evaluated; it is at any rate clear that the relationship is positive: the higher the impact speed, the higher the probability of a fatal injury.

The relationship between speed and accidents is likely to be basically the same for all types of accident and all groups of road users. Any difference would mainly be related to the level of impact speed associated with a certain probability of fatal or serious injury. This level is lowest for pedestrians, highest for occupants of large motor vehicles (buses or trucks).

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7.2.4 The reliability of speed data

It has not been possible to assess the reliability of speed data. These data have simply been taken at face value. It is known, however, that speed data may contain errors.

Random errors in speed data will attenuate the relationship between speed and accidents, meaning that the true relationship, based on perfectly reliable speed data, is stronger than the relationship estimated on the basis of less than perfectly reliable speed data. In principle, if the reliability of speed data had been known, it would have been possible to correct for the attenuation introduced by lack of reliability (Hunter and Schmidt 1990).

Lack of reliability in speed data is compounded by a lack of reliability in accident data, which further attenuates the relationship between speed and accidents.

The fact that errors in speed data cannot be ruled out, suggests that the true relationship between speed and road safety is stronger, i.e. more law-like with smaller residuals, than the relationship estimated in this study.

7.2.5 Alternative models of the relationship between speed and road safety

The fact that the findings support the Power Model does of course not rule out the possibility that other models could describe the relationship between speed and road safety equally well. One can imagine a plethora of models that are all consistent with the idea that when speed goes down, road safety gets better and when speed goes up, road safety gets worse. Only two models will be discussed:

1. A linear model, in which changes in safety are modelled as a function of the absolute (as opposed to relative) change in speed.
2. A logistic model, in which the impacts of changes in speed depend on the initial level of speed.

A plot of the data described in terms of the linear model is shown in Figure 20. The absolute change in speed is shown in the abscissa, the relative change in accidents is shown on the ordinate. As can be seen, the data points are widely dispersed. A line has been fitted to the data points. The slope of this line has the correct sign, but the line does not fit the data very well.

A meta-regression was run and parameters of the linear model estimated for fatal accidents and fatalities. The slope parameter for fatal accidents was 0.042, indicating that fatal accidents increase by 4.2 percent when speed increases by 1 km/h. Fatalities were found to increase by 4.4 percent when speed increases by 1 km/h. Further analyses of the linear model were, however, not made.

The linear model is not very plausible. In the first place, it predicts that the effect on, say, fatalities of reducing speed from 125 to 120 km/h will be the same as the effect of reducing speed from 50 to 45 km/h – both these reductions are 5 km/h. This is highly implausible. An impact speed of 125 or 120 km/h probably makes no difference for fatalities – both speeds are above the threshold for fatal injury. A reduction from 50 to 45 km/h could, on the other hand, have an impact on fatalities, in particular in mixed traffic.

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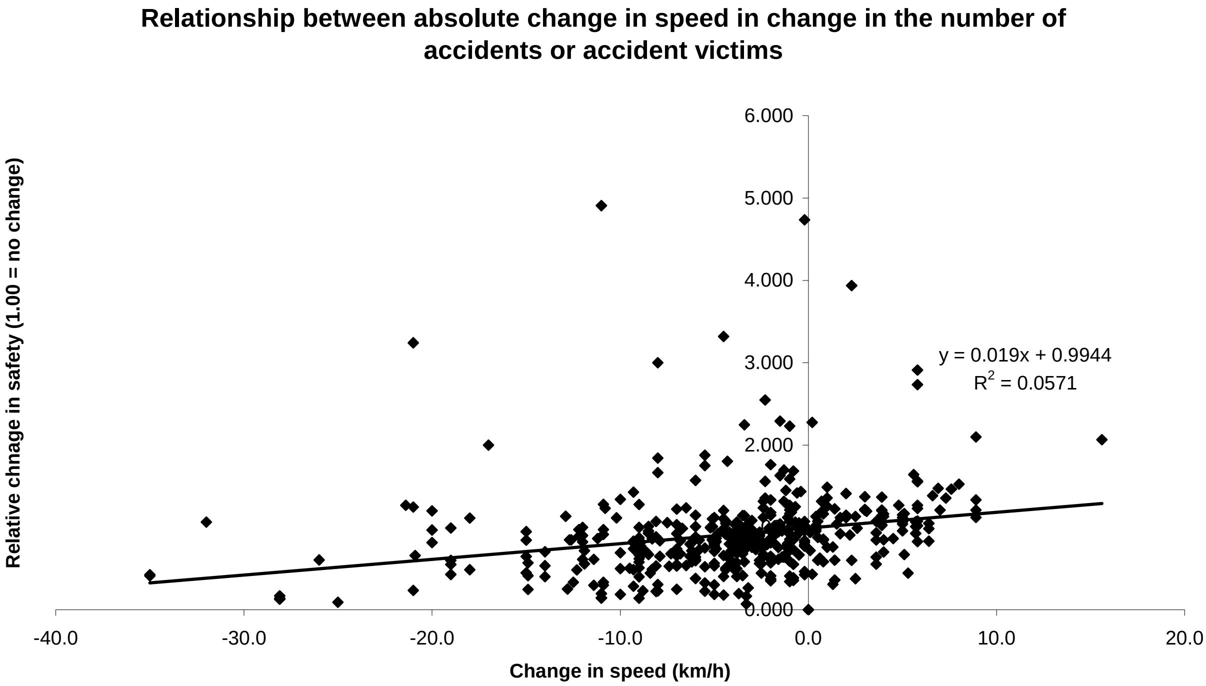

Source: TØI report 740/2004

Figure 20: The linear model of the relationship between changes in speed and changes in road safety

In the second place, a linear model can give nonsensical results if there are large changes in speed. The estimated slope for fatalities, for example, predicts a reduction of 25 · 4.4 = 110 percent if speed is reduced by 25 km/h. For these reasons, the linear model has not been pursued further.

In the third place, the linear model cannot be deduced theoretically by relying on plausible assumptions. It is theoretically not very plausible to model a ratio (a relative change) as a function of a difference (an absolute change).

While the linear model is implausible, some of the implications of the Power Model are also unlikely to be true. The Power Model predicts that the effect on fatalities of reducing speed from 100 to 50 km/h is the same as the effect of reducing speed from 10 to 5 km/h. This seems unlikely to be the case. The difference between 100 km/h and 50 km/h could decide whether an accident is survivable or not. The difference between 10 km/h and 5 km/h is, however, unlikely to determine if an accident is survivable.

A model that allows the effect of relative changes in speed to depend on initial speed is the logistic function, which has the following general form (Rothman and Greenland 1998):

$$R(x) = \frac{\exp(\alpha + \beta_i x_i)}{1 + \exp(\alpha + \beta_i x_i)} \qquad (29)$$

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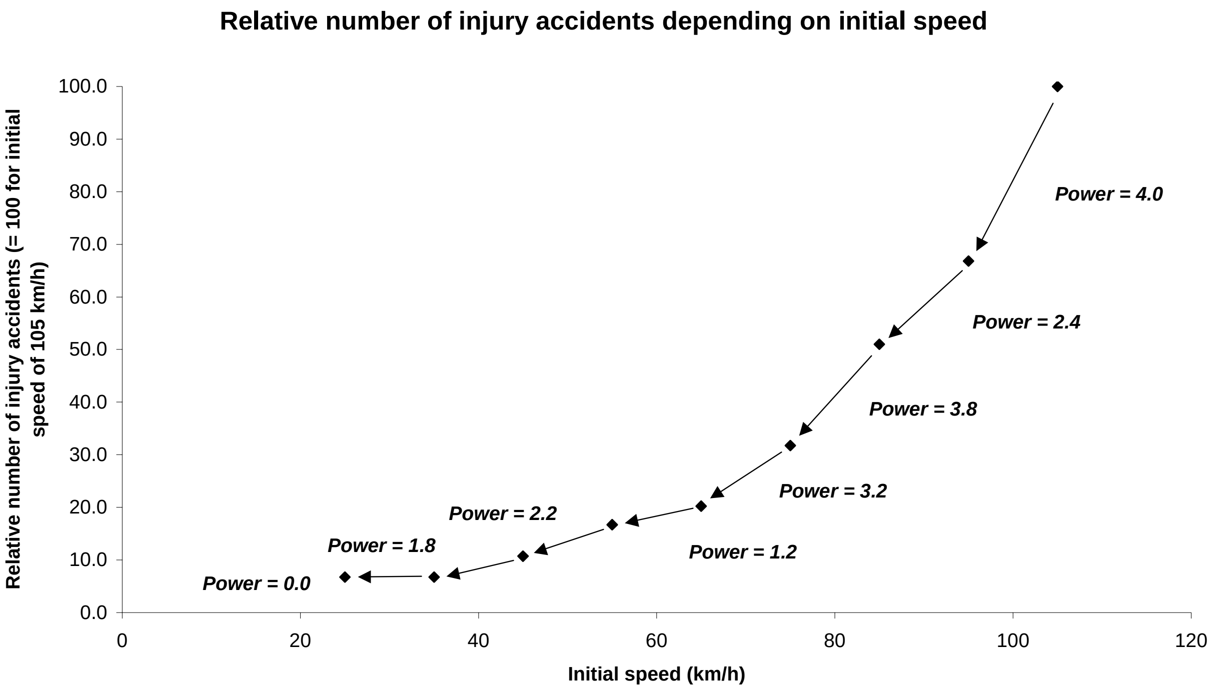
Exp is the exponential function, that is e (e = 2.71828) raised to the power of the expression in parenthesis. α and β are parameters to be estimated. R(x) is usually defined as a continuous probability function.

A logistic function is typically S-shaped, rising slowly at first (when speed is too low to kill), then more rapidly until it flattens out again as the probability of observing the outcome of interest gets close to 1 (when speed is so high that nobody survives).

To test the plausibility of the logistic model, a synthesis of evidence was made for injury accidents. This was the only group that contained enough estimates of power to test for dependence on initial speed.

The estimate of power did vary, depending on initial speed, but not in a very systematic way. There was a tendency for the power to become lower as initial speed became lower. The relative number of injury accidents, depending on initial speed is plotted in Figure 21.



Source: TØI report 740/2004

Figure 21: Relative number of injury accidents, depending on initial speed

Figure 21 indicates that reducing speeds above 80 km/h has a greater effect on injury accidents than reducing speeds below 40 km/h. The findings suggest that the gain in precision by using different exponents for different initial speeds is too small to justify it, given the uncertainty of the estimates of power for different initial speeds. Hence, a logistic function does not perform better than a simple power function in predicting the effects of changes in speed.

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7.2.6 Statistical dependency between multiple results of the same study

The 98 studies that were included in the meta-analysis contained a total of 460 estimates of effect, corresponding to 4.69 estimates per study on the average. The use of multiple estimates of effect from the same study can be problematic if there is statistical dependency between these estimates. Estimates that are statistically related tend to vary less – be more consistent among themselves – than statistically independent estimates. Thus, statistical dependency between multiple estimates of effect from the same study can give the impression that the relationship between speed and road safety is stronger than it actually is, if estimated on the basis of statistically independent observations.

In order to test for the possible presence of this sort of dependency in the data set, studies have been divided into two groups:

1. Studies that contain a single estimate of power only (20 studies)
2. Studies that provide multiple estimates of power (78 studies)

The variance in study findings between studies that each give only one estimate of power will be taken to reflect the variance of independent estimates, that is estimates that are not statistically dependent on each other. The variance between results of the same study may, on the other hand, be influenced by statistical dependency between these results. To the extent that this dependency reduces variance, one would expect the contribution to overall variance of between-results variance in studies containing multiple results to be smaller than the corresponding contribution of between-studies variance to the overall variance found for studies that each provide only one estimate of power.

Table 25 presents selected tests designed to probe for the presence of dependency between multiple results of the same study. Listed in the first row of Table 25 are some statistics that apply to nine studies, each of which provided a single estimate of power for injury accidents. This group was selected, as it contains the largest number of results (222 in total).

The mean within-study variance is simply the (unweighted) mean value of the variance of each study. The greater the variance, the smaller is the statistical weight of the study, since this weight is the inverse of the variance. A large within-study variance indicates a small study (i.e. a study based on a small accident sample and/or a small change in mean speed). The mean between-study variance is the value of the homogeneity statistic, Q (see equation 23), divided by the degrees of freedom (G – 1; G = number of results) applying to it. The indicates the mean value of the between-study variance. The sum of the within-study and between-study variance is the total variance. Table 25 also shows the value of the variance component (see equation 24).

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Table 25: Test of the possible presence of statistical dependency between multiple results of the same study. Injury accidents

Number of studies	Number of results per study	Mean within study variance	Mean between study variance	Mean total variance	Variance component
9	1	108.83 (97.3%)	2.99 (2.7%)	111.82 (100%)	1.63
1	6	106.92 (92.2%)	9.03 (7.8%)	115.95 (100%)	80.75
1	7	21.31 (94.0%)	1.35 (6.0%)	22.66 (100%)	2.10
1	16	62.94 (98.5%)	0.93 (1.5%)	63.87 (100%)	-0.30
1	17	14.54 (71.1%)	5.90 (28.9%)	20.44 (100%)	2.71
1	28	26672.80 (100%)	1.03 (0.0%)	26673.83 (100%)	13.70

Source: TØI report 740/2004

For studies that provided only one estimate of power, the between-study variance represented 2.7% of the total variance. The second row of Table 25 shows corresponding statistics for a single study that provided six estimates of power. The “between-study” variance in this case is really a “between-results-of-the-same-study” variance. As can be seen, this variance contributed to 7.8% of the total variance, which is considerably more than the between-study variance for independent findings.

By and large, the pseudo between-study variance for studies giving multiple results is as large as the genuine between-study variance for studies giving only a single result. Thus, Table 25 suggests that statistical dependency, generating a spuriously small variance between findings, is not a source of error in the present study.

7.3 Criteria of causality in non-experimental research

Despite the limitations discussed above, some powerful arguments can be given for taking the results of the analyses seriously. These arguments are based on commonly applied criteria for causality in non-experimental research (Blalock 1979, Asher 1976; Elvik 2001; Shadish, Cook and Campbell 2002). These criteria include:

1. There should be a statistical relationship between the presumed cause and the presumed effect.
2. A strong statistical relationship is, keeping everything else constant, more likely to be causal than a weak statistical relationship.
3. The statistical relationship should be internally consistent in subsets of the data.
4. The direction of causality should be clear, that is it should be clear which variable is the cause and which is the effect.
5. The statistical relationship between cause and effect should not disappear when confounding factors are controlled for.

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6. If the cause comes in different doses, there should be a dose-response pattern between cause and effect.
7. If the cause can reasonable be assumed to be effective only within a certain subset of the data, effects should be found only in that subset and not outside of it (the specificity of effect criterion, to be elaborated below).
8. The causal mechanism through which effects are transmitted should be known.
9. The findings of the study should be explicable in terms of a well-established scientific.

These points will be discussed in turn.

7.3.1 Presence, strength and consistency of statistical relationship

There is a clear statistical relationship between changes in speed and changes in road safety. A total of 50 summary estimates of power have been produced in the six models that were developed. All these summary estimates have the expected sign (positive). 39 of the summary estimates of power are statistically significant at the 5 percent level of significance. Among the 460 individual estimates of power, estimates that represent 94.1 percent of the statistical weights are internally consistent, i.e. they show that speed and road safety change in the same direction. Such a high level of consistency is rarely seen in road safety evaluation studies. It is almost always the case that when speed goes down, so does the number of accidents and the severity of injuries. When speed goes up, the number of accidents or the severity of injuries increases in about 70 percent of the cases.

The relationship between changes in speed and changes in road safety is very strong. If, for example, speed is reduced by 10 percent, one may expect the number of road accident fatalities to be reduced by almost 38 percent. This is a remarkably large effect of a 10 percent change in exposure to a risk factor. By comparison, if traffic volume is reduced by 10 percent, one may expect the number of fatalities to be reduced by about 8 percent. If the incidence of drinking and driving is reduced by 10 percent, one may – assuming that those who stop drinking and driving have the same accident involvement rate as those who continue to drink and drive – expect the number of fatalities in Norway to go down by about 3 percent.

It may be concluded that the first three criteria of causality are fulfilled: (1) There is a statistical relationship between changes in speed and changes in road safety, (2) This statistical relationship is (very) strong, and (3) The statistical relationship between speed and road safety is highly consistent.

7.3.2 Direction of causality

A clear direction of causality means that it should be clear whether A is the cause of B or B the cause of A. In this report changes in speed is regarded as a cause of changes in road safety. There are three characteristics of the study that make this assumption reasonable.

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First, nearly all the studies serving as the basis of the meta-analysis are before-and-after studies. This study design is, in general, not affected by the ambiguity about causal direction that characterises many cross-sectional studies. The before-and-after design ensures that the cause precedes the effect in time.

Second, it has been found that when the cause changes direction, so does the effect. An increase in speed tends to lead to more accidents and more severe injuries, whereas a reduction in speeds tends to be associated with fewer accidents and less serious injuries.

Third, the effects attributed to changes in speed do not disappear when important confounding variables are controlled for.

It is concluded that the direction of causality is clear.

7.3.3 Control for confounding factors

In the meta-analysis, the effects of confounding factors were controlled for in two ways. The first approach was to select only those studies that had controlled for regression-to-the-mean, long-term trends, changes in traffic volume, and differences in other risk factors (than speed) affecting accidents. Large effects of speed were found in these studies; there was no clear tendency for the effects of speed to be smaller in the well-controlled studies than in less well-controlled studies.

The second approach was to control statistically for confounding and contextual variables by means of meta-regression. In the most extensive of these analyses, the following variables were controlled for statistically: publication type, study design, type of measure influencing speed, type of traffic environment, decade study was published, country in which study was made, and how many of the confounding factors listed above a study controlled for. The effects of speed did not vanish or show any systematic tendency to become smaller in these well-controlled analyses than in analyses that did not control statistically for as many variables.

It is therefore concluded that the effects attributed to speed hold up well when potentially confounding factors are controlled for.

7.3.4 Dose-response relationship

A dose-response relationship means that a large dose of the cause will be associated with a larger effect than a small dose of the cause.

Such a relationship is clearly found in this study: The larger the changes in speed, the greater are the changes in road safety. This tendency is highly consistent in all the models that have been estimated.

7.3.5 Specificity of effects

Some studies (e.g. Chen et al 2002) try to define something called “speed-related” accidents. This concept has no meaningful interpretation. All accidents are speed-related. Hence, one would expect changes in speed to influence all accidents, and

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not just a particular subset of accidents. This is indeed what this study has found. It does not make sense, and does not have empirical support, to speak about some types of accidents that are influenced by speed and other types of accidents that are not influenced by speed.

7.3.6 Causal mechanisms

The causal mechanisms producing a relationship between speed and accidents should be obvious: The higher the speed, the longer you travel before reacting, and the longer you need to stop. Moreover, at high speeds, motor vehicles become more difficult to manoeuvre, especially in a critical situation, when fact action must be taken to avoid an accident. In critical situations, many drivers tend to react too violently, for example, by steering too hard, leading to loss of control of the vehicle. However, not everybody believe that these causal mechanisms are real. Thus, a lobbying organisation in Great Britain, called "Safe speed" writes the following on its Website (Safe speed 2004):

"They believe that road accidents are rooted in physics. This is the "faster you go, the harder you'll crash" school of thinking. Like many oversimplified beliefs it contains a grain of truth to add plausibility. But the physics has no effect until the safety systems have failed and an accident is inevitable. For an average UK driver this happens once in about 7 years and results in a damage only accident. … With a little consideration, it should be obvious that the physics are the same every day. Until the driver makes his critical mistake that is. On that special day when he crashes the physics are exactly the same. But the driver fails to respond to a hazard in good time. Thus, accidents are not rooted in physics. Accidents are rooted in psychology."

It is obviously correct, as this quotation suggests, that driving speed is just one risk factor among the very many risk factors that influence road safety. It is also clearly correct that most of the time, most drivers are – fortunately – able to avoid accidents. Most trips end safely. Even drunk drivers get home on most of their trips. Yet, few would doubt that being drunk significantly reduces driver performance, and thus reduces the safety margins any driver normally adopts.

Most of the time, driving is easy, at least for experienced drivers, for whom most of the routine tasks of driving have become fully automated and do not require any conscious attention. On a few very rare occasions, driving suddenly becomes difficult, for example, because something happens for which the driver was totally unprepared. Speed then determines the time, and indeed the space, a driver has available to avoid an accident. The driver does not need to have made any mistake at all. If a large package drops from a lorry driving in front of you, you must either be able to stop before hitting the package, or steer around it, otherwise there will be an accident. If an oncoming driver suddenly veers into your driving lane, what do you do? Again, the more time you have to deal with the problem, the more likely you are to find a way out of it that will prevent an accident from occurring, or at least reduce its severity.

It is true that "the laws of psychology", and not just the laws of physics, affect accident occurrence. Having a new car, with state-of-the-art technology, can help the driver to drive safely. But no driver can repeal the laws of physics, however

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skilled that driver may be. There is only so much road surface friction, only so much room to manoeuvre, only so much distance to stop, as the laws of physics and the physical layout of the road allows for. If a driver enters a curve at a very high speed, there will not be enough sideways friction to steer the vehicle around the curve. Centrifugal forces will throw the vehicle into the ditch; there is absolutely nothing the driver can do about this, except to reduce speed before entering the curve, to a level that leaves enough friction to be able to negotiate the curve.

Are drivers more alert, do they react more quickly, when going at a high speed than when going at a low speed? Yes, there is evidence (Törnros 1995) for this. The differences in reaction time are, however, far too small to compensate for the increase in the distance travelled during reaction time as speed increases.

Speed is directly related to safety by way of laws of physics that determine the length of stopping distances, the size of the "field of safe travel" (Gibson and Crooks 1938) and the forces acting on humans when an impact occurs.

7.3.7 Consistency with theory

As noted above, one would expect there to be a relationship between speed and road safety according to the laws of physics, although the relationship observed in the real world will be a lot more noisy than the laws of physics suggest, being modified by a host of other risk factors (some of them unknown), as well as the partly random nature of accident occurrence.

This study has confirmed what the laws of physics would lead us to believe. It is therefore concluded that *there is a direct causal relationship between speed and road safety* and that *the shape of this relationship can be described in terms of a set of power functions. These functions are approximations to law-like relationships, and can therefore be applied universally*. The relationships expressed in the Power Model are likely to be valid for any road transport system.

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8 Practical implications

8.1 The importance of speed as a risk factor

The results of this study shed light on the importance of speed as a risk factor. The number of risk factors that are associated with accident occurrence or injury severity is very large; speed is just one of these factors. Is there any way to determine which risk factors are the most important for accident occurrence or the number of accident victims?

The size of the contributions to accidents or injuries made by various risk factors can be compared in terms of the risk attributable to them and the elasticities of the number of accidents or accident victims with respect to changes in the risk factors. An analysis of road safety problems in Sweden in terms of the risk attributable to various factors has been reported (Elvik and Amundsen 2000). More recently, a framework for a rational analysis of road safety problems has been developed, incorporating not just the size of a problem (the risk attributable to it), but also other dimensions of road safety problems (Elvik 2004B). In this section, an attempt will be made to assess the importance of speed as a risk factor, compared to other risk factors that are often presented as road safety problems.

Table 26 presents the best estimates of the percentage changes in the number of accidents or accident victims as a function of changes in speed, based on the exponents proposed in Chapter 6. It is seen that a variation in speed between a reduction of 15% and an increase of 15% is associated with an expected variation in the relative number of fatalities from 0.48 to 1.88, i.e. a factor of 3.92 (1.88/0.48 = 3.917). Stated in other words, a variation in the mean speed of traffic by a factor of 1.35 (1.15/0.85 = 1.353) produces a variation in the number of fatalities by a factor of 3.92. This shows that speed is indeed a very potent risk factor: even small changes in speed are associated with very large changes in the number of road accident fatalities.

Based on the previous analysis of road safety problems in Sweden (Elvik and Amundsen 2000), figure 22 compares the first order risks attributable to various road safety problems. Please note that the risk attributable to speeding (i.e. driving above the speed limit) has been re-estimated based on the findings of this report.

Speeding is clearly the most important contributor to fatalities, but makes a large contribution to all injuries as well. The only other factor that makes a contribution of a similar magnitude is to be an unprotected road user. Pedestrians, cyclists and riders of mopeds or motorcycles run a substantially higher risk of being killed or injured than do occupants of cars, trucks or buses.

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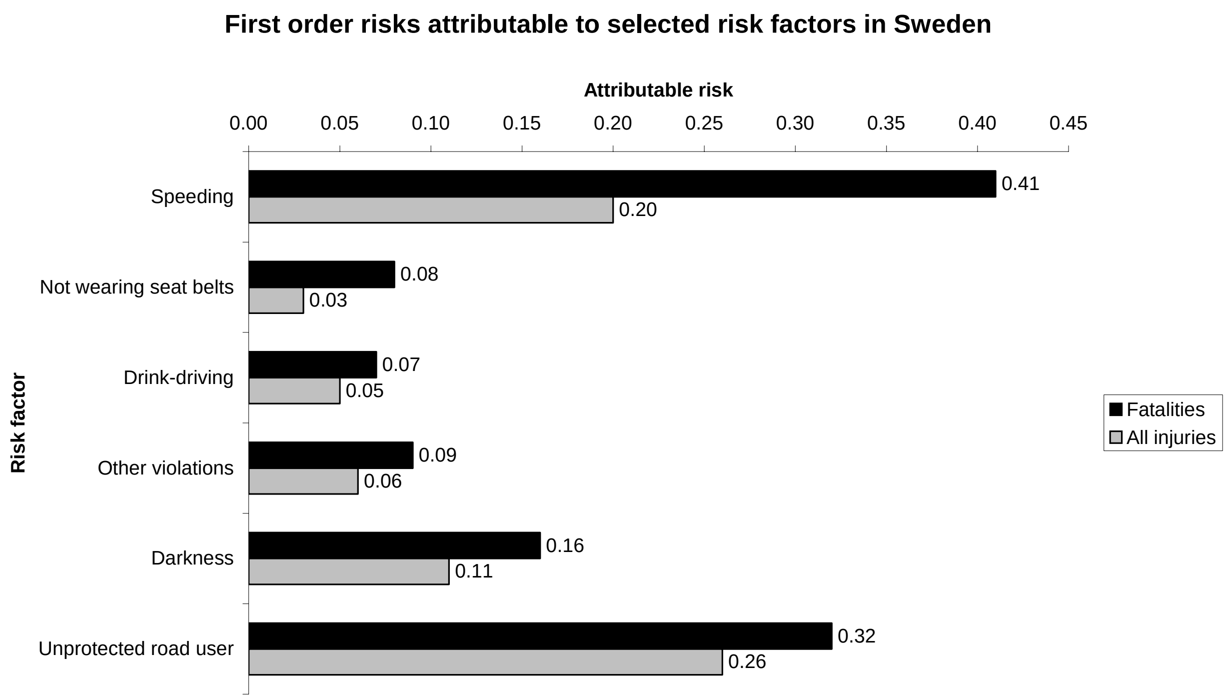
Table 26: Change in accidents or accident victims as a function of change in speed

	Relative change (%) in the number of accidents or victims					
Accident or injury severity	**-15%**	**-10%**	**-5%**	**+5%**	**+10%**	**+15%**
Fatalities	-52	-38	-21	+25	+54	+88
Serious injuries	-39	-27	-14	+16	+33	+52
Slight injuries	-22	-15	-7	+8	+15	+23
All injured road users	-35	-25	-13	+14	+29	+46
Fatal accidents	-44	-32	-17	+19	+41	+65
Serious injury accidents	-32	-22	-12	+12	+25	+40
Slight injury accidents	-18	-12	-6	+6	+12	+18
All injury accidents	-28	-19	-10	+10	+21	+32
Property-damage-only accidents	-15	-10	-5	+5	+10	+15

Source: TØI report 740/2004



Source: TØI report 740/2004

Figure 22: Risks attributable to selected risk factors in Sweden

The severity of the impact of a risk factor can be indicated by the gradient of the risk attributable to it with respect to injury severity. The risk attributable to speeding in Sweden has been estimated to 0.410. The corresponding contribution for all injured road users has been estimated to 0.201. The means that the severity gradient with respect to fatalities is 0.410/0.201 = 2.04. The corresponding gradients are 2.63 for not wearing seat belts, 1.50 for drink-driving, 1.51 for other violations, 1.54 for darkness and 1.20 for being an unprotected road user. Again, speeding is found to be an important and severe risk factor.

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The importance of speed becomes even more pronounced when the elasticity of road accident fatalities with respect to changes in speed and changes in other risk factors is considered. A 10% reduction in speed can be estimated to reduce the number of road accident fatalities by 37.8%. Consider the following impacts of a 10% change in several factors affecting road accident fatalities:

A 10% reduction in …..	Gives a reduction in fatalities of …..
Total traffic volume	6.5%
Exposure of unprotected road users	3.4%
Exposure to darkness	1.7%
Exposure to snow- or ice covered roads	1.6%
Drink-driving	1.0%
Non-wearing of seat belts	0.8%
Mean speed of traffic	37.8%

Source: TØI report 740/2004

If total traffic volume is reduced by 10%, the number of road accident fatalities in Sweden can be estimated to go down by about 6.5% (Fridstrøm et al 1995). If the exposure of unprotected road users, that is travel by pedestrians, cyclists, moped riders and motorcycle riders, is reduced by 10%, the number of road accident fatalities can be reduced by about 3.4%. Reducing exposure to darkness by 10%, from about 20% to about 18% of travel, can reduce road accident fatalities by 1.7%. A similar 10% reduction in exposure to snow- or ice covered roads can reduce fatalities by about 1.6%. If drink-driving is reduced by 10%, from 0.2% of all travel to 0.018% of all travel (Nilsson 2004B), and if those who stop drinking and driving have the same accident rate as those who continue to drink-drive, road accident fatalities in Sweden can be reduced by about 1%. A 10% reduction in the non-wearing of seat belts, from about 10% to about 9%, is expected to reduce road accident fatalities by about 0.8%. All these estimates are subject to uncertainty, but their order of magnitude is likely to be correct.

A 10% reduction in the mean speed of traffic, from, say, 60 km/h to 54 km/h can reduce the number of fatalities by nearly 38%. This dwarfs the impact of a similar reduction in the exposure to any of the other risk factors listed above – indeed it is likely that a 10% reduction in the mean speed of traffic will have a greater impact on road accident fatalities than a 10% reduction in any other known risk factor, including very potent risk factors, like drinking and driving.

The mean speed of traffic is the most important risk factor for road accident fatalities. It has a more powerful effect on road accident fatalities than any other known risk factor, including the overall amount of travel. Speed as a risk factor is always present. Many other risk factors are, like darkness or a slippery road surface, are not always present.

It may be objected that the estimates presented above are hypothetical, and that it is impossible in practice to change exposure to a risk factor by as much as 10%.

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This objection is not correct. When the national maximum speed limit of 55 miles per hour was introduced in the United States of America in 1974, mean speed on rural interstate roads dropped from 65.0 miles/h to 57.6 miles/h, a reduction of 11.4% (Transportation Research Board 1984). Fatality rate (number of fatalities per billion vehicle miles of travel) declined by 32%.

From 1998 to 2003, the number of road users checked for seat belt wearing by officials of the Norwegian Public Roads Administration increased from about 345,000 to about 713,000. Seat belt wearing by drivers in urban areas increased from 79% in 1998 to 84% in 2003, corresponding to a reduction in non-wearing of 24% (from 21% in 1998 to 16% in 2003; a reduction of 24%). In rural areas, seat belt wearing increased from 91% in 1998 to 94% in 2003, corresponding to a reduction in non-wearing of 33% (from 9% to 6%).

According to estimates presented by Elvik (2003) cars are exposed to a road surface covered by snow or ice 13% of the time on roads that are salted in Norway. If these roads were not salted, it was estimated that cars would be exposed to snow or ice 19% of the time. Thus, applying salt reduces exposure to snow or ice by 32% (from 19% to 13%).

Many other examples could be given. Changing exposure to a certain risk factor by 10% is by no means unrealistic or uncommon.

8.2 The need for regulating speed

Can drivers be trusted to choose the speed of travel without any public regulation, or should speed choice be regulated by means of speed limits?

Ultimately, this is of course a political question, that does not have a right or wrong answer from a scientific point of view. Some risk factors are regulated; others are not. In all highly motorised countries, road users are allowed to freely choose:

- When and where to travel
- The route taken between given destinations
- The means of transport used, subject to having a valid driving licence

In particular, the use of mopeds and motorcycles is allowed, despite the fact that these means of transport are very hazardous. Drivers of motor vehicles are, however, usually not allowed to:

- Operate the vehicle when under the influence of alcohol
- Operate the vehicle at a speed exceeding the speed limit
- Operate the vehicle without using protective devices like seat belts or crash helmets

Speed is therefore generally recognised by governments as a risk factor that needs regulation. Drivers, on the other hand, often resent speed limits. Violation rates approaching 50% of all kilometres driven are found in many motorised countries. In practice, this violation rate appears to be tolerated. Speed limits are therefore widely seen as guidelines only – it is not necessary to adhere to them strictly.

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As noted above, it is outside the scope of science to determine whether a certain risk factor should be regulated or not. Society permits individuals to indulge in alcohol and tobacco, despite the fact that excessive consumption of these commodities is very harmful to health and increases the costs of medical care. One could argue that most drivers are capable of choosing a safe speed and should therefore be allowed to do so.

If viewed from a purely statistical point of view, one could say that most road users are behaving safely most of the time, since the risk of an accident is very low in absolute terms. Statistically speaking, a driver has to drive several million kilometres before becoming involved in an injury accident. Against this background, it can be argued that if safety is the hallmark of rationality, and accidents the manifestation of a breakdown of rationality, then, on the average, road users are very rational.

The choice of speed has a number of characteristics that make it likely that this choice will not always be perfectly rational. More specifically, it can be argued that speeding is habit forming, contagious and the result of weakness of will.

The addictive nature of speed is shown in an experiment reported by Schmidt and Tiffin (1969). In the first experimental condition, subjects were asked to accelerate from a standstill to 40 miles/h. They were not able to see the speedometer. On the average, subjects accelerated to 41.4 miles/h, not very far from the correct speed. In the second experimental condition, subjects accelerated to 70 miles/h, held that speed for 5 seconds, and were then instructed to reduce their speed to 40 miles/h. On the average, speed was reduced to 44.5 miles/h. In the third experimental condition, subjects were once more asked to accelerate to 70 miles/h, hold that speed for 20 miles (about 17 minutes) and then reduce speed to 40 miles/h. On the average, subjects reduced speed to 50.5 miles/h. Finally, in the fourth experimental condition, subjects accelerated again to 70 miles/h, stayed there for 20 miles, and then were instructed to reduce speed to 40 miles/h. This experiment was performed after the third experiment, using the same subjects. On the average, speed was reduced to 53.4 miles/h.

This shows that when you have been driving at a speed of 70 miles/h for nearly 20 minutes, 50 miles/h feels like 40 miles/h. When you have been driving at 70 miles/h for 35-40 minutes, 53 miles/h feels like 40 miles/h. In short, speed is almost addictive: The more you consume of it, the more difficult it is to reduce consumption to a targeted, lower level. When you think that you have reached the target level, you are in fact still consuming more speed than your target value.

Speeding is contagious. The speed chosen by a driver is not independent of the speed chosen by other drivers. Connolly and Åberg (1993) have explored the implications of various contagion models of speeding. These models show that, given certain assumptions regarding the preferences of drivers, even a small proportion of drivers can determine the whole speed distribution. Suppose, for example, that 10% of drivers will speed regardless of what other drivers do. A second group of drivers, also representing 10% of the driver population, will speed if they see that more than 10% of other drivers do so. Consequently, if 10% of drivers speed no matter what other drivers do, an additional 10% of drivers will speed because the first 10% of drivers are speeding. Suppose that there is a third group of drivers who will speed if they see that more than 20% of other drivers

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are speeding. Then, this third group will also speed. If we continue this line of reasoning, at each stage adding another group of drivers who will conform to what the others are doing, we see that everybody will speed.

This model may not be very realistic. Drivers differ with respect to how easily their behaviour is influenced by the behaviour of other drivers. A few drivers will speed no matter what other drivers do. A few drivers will never speed no matter what other drivers do. A large group of drivers will try to conform to what the majority of drivers do. Depending on the shape of the distribution of drivers with respect to how much their choice of speed is influenced by the speed chosen by other drivers, one can get major changes in the proportion speeding as a result of temporary changes in this proportion. In one example, as long as the proportion of speeders stays below 40%, it will remain stable at about 10%. If, however, for some reason, the proportion of speeders were to rise above 40%, it would go on increasing until a new equilibrium point is reached at 85% speeders.

Most drivers probably prefer a speed that does not deviate too much from the speed chosen by other drivers. Speed choice can therefore be modelled as a simple coordination game. If all drivers preferred to go at the same speed, solving the coordination game would be easy. As preferences with respect to speed vary, the coordination game becomes difficult. A simple model of the game is given in Table 27.

Table 27: Speed choice as a coordination game

		The fast movers	
		80 km/h	100 km/h
The slow movers	80 km/h	4 3	1 2
	100 km/h	2 1	3 4

Source: TØI report 740/2004

There are two groups of drivers: the slow movers and the fast movers. The slow mover choose between the rows of the table, the fast movers choose between the columns. The preferences with regard to outcomes of the game are indicated by numbers, 4 being the most preferred outcome, 1 the least preferred outcome. The preferences of the slow mover are entered in the upper right corner of each cell of the table, the preferences of the fast movers are entered in the lower left corner of each cell of the table. The first preference of the slow movers, indicated by the number 4, is that everybody drives at 80 km/h. The slow movers are, however, willing to speed up to 100 km/h if the fast movers are going at that speed. This is their second most preferred outcome (indicated by the number 3). The worst outcome for the slow movers is that they go at 100 km/h and the fast movers at 80 km/h.

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As can be seen, the game has two equilibrium points: either that everybody goes at the slow speed, or that everybody goes at the fast speed. These equilibria are not stable. Accordingly, at any point in time, there is likely to a sizable proportion of drivers who feel more or less forced to adopt a speed, which differs from their most preferred speed.

Speeding can be reasonably interpreted as a case of weakness of will. Weakness of will is generally (Elster 1979) defined as choosing an action one does not regard as best in the long term. Typical examples from everyday life would be to have one more drink, although one has already had enough, to have another piece of cake, although one is already well fed, and so on. To prefer immediate gratification to an option that defers it is the paradigmatic case of weakness of will.

The gratification from speeding is immediate and certain. The unwanted impacts of speeding are delayed and probabilistic. Most drivers do not experience these impacts at all. Hence, the temptation to speed may be irresistible. The choice of speed is also influenced by emotional factors, whose effects may vary, depending on the situation (Vaa, 2004).

On the whole, it can be argued that speed choice is unlikely to be fully informed or rational. A case can therefore be made for regulating speed choice by introducing speed limits.

8.3 The controllability of speed by means of road safety measures

To what extent can speed be influenced by various road safety measures or other policy instruments?

As part of the review of studies of the relationship between changes in speed and changes in road safety, the effects of changes in speed limits on the mean speed of traffic have also been summarised. The results of this summary are presented in Figure 23. The figure shows the relationship between changes in speed limits and changes in the mean speed of traffic.

As can be seen from Figure 23, speed limits do influence the mean speed of traffic, which almost always changes in the same direction as the speed limit. There is, however, great variation in the effects of changes in speed limits. Such changes rarely lead to fully proportional changes in mean speed, i.e. mean speed rarely changes by as much as 10 km/h if the speed limit changes by 10 km/h. On the average, the change in the mean speed of traffic induced by a change in speed limit appears to be around 25% of the change in speed limit. This means that if the speed limit is reduced by 10 km/h, on may expect the mean speed of traffic to go down by about 2.5 km/h. This will often mean that the percentage of violators increases when speed limits are lowered (fewer drivers will comply with the new speed limit than with the old speed limit) and decreases when speed limits are increased. In fact, raising speed limits is sometimes proposed as a means of reducing the proportion of violations.

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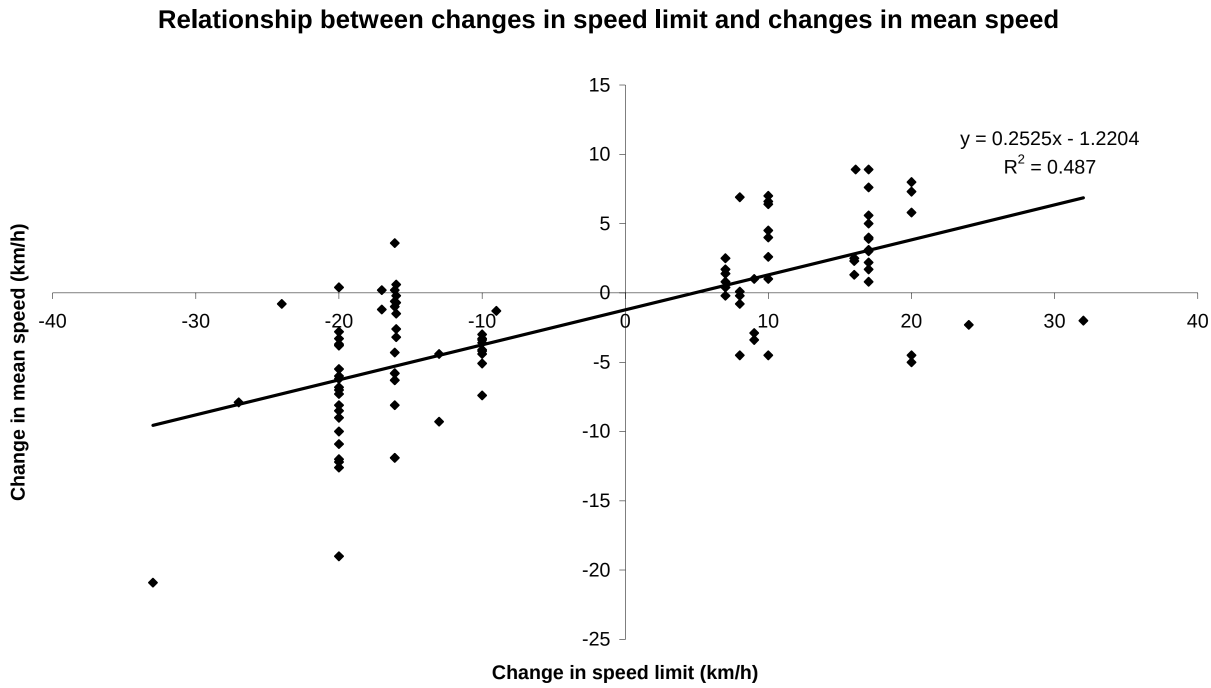
If changes in speed limits are combined with traffic engineering measures, like humps or environmental streets, the effect on speed tends to be greater than if a change in the speed limit is the only measure used.



Source: TØI report 740/2004

Figure 23: Relationship between changes in speed limits and changes in the mean speed of traffic

The changes in mean speed associated with police enforcement tend to be smaller than those resulting from changes in speed limits, in most cases smaller than 3 km/h (as evidenced in the data used in this study).

8.4 Principles for setting speed limits

Various approaches can be taken for setting speed limits. A discussion of the various options and perspectives that can be taken is given in paper by Elvik (2002). Some of the major points of that paper are repeated here. Various principles have been proposed for setting speed limits. These include:

1. Adapting speed limits to actual driving speed, such as the 85th percentile of the speed distribution, to ensure that the limits seem reasonable from motorists' point of view and are not too widely disregarded,
2. Setting speed limits according to roadway geometry (low speed limits on narrow and winding roads, high speed limits on straight and wide roads),
3. Setting speed limits according the type and level of roadside development (low speed limits in residential and commercial areas, high speed limits in rural areas)

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4. Setting speed limits according to human tolerance for biomechanical energy, in order to ensure that nobody is killed or permanently injured (Vision Zero speed limits)
5. Setting speed limits so as to minimize the total societal costs of transport. Speed limits set this way are generally referred to as optimal speed limits.

In practice, the setting of speed limits is often based on a mixture of these principles, as well as other considerations, not taken explicitly into account by any of the principles.

Optimal speed limits are those that minimize the total costs to society of transport. The following impacts of speed are normally included in these costs when optimal speed limits are estimated:

1. Costs of travel time
2. Vehicle operating costs
3. Road accident costs
4. Costs of traffic noise
5. Costs of air pollution, and possibly
6. Costs of road maintenance, as these depend on speed

In the paper published in 2002 (Elvik 2002), four perspectives that can be adopted when assessing costs and benefits of speed limits were compared. These were:

1. The societal perspective, which includes all cost items listed above, without regard to whether these costs are internal or external from the motorists point of view.
2. The road user perspective, which includes those costs that the road user either pays out-of-pocket, or which can reasonably be assumed to be completely internalised by the road user in his or her choice of speed.
3. The taxpayer perspective, which includes those costs that are not subject to taxation of the use of motor vehicles.
4. The residential perspective, according to which the choice of speed is seen from the point of view of residents along a road. In this perspective fast traffic may be a nuisance and a source of worry, despite the fact that saving travel time is generally regarded as a benefit from a traveler's point of view.

These perspectives differ in terms of the cost items included. The societal perspective includes all cost items fully. The road user perspective includes 100% of the costs of travel time and vehicle operating costs. It also includes the internalised costs of road accidents, but not the costs of traffic noise or air pollution.

The various perspectives were found to give very different results with respect to optimal speed limits. An analysis was made for Norway and Sweden. Table 28 shows the speed limits that were found to be optimal in Sweden, employing the societal perspective, as well as current speed limits and those that are based on Vision Zero, making human tolerance to biomechanical impact the basic design parameter of the road transport system.

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As can be seen from Table 28, current speed limits in Sweden tend to be too high in rural areas and, somewhat surprisingly, too low in urban areas. The latter finding is attributable mainly to the high value accorded to travel time in Sweden. Speeds as high as 60 km/h would make interaction between cars and unprotected road users more difficult.

Table 28: Speed limits in Sweden according to different principles

Type of road	Current mean speed of travel (km/h)	Current speed limit (km/h)	Optimal speed limit from the societal perspective	Vision Zero speed limits
Motorway A	109	110	110	110
Motor traffic road	108	110	90	70
Motor traffic road	96	90	80	70
Rural highway	95	90	80	70
Urban arterial	50	50	60	50
Access road	39	30	60	30

Source: TØI report 740/2004

While theoretically attractive, optimality models have a number of limitations when applied to the determination of speed limits. These limitations include:

1. The list of impacts included is unlikely to be complete. Historically, this list has expanded as new items have been added once acceptable monetary valuations of these items have been obtained. Items not included in this analysis are driver comfort, speed as a barrier to pedestrians or cyclists, and anxiety among residents.
2. Monetary valuation of non-market impacts of speed is not an exact science. The valuations used tend to be highly uncertain and are often controversial.
3. For motorways, optimal speed limits are close to indeterminate in the range between about 70 km/h and 110 km/h. Total costs are almost flat in this range. Speeds limit of 70 or 110 km/h will, however, have very different impacts on road safety. These different impacts are not apparent when the total costs of transport are considered.
4. The determination of optimal speed limits needs to consider enforcement explicitly. A speed limit requiring very extensive enforcement will not be optimal, even if it minimises all other costs of transport. While enforcement is subject to decreasing marginal returns, which means that it is usually not optimal to enforce speed limits so as to ensure 100% compliance, it is not desirable to set speed limits that nobody complies with.

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8.5 The enforcement of speed limits

Speed limits need enforcement. Traditionally, enforcement has been carried out by uniformed police, operating patrols or stationary check points. In recent years, new technology has been introduced in speed enforcement, in particular speed cameras. Speed cameras are now widely used in many countries, including Australia, Great Britain, Norway and Sweden. In the immediate vicinity of speed cameras, compliance tends to be quite high, normally in the range 80-95 percent.

In-vehicle technology that supports drivers, or even forces them to comply with speed limits, has also been developed (see e.g. Varhelyi et al 2004). This technology has so far not been widely applied, but is sufficiently reliable to be used on a wider scale. If widely applied, in-vehicle technology could eliminate the need for traditional enforcement.

It is outside the scope of this report to discuss the enforcement of speed limits in greater detail.

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9 Main conclusions

The main findings of the research presented in this report can be summarised as follows:

1. There is a strong statistical relationship between speed and road safety. When the mean speed of traffic is reduced, the number of accidents and the severity of injuries will almost always go down. When the mean speed of traffic increases, the number of accidents and the severity of injuries will usually increase.
2. The relationship between changes in speed and changes in road safety holds for all speeds in the range between about 25 km/h and about 120 km/h.
3. The relationship between changes in speed and changes in road safety can be adequately described in terms of a power model, in which the relative change in the number of accidents or accident victims is a function of the relative change in the mean speed of traffic, raised to an exponent. The following exponents summarise the effects of changes in speed:
    a. Fatalities: 4.5
    b. Fatal accidents: 3.6
    c. Seriously injured road users: 3.0
    d. Serious injury accidents: 2.4
    e. Slightly injured road users: 1.5
    f. Slight injury accidents: 1.2
    g. Injured road users (severity unspecified): 2.7
    h. Injury accidents (severity unspecified): 2.0
    i. Property-damage-only accidents : 1.0
4. Several other mathematical functions may describe the relationship between speed and road safety, but the generality and simplicity of the power model makes it superior to other models. The model is, however, not necessarily valid outside the range of speeds found in the present study (from about 25 km/h to about 120 km/h).
5. The relationship between speed and road safety is causal and can be explained in terms of elementary laws of physics and biomechanics. Speed is clearly a very important risk factor with respect to both accident occurrence and injury severity.

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6. The relationship between speed and road safety can to some extent be modified by the road environment, by vehicle-related factors, and by driver behaviour, but the effects of speed on road safety appear to be remarkably consistent across different contexts.
7. The findings of the review presented in this report are very unlikely to be artefacts of poor data or inadequate research methods. In particular, it can be ruled out that the findings are attributable to publication bias, poor quality of the primary studies or analytic choices made as part of the meta-analysis.
8. The regulation of speed remains a controversial and emotionally charged subject. It is not within the remit of research to determine how best to regulate speed, but the importance of speed as a risk factor, and the fact that not all drivers at all times are likely to be perfectly rational in their choice of speed clearly suggests that there is a need to formally regulate permitted driving speed.

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APPENDIX 1: DATA RECORDED FOR EACH STUDY

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Study rec no	Result rec no	Authors	Publ year	Data country	Acc/inj severity	Accs or victims	Speed change	Acc/Vic change	Est of power
1	1	Munden	1966	GBR	SER	ACC	0.951	0.446	15.968
1	2	Munden	1966	GBR	SLI	ACC	0.951	0.56	11.459
1	3	Munden	1966	GBR	SER	ACC	1.029	0.36	-35.667
1	4	Munden	1966	GBR	SLI	ACC	1.029	1.226	7.117
1	5	Munden	1966	GBR	SER	ACC	0.955	1.355	-6.647
1	6	Munden	1966	GBR	SLI	ACC	0.955	1.56	-9.739
1	7	Munden	1966	GBR	SER	ACC	0.972	1.317	-9.752
1	8	Munden	1966	GBR	SLI	ACC	0.972	0.637	15.993
1	9	Munden	1966	GBR	SER	ACC	0.983	0.854	9.146
1	10	Munden	1966	GBR	SLI	ACC	0.983	0.553	34.319
1	11	Munden	1966	GBR	SER	ACC	0.983	0.731	18.44
1	12	Munden	1966	GBR	SLI	ACC	0.983	1.685	-30.734
2	13	Ekstrøm,Kritz,Strømgren	1967	SWE	INJ	ACC	0.979	0.373	47.375
2	14	Ekstrøm,Kritz,Strømgren	1967	SWE	PDO	ACC	0.979	0.814	9.895
3	15	Hall	1970	IRL	INJ	ACC	1.005	0.956	-9.111
3	16	Hall	1970	IRL	FAT	ACC	1.005	1.021	4.16
4	17	Jønrup,Svensson	1971	SWE	INJ	ACC	0.992	1.065	-7.551
4	18	Jønrup,Svensson	1971	SWE	INJ	ACC	1.095	1.356	3.359
4	19	Jønrup,Svensson	1971	SWE	INJ	ACC	1.075	1.475	5.344
4	20	Jønrup,Svensson	1971	SWE	INJ	ACC	0.972	0.835	6.359
4	21	Jønrup,Svensson	1971	SWE	INJ	ACC	0.968	0.926	2.381
4	22	Jønrup,Svensson	1971	SWE	INJ	ACC	0.952	0.807	4.312
4	23	Jønrup,Svensson	1971	SWE	INJ	ACC	0.935	0.87	2.062
4	24	Jønrup,Svensson	1971	SWE	INJ	ACC	0.934	0.741	4.397
4	25	Jønrup,Svensson	1971	SWE	INJ	ACC	0.921	0.838	2.135
4	26	Jønrup,Svensson	1971	SWE	INJ	ACC	0.913	0.68	4.266
4	27	Jønrup,Svensson	1971	SWE	INJ	ACC	0.907	0.773	2.635
4	28	Jønrup,Svensson	1971	SWE	INJ	ACC	0.907	0.697	3.679
4	29	Jønrup,Svensson	1971	SWE	INJ	ACC	0.891	0.693	3.19
4	30	Jønrup,Svensson	1971	SWE	INJ	ACC	0.884	0.864	1.179
4	31	Jønrup,Svensson	1971	SWE	INJ	ACC	0.881	1.117	-0.873
4	32	Jønrup,Svensson	1971	SWE	INJ	ACC	0.867	0.704	2.462
4	33	Jønrup,Svensson	1971	SWE	INJ	ACC	0.866	0.85	1.13
4	34	Jønrup,Svensson	1971	SWE	FAT	ACC	0.972	0.767	9.344
4	35	Jønrup,Svensson	1971	SWE	FAT	ACC	0.935	0.827	2.816
4	36	Jønrup,Svensson	1971	SWE	FAT	ACC	0.907	0.821	2.023
5	37	Wahlgren	1972	FIN	INJ	ACC	1.108	1.525	4.11
5	38	Wahlgren	1972	FIN	INJ	ACC	0.892	1.277	-2.134
5	39	Wahlgren	1972	FIN	INJ	ACC	0.988	0.856	12.836
6	40	Andersson,Nilsson	1974	SWE	PDO	ACC	0.915	0.84	1.962
6	41	Andersson,Nilsson	1974	SWE	INJ	ACC	0.915	0.834	2.049
6	42	Andersson,Nilsson	1974	SWE	FAT	VIC	0.915	0.875	1.505
7	43	Brodersen,Jørgensen,Lund	1975	DEN	INJ	ACC	0.952	0.718	6.704
7	44	Brodersen,Jørgensen,Lund	1975	DEN	INJ	ACC	0.915	0.823	2.199
7	45	Brodersen,Jørgensen,Lund	1975	DEN	INJ	ACC	0.908	0.621	4.947
8	46	Brodin,Ringhagen	1975	SWE	INJ	ACC	1.012	1.133	10.232

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Study rec no	Result rec no	Authors	Publ year	Data country	Acc/inj severity	Accs or victims	Speed change	Acc/Vic change	Est of power
9	47	Nilsson	1976	SWE	FAT	ACC	1.071	2.912	15.637
9	48	Nilsson	1976	SWE	INJ	ACC	1.071	1.23	3.031
9	49	Nilsson	1976	SWE	PDO	ACC	1.071	1.27	3.495
9	50	Nilsson	1976	SWE	FAT	VIC	1.071	0.828	-2.766
9	51	Nilsson	1976	SWE	INJ	ACC	1.071	1.02	0.284
9	52	Nilsson	1976	SWE	PDO	ACC	1.071	1.079	1.106
9	53	Nilsson	1976	SWE	FAT	VIC	1.071	2.734	14.715
9	54	Nilsson	1976	SWE	INJ	ACC	1.071	1.554	6.448
9	55	Nilsson	1976	SWE	PDO	ACC	1.071	1.561	6.518
9	56	Nilsson	1976	SWE	FAT	VIC	0.922	0.581	6.653
9	57	Nilsson	1976	SWE	INJ	ACC	0.922	0.717	4.081
9	58	Nilsson	1976	SWE	PDO	ACC	0.922	0.812	2.549
10	59	Burritt et al	1976	USA	FAT	ACC	0.922	0.649	5.324
11	60	Scott,Barton	1976	GBR	INJ	ACC	0.965	0.896	3.102
11	61	Scott,Barton	1976	GBR	INJ	ACC	0.814	0.658	2.04
12	62	Kemper,Byington	1977	USA	FAT	VIC	0.886	0.558	4.829
12	63	Kemper,Byington	1977	USA	FAT	VIC	0.931	0.649	6.083
12	64	Kemper,Byington	1977	USA	FAT	VIC	0.937	0.732	4.794
12	65	Kemper,Byington	1977	USA	INJ	VIC	0.886	0.718	2.744
12	66	Kemper,Byington	1977	USA	INJ	VIC	0.931	0.798	3.177
12	67	Kemper,Byington	1977	USA	INJ	VIC	0.937	0.846	2.567
13	68	Daltrey, Healy	1980	AUS	FAT	ACC	0.956	0.916	1.977
13	69	Daltrey, Healy	1980	AUS	INJ	ACC	0.956	0.505	15.31
13	70	Daltrey, Healy	1980	AUS	FAT	ACC	0.956	1.076	-1.65
13	71	Daltrey, Healy	1980	AUS	INJ	ACC	0.956	1.048	-1.056
14	72	Nilsson	1980	SWE	FAT	ACC	0.888	0.613	4.121
14	73	Nilsson	1980	SWE	SER	ACC	0.888	0.901	0.874
14	74	Nilsson	1980	SWE	SLI	ACC	0.888	0.824	1.626
14	75	Nilsson	1980	SWE	FAT	ACC	0.929	0.656	5.684
14	76	Nilsson	1980	SWE	SER	ACC	0.929	0.926	1.037
14	77	Nilsson	1980	SWE	SLI	ACC	0.929	1.224	-2.723
15	78	Roop, Brackett	1980	USA	FAT	ACC	0.974	1.238	-8.178
15	79	Roop, Brackett	1980	USA	INJ	ACC	0.974	0.763	10.358
15	80	Roop, Brackett	1980	USA	PDO	ACC	0.974	0.673	15.169
15	81	Roop, Brackett	1980	USA	FAT	ACC	1.042	0.848	-4.002
15	82	Roop, Brackett	1980	USA	INJ	ACC	1.042	1.073	1.718
15	83	Roop, Brackett	1980	USA	PDO	ACC	1.042	0.934	-1.669
15	84	Roop, Brackett	1980	USA	FAT	ACC	0.96	0.5	16.816
15	85	Roop, Brackett	1980	USA	INJ	ACC	0.96	1.066	-1.544
15	86	Roop, Brackett	1980	USA	PDO	ACC	0.96	0.973	0.67
15	87	Roop, Brackett	1980	USA	FAT	ACC	0.998	0.818	93.212
15	88	Roop, Brackett	1980	USA	INJ	ACC	0.998	0.79	109.397
15	89	Roop, Brackett	1980	USA	PDO	ACC	0.998	0.84	80.915
15	90	Roop, Brackett	1980	USA	FAT	ACC	0.973	0.933	2.481
15	91	Roop, Brackett	1980	USA	INJ	ACC	0.973	0.562	20.701
15	92	Roop, Brackett	1980	USA	PDO	ACC	0.973	0.92	3.01
15	93	Roop, Brackett	1980	USA	FAT	ACC	1.005	0.6	-94.758
15	94	Roop, Brackett	1980	USA	INJ	ACC	1.005	1.075	13.386
15	95	Roop, Brackett	1980	USA	PDO	ACC	1.005	0.88	-23.771

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Study rec no	Result rec no	Authors	Publ year	Data country	Acc/inj severity	Accs or victims	Speed change	Acc/Vic change	Est of power
16	96	Brackett, Beecher	1980	USA	FAT	ACC	0.967	0.967	0.991
16	97	Brackett, Beecher	1980	USA	INJ	ACC	0.967	0.994	0.188
16	98	Brackett, Beecher	1980	USA	PDO	ACC	0.967	1.043	-1.256
17	99	Amundsen	1981	NOR	INJ	ACC	0.953	0.583	11.095
17	100	Amundsen	1981	NOR	INJ	ACC	0.891	0.528	5.512
18	101	Christensen	1981	DEN	INJ	ACC	0.952	0.765	5.425
18	102	Christensen	1981	DEN	INJ	ACC	0.908	0.667	4.21
19	103	Koshi, Kashima	1981	JPN	INJ	ACC	1.169	0.984	-0.1
19	104	Koshi, Kashima	1981	JPN	FAT	VIC	1.169	0.833	-1.166
19	105	Koshi, Kashima	1981	JPN	SER	VIC	1.169	1.05	0.309
20	106	Salusjärvi	1981	FIN	FAT	ACC	0.967	1.333	-8.558
20	107	Salusjärvi	1981	FIN	FAT	ACC	0.927	0.87	1.832
20	108	Salusjärvi	1981	FIN	FAT	ACC	0.92	0.529	7.6
20	109	Salusjärvi	1981	FIN	FAT	ACC	0.908	0.8	2.311
20	110	Salusjärvi	1981	FIN	INJ	ACC	0.967	0.649	12.876
20	111	Salusjärvi	1981	FIN	INJ	ACC	0.927	0.613	6.415
20	112	Salusjärvi	1981	FIN	INJ	ACC	0.92	0.547	7.213
20	113	Salusjärvi	1981	FIN	INJ	ACC	0.908	0.579	5.655
20	114	Salusjärvi	1981	FIN	PDO	ACC	0.967	0.955	1.384
20	115	Salusjärvi	1981	FIN	PDO	ACC	0.927	0.643	5.793
20	116	Salusjärvi	1981	FIN	PDO	ACC	0.92	0.684	4.544
20	117	Salusjärvi	1981	FIN	PDO	ACC	0.908	0.844	1.758
20	118	Salusjärvi	1981	FIN	FAT	ACC	0.957	0.679	8.834
20	119	Salusjärvi	1981	FIN	FAT	ACC	0.977	0.569	24.307
20	120	Salusjärvi	1981	FIN	FAT	ACC	1.021	1.132	5.954
20	121	Salusjärvi	1981	FIN	INJ	ACC	0.957	0.706	7.967
20	122	Salusjärvi	1981	FIN	INJ	ACC	0.977	0.96	1.752
20	123	Salusjärvi	1981	FIN	INJ	ACC	1.021	1.412	16.552
20	124	Salusjärvi	1981	FIN	PDO	ACC	0.957	0.795	5.234
20	125	Salusjärvi	1981	FIN	PDO	ACC	0.977	1.182	-7.208
20	126	Salusjärvi	1981	FIN	PDO	ACC	1.021	1.118	5.372
21	127	Frith,Toomath	1982	NZL	INJ	ACC	0.9	0.815	1.935
21	128	Frith,Toomath	1982	NZL	FAT	VIC	0.9	0.741	2.837
21	129	Frith,Toomath	1982	NZL	SER	VIC	0.9	0.833	1.731
21	130	Frith,Toomath	1982	NZL	SLI	VIC	0.9	0.814	1.949
22	131	Salusjärvi	1982	NOR	INJ	ACC	0.832	0.85	0.884
22	132	Salusjärvi	1982	NOR	INJ	ACC	0.943	0.955	0.798
23	133	Baguley	1982	GBR	INJ	ACC	0.523	0.235	2.235
23	134	Baguley	1982	GBR	INJ	ACC	0.65	0.253	3.195
23	135	Baguley	1982	GBR	INJ	ACC	0.558	1.269	-0.408
23	136	Baguley	1982	GBR	INJ	ACC	0.704	1.232	-0.595
23	137	Baguley	1982	GBR	INJ	ACC	0.658	4.907	-3.806
23	138	Baguley	1982	GBR	INJ	ACC	0.787	0.196	6.82
23	139	Baguley	1982	GBR	INJ	ACC	0.667	0.301	2.962
24	140	Amundsen	1983	NOR	INJ	ACC	0.932	0.943	0.83
25	141	Jørgensen et al	1985	NORD	INJ	VIC	1.053	0.865	-2.826
25	142	Jørgensen et al	1985	NORD	INJ	VIC	1.078	1.211	2.556
25	143	Jørgensen et al	1985	NORD	INJ	VIC	1.01	1.258	22.345
25	144	Jørgensen et al	1985	NORD	INJ	VIC	0.956	0.982	0.406

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Study rec no	Result rec no	Authors	Publ year	Data country	Acc/inj severity	Accs or victims	Speed change	Acc/Vic change	Est of power
26	145	Borges et al	1985	DEN	INJ	ACC	0.617	0.486	1.495
26	146	Borges et al	1985	DEN	PDO	ACC	0.617	1.113	-0.222
26	147	Borges et al	1985	DEN	INJ	ACC	0.915	0.164	20.418
26	148	Borges et al	1985	DEN	INJ	ACC	0.983	0.409	52.257
26	149	Borges et al	1985	DEN	PDO	ACC	0.983	0.341	62.922
27	150	Sakshaug	1986	NOR	INJ	ACC	1.032	0.99	-0.323
27	151	Sakshaug	1986	NOR	INJ	ACC	0.945	0.979	0.366
27	152	Sakshaug	1986	NOR	INJ	ACC	0.941	1.143	-2.191
27	153	Sakshaug	1986	NOR	INJ	ACC	0.94	0.883	1.996
28	154	Engel	1987	DEN	INJ	ACC	0.946	0.937	1.174
29	155	Ullmann, Dudek	1987	USA	PDO	ACC	1.003	0.431	-322.02
29	156	Ullmann, Dudek	1987	USA	INJ	ACC	1.003	0.961	-15.322
29	157	Ullmann, Dudek	1987	USA	PDO	ACC	0.988	0.588	45.156
29	158	Ullmann, Dudek	1987	USA	INJ	ACC	0.988	2.231	-68.279
29	159	Ullmann, Dudek	1987	USA	PDO	ACC	1.046	0.639	-10.012
29	160	Ullmann, Dudek	1987	USA	INJ	ACC	1.046	0.554	-13.179
29	161	Ullmann, Dudek	1987	USA	PDO	ACC	0.992	1.419	-41.032
29	162	Ullmann, Dudek	1987	USA	INJ	ACC	0.992	1	0
29	163	Ullmann, Dudek	1987	USA	PDO	ACC	0.903	1.073	-0.697
29	164	Ullmann, Dudek	1987	USA	INJ	ACC	0.903	0.886	1.194
29	165	Ullmann, Dudek	1987	USA	PDO	ACC	0.95	0.926	1.499
29	166	Ullmann, Dudek	1987	USA	INJ	ACC	0.95	1.804	-11.534
30	167	Dietrich et al	1988	SWT	PDO	ACC	0.957	1.009	-0.215
30	168	Dietrich et al	1988	SWT	FAT	VIC	0.957	1.104	-2.264
30	169	Dietrich et al	1988	SWT	SER	VIC	0.957	0.845	3.859
30	170	Dietrich et al	1988	SWT	SLI	VIC	0.957	0.855	3.604
30	171	Dietrich et al	1988	SWT	PDO	ACC	0.905	0.958	0.434
30	172	Dietrich et al	1988	SWT	FAT	VIC	0.905	0.67	4.004
30	173	Dietrich et al	1988	SWT	SER	VIC	0.905	0.929	0.737
30	174	Dietrich et al	1988	SWT	SLI	VIC	0.905	1.013	-0.126
31	175	Engel,Krogsgård Thomsen	1988	DEN	FAT	VIC	0.949	0.759	5.22
31	176	Engel,Krogsgård Thomsen	1988	DEN	SER	VIC	0.949	0.93	1.374
31	177	Engel,Krogsgård Thomsen	1988	DEN	SLI	VIC	0.949	0.894	2.121
32	178	Salusjärvi,Mäkinen	1988	FIN	INJ	ACC	0.982	1.166	-8.379
32	179	Salusjärvi,Mäkinen	1988	FIN	PDO	ACC	0.982	1.076	-4.014
32	180	Salusjärvi,Mäkinen	1988	FIN	INJ	ACC	0.982	1.269	-12.972
32	181	Salusjärvi,Mäkinen	1988	FIN	PDO	ACC	0.982	1.214	-10.577
33	182	Stølan	1988	NOR	INJ	ACC	0.91	0.179	18.193
33	183	Stølan	1988	NOR	INJ	ACC	0.829	1.236	-1.13
34	184	Upchurch	1989	USA	FAT	ACC	1.093	1.211	2.156
34	185	Upchurch	1989	USA	INJ	ACC	1.093	1.333	3.232
34	186	Upchurch	1989	USA	PDO	ACC	1.093	1.12	1.279
35	187	US DOT	1989	USA	FAT	VIC	1.032	1.197	5.716
35	188	US DOT	1989	USA	FAT	VIC	1.022	0.909	-4.399
36	189	Gallaher et al	1989	USA	FAT	ACC	1.059	1.641	8.65
37	190	Rijkswaterstaat	1989	NED	INJ	VIC	0.965	1.206	-5.231
37	191	Rijkswaterstaat	1989	NED	FAT	VIC	0.965	0.659	11.637

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38	192	McCartt, Rood	1989	USA	FAT	ACC	0.995	0.667	80.16
38	193	McCartt, Rood	1989	USA	INJ	ACC	0.995	1.057	-10.92
38	194	McCartt, Rood	1989	USA	PDO	ACC	0.995	0.935	13.24
38	195	McCartt, Rood	1989	USA	FAT	ACC	0.998	0.776	126.344
38	196	McCartt, Rood	1989	USA	INJ	ACC	0.998	1.021	-10.456
38	197	McCartt, Rood	1989	USA	PDO	ACC	0.998	1.073	-34.952
39	198	Pigman et al	1989	USA	INJ	ACC	0.972	1.086	-2.886
39	199	Pigman et al	1989	USA	PDO	ACC	0.972	0.973	0.953
39	200	Pigman et al	1989	USA	INJ	ACC	0.998	0.959	21.024
39	201	Pigman et al	1989	USA	PDO	ACC	0.998	0.771	131.457
40	202	Brown et al	1990	USA	FAT	ACC	1.038	1.368	8.342
40	203	Brown et al	1990	USA	FAT	ACC	1.038	1.211	5.1
40	204	Brown et al	1990	USA	INJ	ACC	1.038	1.025	0.659
40	205	Brown et al	1990	USA	INJ	ACC	1.038	1.143	3.562
40	206	Brown et al	1990	USA	PDO	ACC	1.038	1.032	0.85
40	207	Brown et al	1990	USA	PDO	ACC	1.038	1.205	4.963
41	208	Nilsson	1990	SWE	FAT	ACC	0.889	0.975	0.219
41	209	Nilsson	1990	SWE	INJ	ACC	0.889	0.893	0.958
41	210	Nilsson	1990	SWE	FAT	ACC	0.971	0.602	17.332
41	211	Nilsson	1990	SWE	INJ	ACC	0.971	0.941	2.076
42	212	Smith	1990	USA	FAT	ACC	1.04	1.134	3.202
42	213	Smith	1990	USA	FAT	ACC	1.04	1.165	3.886
43	214	Roszbach	1990	NED	FAT	ACC	0.955	1.12	-2.473
44	215	Sidhu	1990	USA	FAT	ACC	1.053	0.958	-0.838
44	216	Sidhu	1990	USA	INJ	ACC	1.053	1.04	0.761
44	217	Sidhu	1990	USA	PDO	ACC	1.053	1.152	2.738
44	218	Sidhu	1990	USA	FAT	ACC	1.053	1.119	2.174
44	219	Sidhu	1990	USA	INJ	ACC	1.053	1.069	1.29
44	220	Sidhu	1990	USA	PDO	ACC	1.053	1.074	1.39
45	221	Giæver,Meland	1990	NOR	INJ	ACC	0.824	0.249	7.166
45	222	Giæver,Meland	1990	NOR	INJ	ACC	0.761	0.446	2.953
45	223	Giæver,Meland	1990	NOR	INJ	ACC	0.748	0.486	2.483
46	224	Engel, Thomsen	1990	DEN	SER	VIC	0.725	0.297	3.767
46	225	Engel, Thomsen	1990	DEN	SLI	VIC	0.725	0.612	1.522
47	226	Jernigan, Lynn	1991	USA	FAT	ACC	1.08	1.466	4.95
48	227	Andersson	1991	SWE	INJ	ACC	0.961	0.846	4.224
48	228	Andersson	1991	SWE	PDO	ACC	0.961	0.988	0.306
48	229	Andersson	1991	SWE	INJ	ACC	0.983	1.184	-9.778
48	230	Andersson	1991	SWE	PDO	ACC	0.983	0.901	6.035
48	231	Andersson	1991	SWE	INJ	ACC	0.993	1.436	-48.555
48	232	Andersson	1991	SWE	PDO	ACC	0.993	1.027	-3.58
49	233	Angenendt	1991	GER	INJ	ACC	0.956	0.612	10.976
49	234	Angenendt	1991	GER	PDO	ACC	0.956	0.982	0.407
50	235	Freiholtz	1991	SWE	INJ	ACC	0.82	0.14	9.895
50	236	Freiholtz	1991	SWE	PDO	ACC	0.82	0.508	3.413
51	237	Sliogeris	1992	AUS	INJ	ACC	1.01	1.489	40.009
51	238	Sliogeris	1992	AUS	INJ	ACC	1.019	1.148	7.315
52	239	Godwin	1992	USA	FAT	VIC	1.05	1.268	4.918
52	240	Godwin	1992	USA	FAT	VIC	1.016	1.041	2.562

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53	241	Baier m fl	1992	GER	INJ	ACC	0.789	0.23	6.222
53	242	Baier m fl	1992	GER	PDO	ACC	0.789	0.685	1.6
54	243	Schnüll,Lange	1992	GER	SER	ACC	0.923	0.565	7.128
54	244	Schnüll,Lange	1992	GER	SLI	ACC	0.923	0.776	3.176
54	245	Schnüll,Lange	1992	GER	PDO	ACC	0.923	0.71	4.285
55	246	Oei Hway-liem and Polak	1992	NED	INJ	ACC	0.947	0.945	1.033
55	247	Oei Hway-liem and Polak	1992	NED	PDO	ACC	0.947	0.798	4.12
55	248	Oei Hway-liem and Polak	1992	NED	INJ	ACC	0.946	0.619	8.554
55	249	Oei Hway-liem and Polak	1992	NED	PDO	ACC	0.946	0.551	10.652
56	250	Baier	1992	GER	INJ	ACC	0.92	0.818	2.422
56	251	Baier	1992	GER	PDO	ACC	0.92	0.785	2.929
57	252	Aakjer Nielsen,Herrstedt	1993	DEN	INJ	ACC	0.898	0.303	11.101
57	253	Aakjer Nielsen,Herrstedt	1993	DEN	PDO	ACC	0.898	0.533	5.846
57	254	Aakjer Nielsen,Herrstedt	1993	DEN	INJ	ACC	0.877	0.738	2.319
57	255	Aakjer Nielsen,Herrstedt	1993	DEN	PDO	ACC	0.877	1.033	-0.249
57	256	Aakjer Nielsen,Herrstedt	1993	DEN	INJ	ACC	0.846	0.307	7.065
57	257	Aakjer Nielsen,Herrstedt	1993	DEN	PDO	ACC	0.846	1.843	-3.66
58	258	Herrstedt et al	1993	DEN	INJ	ACC	0.571	3.243	-2.102
58	259	Herrstedt et al	1993	DEN	PDO	ACC	0.571	1.247	-0.395
58	260	Herrstedt et al	1993	DEN	INJ	ACC	0.952	0.985	0.309
58	261	Herrstedt et al	1993	DEN	PDO	ACC	0.952	1.63	-10.019
58	262	Herrstedt et al	1993	DEN	INJ	ACC	0.714	0.401	2.716
58	263	Herrstedt et al	1993	DEN	PDO	ACC	0.714	0.535	1.861
58	264	Herrstedt et al	1993	DEN	INJ	ACC	0.746	0.846	0.571
58	265	Herrstedt et al	1993	DEN	PDO	ACC	0.746	0.448	2.739
58	266	Herrstedt et al	1993	DEN	INJ	ACC	0.898	0.595	4.848
58	267	Herrstedt et al	1993	DEN	PDO	ACC	0.898	0.885	1.144
58	268	Herrstedt et al	1993	DEN	INJ	ACC	0.931	0.544	8.513
58	269	Herrstedt et al	1993	DEN	PDO	ACC	0.931	0.505	9.55
58	270	Herrstedt et al	1993	DEN	INJ	ACC	0.963	1.149	-3.671
58	271	Herrstedt et al	1993	DEN	PDO	ACC	0.963	0.351	27.745
58	272	Herrstedt et al	1993	DEN	INJ	ACC	0.795	0.949	0.229
58	273	Herrstedt et al	1993	DEN	PDO	ACC	0.795	0.647	1.894
58	274	Herrstedt et al	1993	DEN	INJ	ACC	0.872	0.493	5.183
58	275	Herrstedt et al	1993	DEN	PDO	ACC	0.872	0.862	1.086
58	276	Herrstedt et al	1993	DEN	INJ	ACC	0.898	1.107	-0.947
58	277	Herrstedt et al	1993	DEN	PDO	ACC	0.898	3.32	-11.203
58	278	Herrstedt et al	1993	DEN	INJ	ACC	0.937	0.553	9.023
58	279	Herrstedt et al	1993	DEN	PDO	ACC	0.937	0.506	10.389
58	280	Herrstedt et al	1993	DEN	INJ	ACC	0.84	0.286	7.207
58	281	Herrstedt et al	1993	DEN	PDO	ACC	0.84	1.429	-2.054
58	282	Herrstedt et al	1993	DEN	INJ	ACC	0.515	1.065	-0.094
58	283	Herrstedt et al	1993	DEN	INJ	ACC	0.478	0.413	1.198
58	284	Herrstedt et al	1993	DEN	PDO	ACC	0.478	0.427	1.152
58	285	Herrstedt et al	1993	FRA	INJ	ACC	0.567	0.605	0.885
58	286	Herrstedt et al	1993	FRA	INJ	ACC	0.5	0.091	3.459
59	287	Sammer	1994	AUT	SER	VIC	0.926	0.764	3.478
59	288	Sammer	1994	AUT	SLI	VIC	0.926	0.879	1.66
60	289	Engel,Andersen	1994	DEN	INJ	ACC	0.818	0.186	8.381

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61	290	Rock	1995	USA	FAT	VIC	1.031	1.374	10.465
61	291	Rock	1995	USA	INJ	VIC	1.031	1.208	6.219
61	292	Rock	1995	USA	PDO	ACC	1.031	1.217	6.473
62	293	Wheeler,Taylor	1995	GBR	INJ	ACC	0.956	0.764	6.027
62	294	Wheeler,Taylor	1995	GBR	INJ	ACC	0.943	0.688	6.324
62	295	Wheeler,Taylor	1995	GBR	INJ	ACC	0.944	0.931	1.239
62	296	Wheeler,Taylor	1995	GBR	INJ	ACC	0.989	1.251	-20.107
62	297	Wheeler,Taylor	1995	GBR	INJ	ACC	0.933	0.486	10.402
62	298	Wheeler,Taylor	1995	GBR	INJ	ACC	0.841	1.34	-1.692
63	299	Webster,Mackie	1996	GBR	FAT	ACC	0.632	0.248	3.038
63	300	Webster,Mackie	1996	GBR	SER	ACC	0.632	0.413	1.927
63	301	Webster,Mackie	1996	GBR	SLI	ACC	0.632	0.57	1.226
64	302	ETSC	1996	DEN	INJ	ACC	0.82	0.4	4.617
64	303	ETSC	1996	DEN	INJ	ACC	0.867	1.667	-3.57
64	304	ETSC	1996	DEN	INJ	ACC	0.836	0.5	3.871
64	305	ETSC	1996	DEN	INJ	ACC	0.791	1	0
64	306	ETSC	1996	DEN	INJ	ACC	0.742	2	-2.327
64	307	ETSC	1996	DEN	INJ	ACC	0.81	1	0
64	308	ETSC	1996	DEN	INJ	ACC	0.904	0.188	16.558
64	309	ETSC	1996	DEN	INJ	ACC	0.82	0.143	9.786
64	310	ETSC	1996	DEN	INJ	ACC	0.833	0.143	10.673
64	311	ETSC	1996	DEN	INJ	ACC	0.862	3	-7.402
65	312	Griborn	1996	SWE	INJ	ACC	0.898	0.75	2.678
66	313	Parker	1997	USA	INJ	ACC	1.008	0.848	-21.572
66	314	Parker	1997	USA	INJ	ACC	1.016	0.923	-4.957
66	315	Parker	1997	USA	INJ	ACC	0.988	0.388	80.491
66	316	Parker	1997	USA	INJ	ACC	1.003	2.277	298.322
66	317	Parker	1997	USA	INJ	ACC	0.983	1.45	-21.556
66	318	Parker	1997	USA	INJ	ACC	1.008	0.628	-60.401
66	319	Parker	1997	USA	INJ	ACC	0.997	0.425	293.245
66	320	Parker	1997	USA	INJ	ACC	0.957	0.267	30.302
66	321	Parker	1997	USA	INJ	ACC	0.988	0.881	10.553
66	322	Parker	1997	USA	INJ	ACC	0.98	2.291	-41.758
66	323	Parker	1997	USA	INJ	ACC	0.982	1.698	-29.09
66	324	Parker	1997	USA	INJ	ACC	0.957	2.248	-18.34
66	325	Parker	1997	USA	INJ	ACC	0.966	0.808	6.174
66	326	Parker	1997	USA	INJ	ACC	0.997	0.464	240.063
66	327	Parker	1997	USA	INJ	ACC	0.989	0.356	89.439
66	328	Parker	1997	USA	INJ	ACC	1.011	1.357	27.864
66	329	Parker	1997	USA	INJ	ACC	0.899	0.404	8.479
66	330	Parker	1997	USA	INJ	ACC	1.028	0.379	-34.739
66	331	Parker	1997	USA	INJ	ACC	1.017	0.602	-30.131
66	332	Parker	1997	USA	INJ	ACC	0.998	4.735	-639.916
66	333	Parker	1997	USA	INJ	ACC	1.005	0.979	-4.624
66	334	Parker	1997	USA	INJ	ACC	1.02	1.119	5.773
66	335	Parker	1997	USA	INJ	ACC	1.009	0.585	-58.229
66	336	Parker	1997	USA	INJ	ACC	1.002	0.719	-186.619
66	337	Parker	1997	USA	INJ	ACC	1.035	0.602	-14.792
66	338	Parker	1997	USA	INJ	ACC	1.054	1.133	2.387
66	339	Parker	1997	USA	INJ	ACC	1.03	0.309	-39.979
66	340	Parker	1997	USA	INJ	ACC	0.967	2.547	-28.071

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67	341	Liu and Popoff	1997	CDN	INJ	VIC			13.824
68	342	Antov and Roivas	1999	EST	FAT	VIC	1.044	0.849	-3.807
68	343	Antov and Roivas	1999	EST	INJ	VIC	1.044	0.701	-8.28
69	344	Aljanahi, Rhodes and Metcalfe	1999	BHR	INJ	ACC			2.609
70	345	Wheeler,Taylor	1999	GBR	INJ	ACC	0.931	0.926	1.071
70	346	Wheeler,Taylor	1999	GBR	INJ	ACC	0.792	0.483	3.112
70	347	Wheeler,Taylor	1999	GBR	INJ	ACC	0.92	0.815	2.462
70	348	Wheeler,Taylor	1999	GBR	INJ	ACC	0.847	0.333	6.616
70	349	Wheeler,Taylor	1999	GBR	INJ	ACC	0.797	1.136	-0.562
70	350	Wheeler,Taylor	1999	GBR	INJ	ACC	0.892	1.059	-0.498
71	351	Eriksson and Àgùstsson	1999	DEN	INJ	ACC	0.783	0.504	2.803
72	352	Lamm, Psarianos, Mailaender	1999	USA	FAT	ACC	0.751	0.137	6.945
72	353	Lamm, Psarianos, Mailaender	1999	USA	INJ	ACC	0.751	0.128	7.198
72	354	Lamm, Psarianos, Mailaender	1999	USA	PDO	ACC	0.751	0.169	6.221
73	355	Buss	1999	GER	PDO	ACC	1.211	2.064	3.789
74	356	Peltola	2000	FIN	INJ	ACC	0.956	0.776	5.686
74	357	Peltola	2000	FIN	INJ	ACC	0.956	0.685	8.494
74	358	Peltola	2000	FIN	FAT	VIC	0.956	0.406	20.234
74	359	Peltola	2000	FIN	FAT	VIC	0.956	0.574	12.429
75	360	Andersson	2000	SWE	INJ	ACC	0.923	0.681	4.797
75	361	Andersson	2000	SWE	INJ	VIC	0.923	0.839	2.194
75	362	Andersson	2000	SWE	INJ	ACC	0.946	1.751	-10.034
75	363	Andersson	2000	SWE	INJ	VIC	0.946	1.875	-11.264
76	364	Wretling	2000	SWE	FAT	VIC	0.938	1.571	-7.086
76	365	Wretling	2000	SWE	SER	VIC	0.938	0.381	15.144
76	366	Wretling	2000	SWE	SLI	VIC	0.938	1.01	-0.153
76	367	Wretling	2000	SWE	PDO	ACC	0.938	1.147	-2.157
77	368	Kronberg, Nilsson	2000	SWE	FAT	VIC	0.939	0.226	23.598
77	369	Kronberg, Nilsson	2000	SWE	SER	VIC	0.939	0.524	10.254
77	370	Kronberg, Nilsson	2000	SWE	SLI	VIC	0.939	0.327	17.719
78	371	Burns, Johnstone, Macdonald	2001	GBR	SLI	ACC	0.947	0.624	8.652
78	372	Burns, Johnstone, Macdonald	2001	GBR	SER	ACC	0.947	0.413	16.236
79	373	Abel, Matthes	2001	GER	INJ	ACC	0.84	0.533	3.596
79	374	Abel, Matthes	2001	GER	PDO	ACC	0.84	0.222	8.603
79	375	Abel, Matthes	2001	GER	SLI	ACC	0.608	0.97	0.062
79	376	Abel, Matthes	2001	GER	SER	ACC	0.608	0.815	0.411
79	377	Abel, Matthes	2001	GER	PDO	ACC	0.608	1.202	-0.369
79	378	Abel, Matthes	2001	GER	SLI	ACC	0.75	0.974	0.09
79	379	Abel, Matthes	2001	GER	SER	ACC	0.75	0.914	0.311
79	380	Abel, Matthes	2001	GER	FAT	ACC	0.75	0.333	3.819
79	381	Abel, Matthes	2001	GER	PDO	ACC	0.75	1.281	-0.86
79	382	Abel, Matthes	2001	GER	SLI	ACC	0.92	0.841	2.09
79	383	Abel, Matthes	2001	GER	SER	ACC	0.92	0.75	3.472
79	384	Abel, Matthes	2001	GER	FAT	ACC	0.92	0.071	31.85
79	385	Abel, Matthes	2001	GER	PDO	ACC	0.92	0.785	2.929
79	386	Abel, Matthes	2001	GER	SLI	ACC	0.513	0.599	0.767
79	387	Abel, Matthes	2001	GER	SER	ACC	0.513	0.549	0.898
79	388	Abel, Matthes	2001	GER	FAT	ACC	0.513	0.429	1.269
79	389	Abel, Matthes	2001	GER	PDO	ACC	0.513	0.994	0.009
80	390	Keall, Povey and Frith	2001	NZL	INJ	ACC	0.969	0.876	4.249
80	391	Keall, Povey and Frith	2001	NZL	INJ	VIC	0.969	0.877	4.236

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81	392	Taylor, Baruya and Kennedy	2002	GBR	INJ	ACC			2.479
82	393	Ossiander,Cummings	2002	USA	FAT	ACC	1.095	2.1	8.21
83	394	Grendstad	2003	NOR	INJ	ACC	0.952	1.763	-11.622
83	395	Grendstad	2003	NOR	INJ	ACC	0.875	0.79	1.765
83	396	Grendstad	2003	NOR	INJ	ACC	0.784	0.227	6.086
83	397	Grendstad	2003	NOR	INJ	ACC	0.851	0.989	0.069
83	398	Grendstad	2003	NOR	INJ	ACC	0.824	0.538	3.209
83	399	Grendstad	2003	NOR	INJ	ACC	1.047	3.936	29.87
84	400	Andersson	2003	SWE	FAT	ACC	0.962	0.417	22.447
84	401	Andersson	2003	SWE	SER	ACC	0.962	0.848	4.221
84	402	Andersson	2003	SWE	SLI	ACC	0.962	0.998	0.048
84	403	Andersson	2003	SWE	FAT	VIC	0.962	0.417	22.447
84	404	Andersson	2003	SWE	SER	VIC	0.962	0.951	1.288
84	405	Andersson	2003	SWE	SLI	VIC	0.962	1.146	-3.498
85	406	Farmer et al	1999	USA	FAT	VIC	1.048	1.166	3.289
86	407	Pez	2002	GER	FAT	VIC	0.988	0.694	29.088
86	408	Pez	2002	GER	SER	VIC	0.988	0.805	17.306
86	409	Pez	2002	GER	SLI	VIC	0.988	0.975	2.048
86	410	Pez	2002	GER	PDO	ACC	0.988	0.947	4.305
87	411	Varhelyi et al	2003	SWE	PDO	ACC	0.937	0.804	3.371
88	412	Rutley	1972	GBR	INJ	ACC	0.942	0.804	3.642
88	413	Rutley	1972	GBR	INJ	ACC	0.938	0.633	7.207
88	414	Rutley	1972	GBR	INJ	ACC	1.032	0.762	-8.675
88	415	Rutley	1972	GBR	INJ	ACC	0.984	0.854	9.559
88	416	Rutley	1972	GBR	INJ	ACC	1.019	0.759	-14.642
88	417	Rutley	1972	GBR	INJ	ACC	1.094	0.67	-4.474
88	418	Rutley	1972	GBR	INJ	ACC	1.087	0.446	-9.648
89	419	Nilsson	1992	SWE	INJ	ACC	0.959	1.037	-0.884
89	420	Nilsson	1992	SWE	INJ	ACC	0.959	0.884	2.962
89	421	Nilsson	1992	SWE	INJ	ACC	0.931	0.865	2.034
89	422	Nilsson	1992	SWE	INJ	ACC	0.931	0.902	1.445
90	423	Andersson	2000	SWE	FAT	VIC	0.985	0.641	30.273
90	424	Andersson	2000	SWE	SER	VIC	0.985	0.956	3.04
91	425	Agustsson	2001	DEN	INJ	VIC	0.766	0.196	6.108
92	426	Myrup,Agustsson	2003	DEN	INJ	ACC	0.965	1.019	-0.53
92	427	Myrup,Agustsson	2003	DEN	INJ	ACC	1.002	0.939	-31.205
92	428	Myrup,Agustsson	2003	DEN	INJ	ACC	0.946	0.73	5.628
92	429	Myrup,Agustsson	2003	DEN	INJ	ACC	0.976	0.767	10.92
92	430	Myrup,Agustsson	2003	DEN	INJ	ACC	0.969	1.044	-1.374
92	431	Myrup,Agustsson	2003	DEN	INJ	ACC	0.964	0.906	2.674
93	432	Goldenbeld et al	2003	NED	INJ	ACC	0.974	0.793	8.84
94	433	Richter et al	2004	ISR	FAT	VIC	1.07	1.385	4.839
95	434	Stuster	2004	USA	INJ	ACC	0.986	1.588	-33.738
95	435	Stuster	2004	USA	INJ	ACC	0.976	0.945	2.342
95	436	Stuster	2004	USA	INJ	ACC	1.012	0.856	-12.715
95	437	Stuster	2004	USA	PDO	ACC	0.986	1.251	-16.365
95	438	Stuster	2004	USA	PDO	ACC	0.976	0.754	11.656
95	439	Stuster	2004	USA	PDO	ACC	1.012	1.164	12.456

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Study rec no	Result rec no	Authors	Publ year	Data country	Acc/inj severity	Accs or victims	Speed change	Acc/Vic change	Est of power
95	440	Stuster	2004	USA	INJ	ACC	0.914	1.003	-0.03
95	441	Stuster	2004	USA	INJ	ACC	0.969	1.316	-8.581
95	442	Stuster	2004	USA	INJ	ACC	1.011	1.316	24.265
95	443	Stuster	2004	USA	INJ	ACC	0.985	1.121	-7.735
95	444	Stuster	2004	USA	PDO	ACC	0.914	0.995	0.055
95	445	Stuster	2004	USA	PDO	ACC	0.969	1.122	-3.59
95	446	Stuster	2004	USA	PDO	ACC	1.011	1.19	15.349
95	447	Stuster	2004	USA	PDO	ACC	0.985	1.275	-16.427
96	448	Vernon et al	2004	USA	FAT	ACC	1.051	1.01	0.211
96	449	Vernon et al	2004	USA	INJ	ACC	1.051	1.067	1.31
96	450	Vernon et al	2004	USA	PDO	ACC	1.051	0.926	-1.552
97	451	Ragnøy	2004	NOR	FAT	VIC	0.946	0.713	6.053
97	452	Ragnøy	2004	NOR	SER	VIC	0.946	0.716	5.962
97	453	Ragnøy	2004	NOR	SLI	VIC	0.946	0.89	2.086
97	454	Ragnøy	2004	NOR	INJ	ACC	0.946	0.817	3.6
98	455	Nilsson	2004	SWE	FAT	ACC			3.931
98	456	Nilsson	2004	SWE	SER	ACC			1.663
98	457	Nilsson	2004	SWE	SLI	ACC			1.168
98	458	Nilsson	2004	SWE	FAT	VIC			5.172
98	459	Nilsson	2004	SWE	SER	VIC			2.957
98	460	Nilsson	2004	SWE	SLI	VIC			1.355

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APPENDIX 2: RESIDUAL PLOTS

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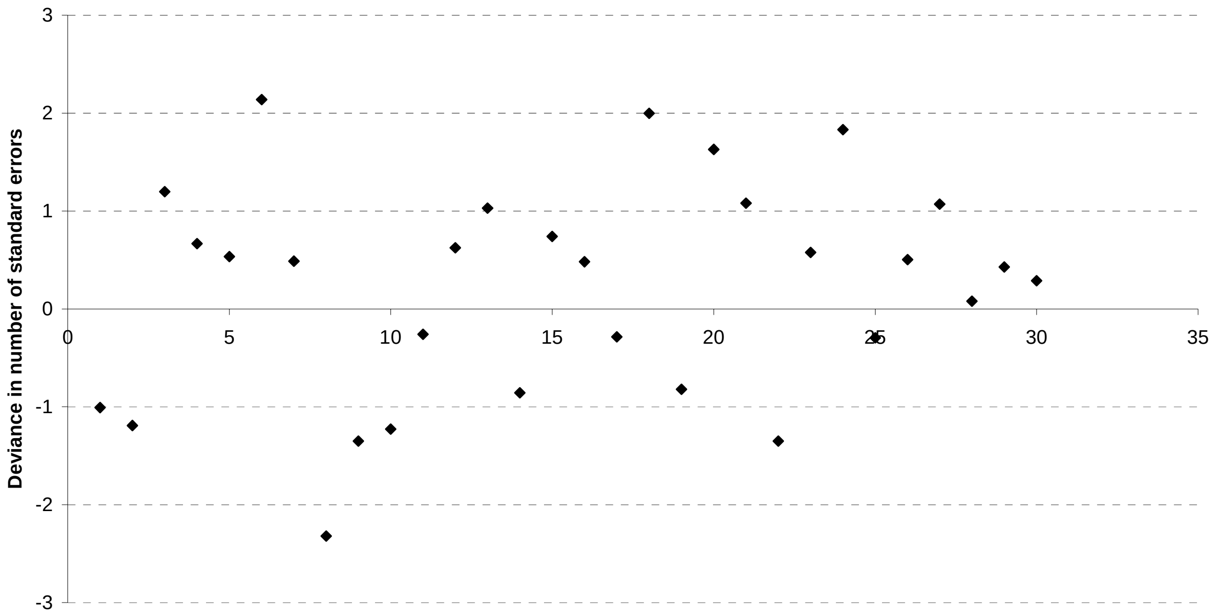
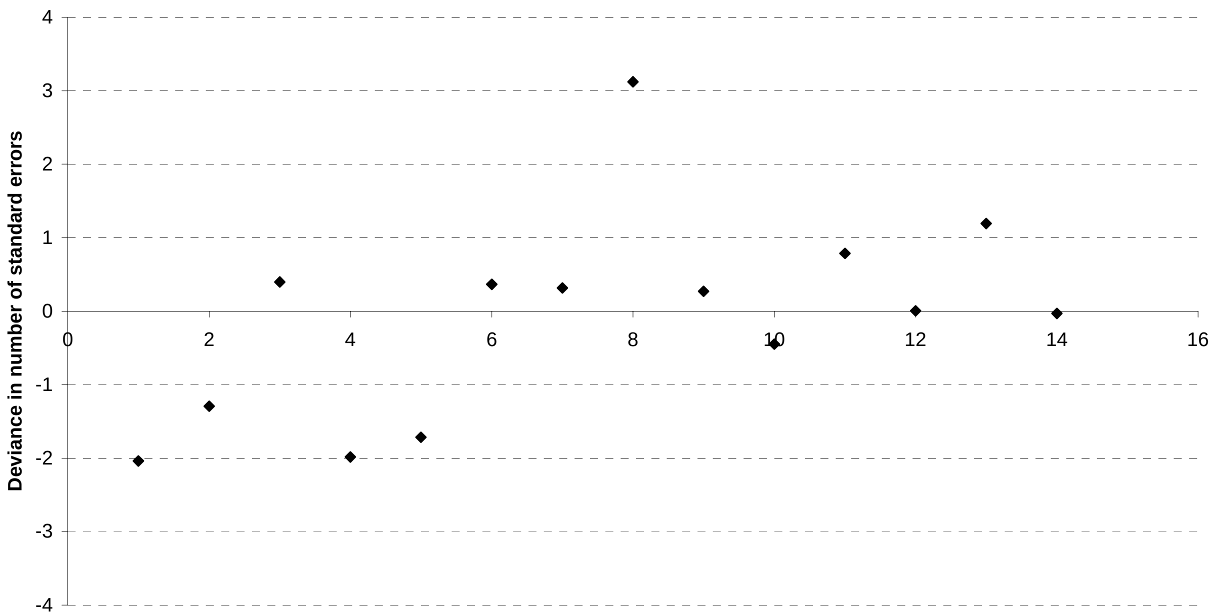
Residual plot for fatalities. Summary estimate of power = 4.5



Residual plot for serious injuries. Summary estimate of power = 3.0



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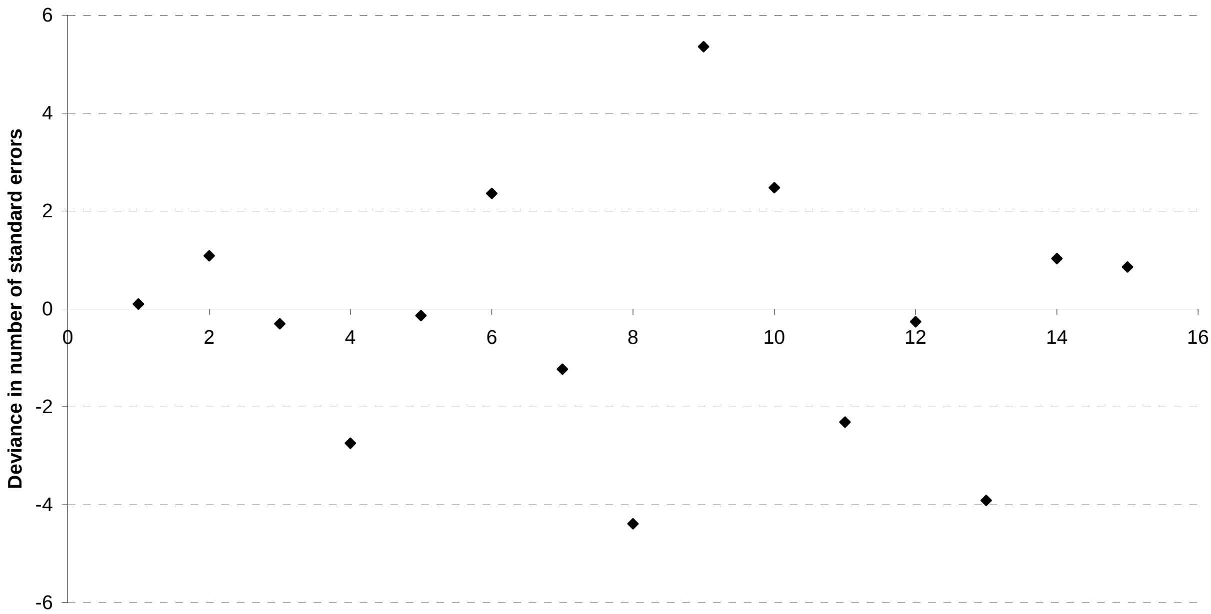
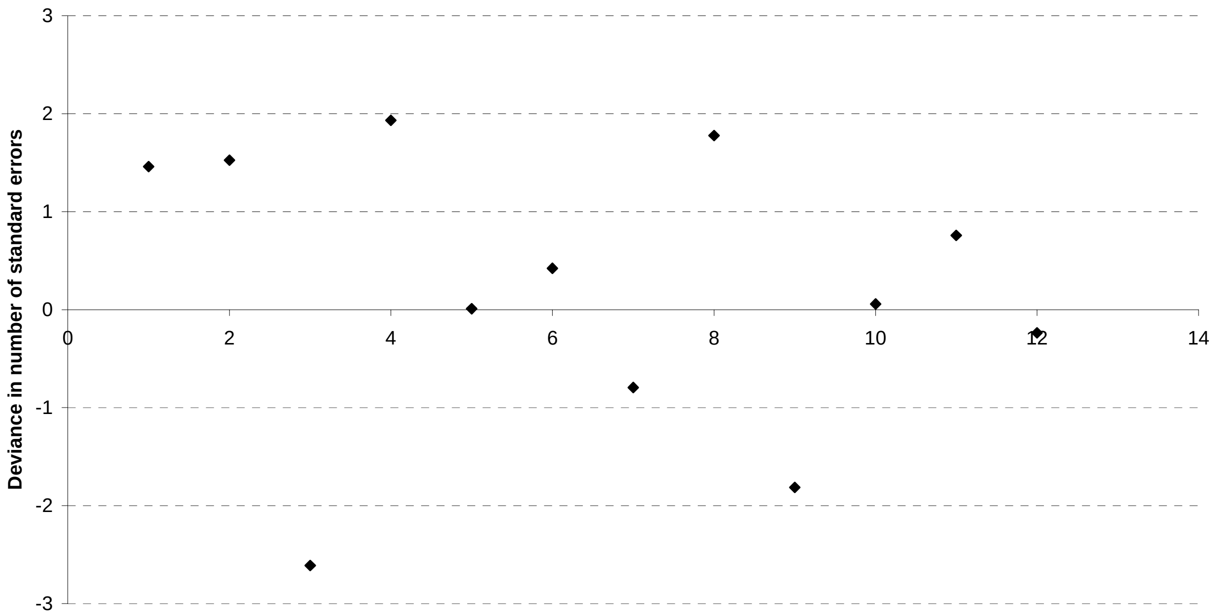
Residual plot for injured road users. Summary estimate of power = 2.7



Residual plot for slight injuries. Summary estimate of power = 1.5



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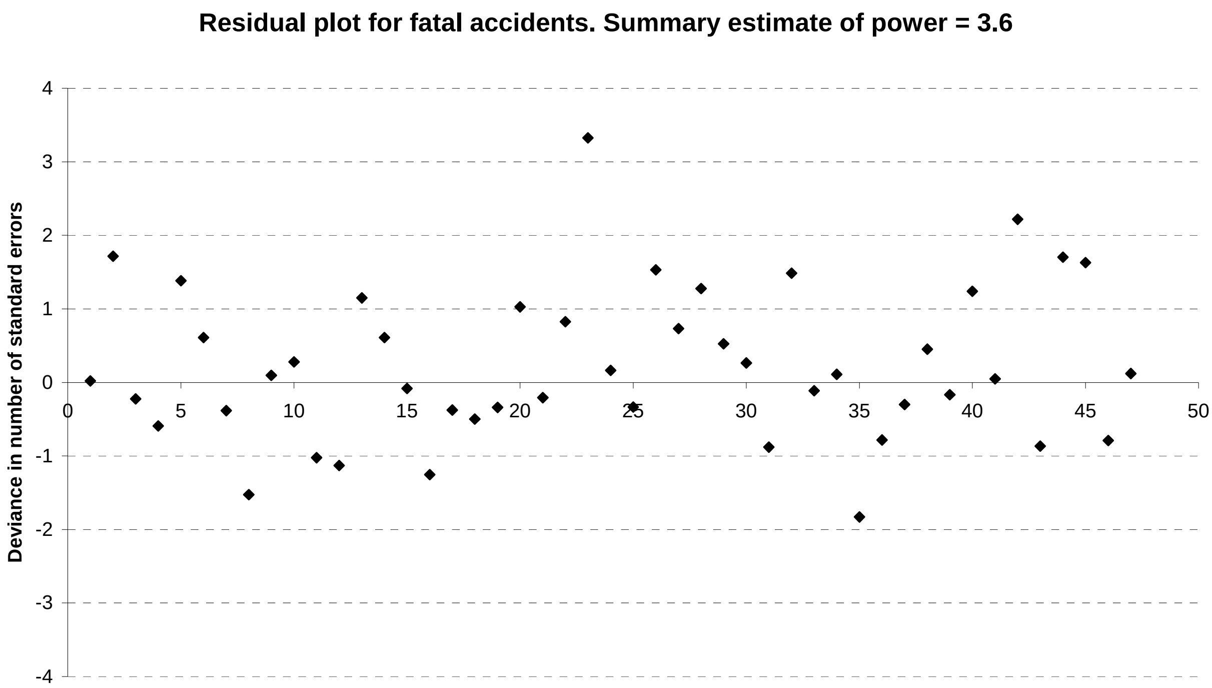

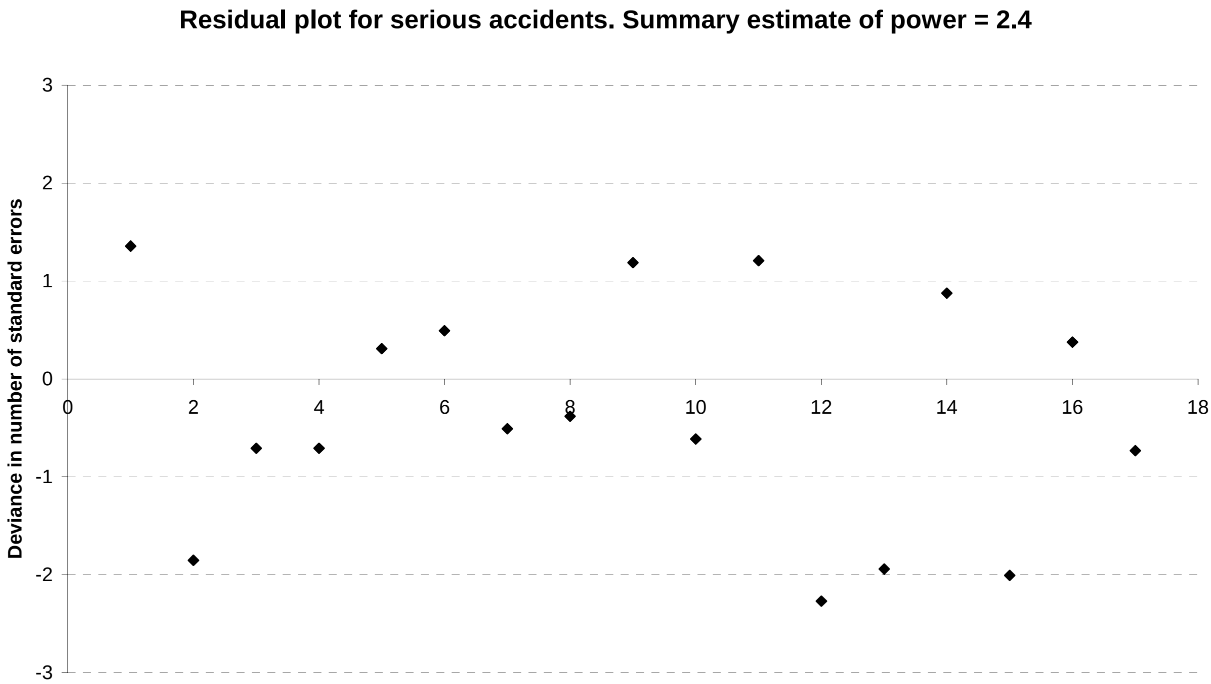

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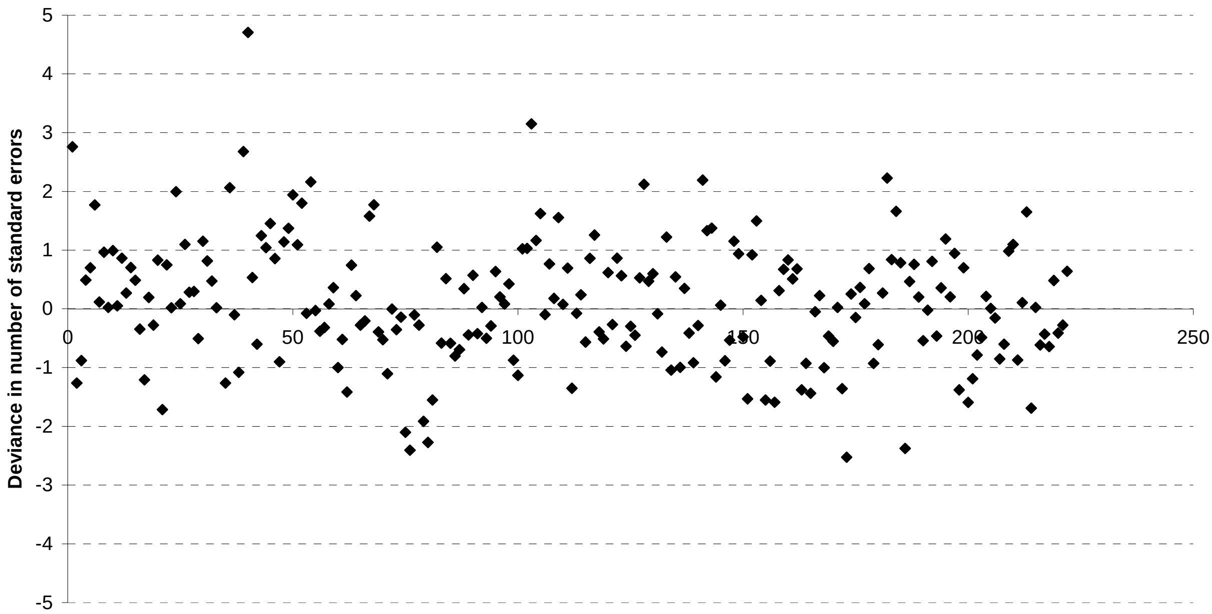
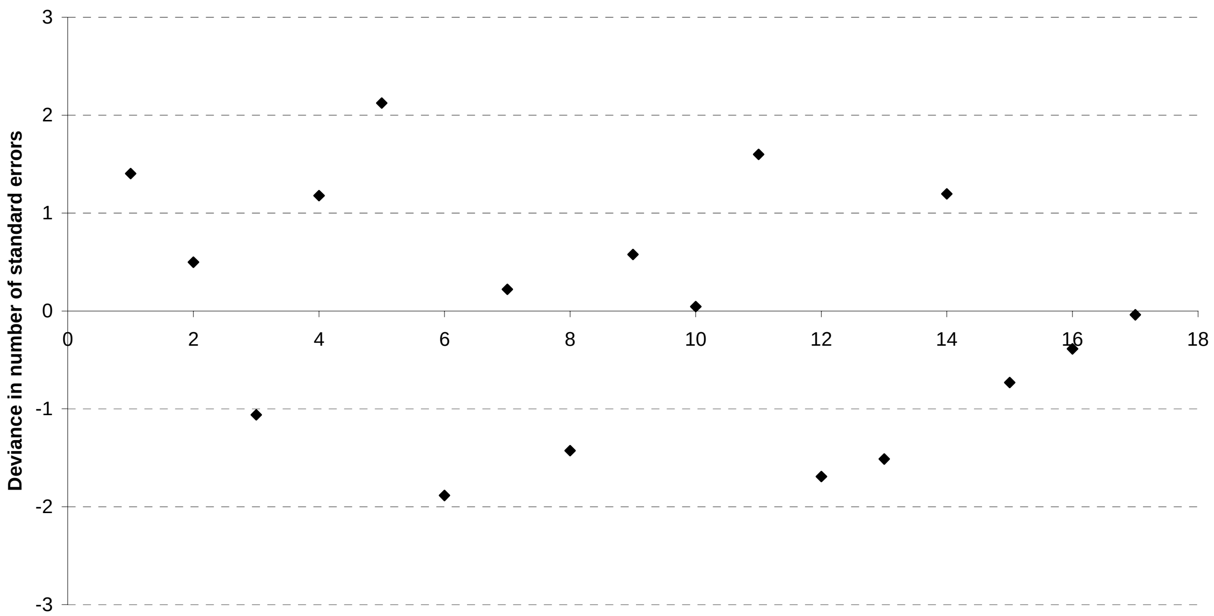
Residual plot for injury accidents. Summary estimate of power = 2.0



Residual plot for slight accidents. Summary estimate of power = 1.2



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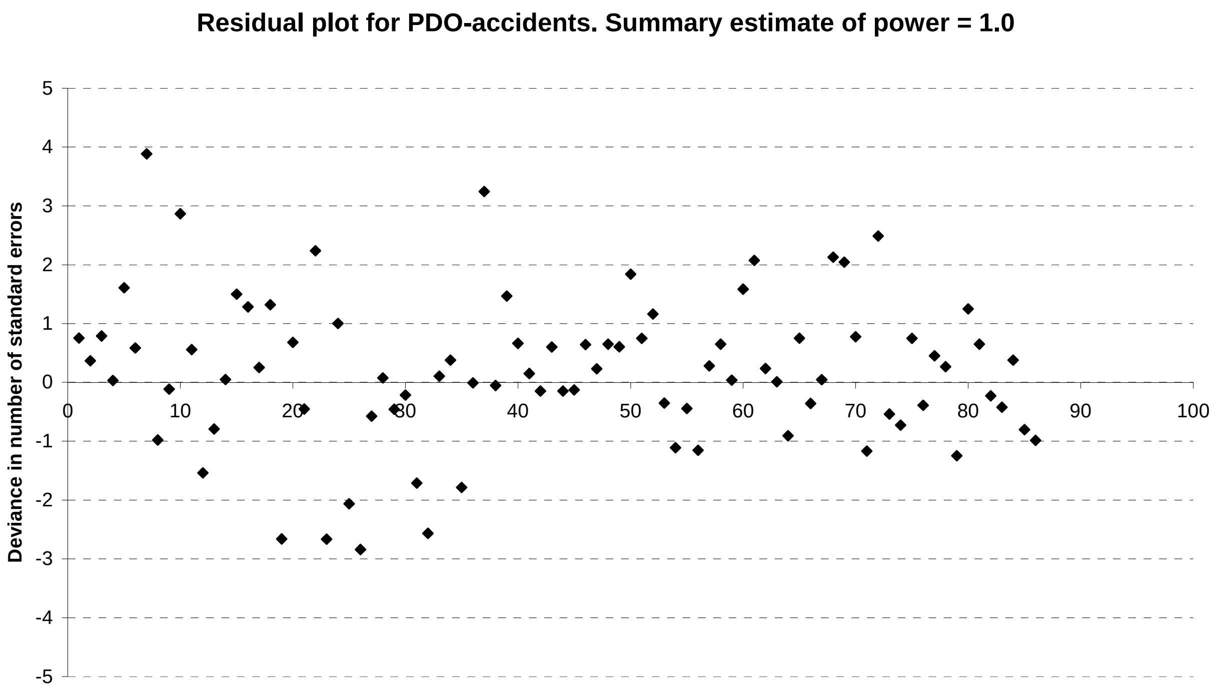

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