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Exposure rates or incidence rate, is the choice so obvious?

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For demographic analysis there is two kind of events:

- non repeatable events such as death, or events classified by order, first marriage, first birth, …,
- repeatable events such as births, marriages, divorces, …

Each of these categories are theoretically susceptible of one specific kind of analysis, probabilities or occurrence-exposure rates for non repeatable events and incidence rates for the repeatable ones.
Nevertheless, non repeatable events can be considered for analysis as if they were repeatable. It is why first marriages and births classified by birth order are often studied using incidence rates.
The choice of kind of rates generally depends on the available data. If for incidence rates only events classified by age of occurrence and population by sex and age are required, for probabilities an additional classification of population is needed, classification of population according to the status towards the phenomenon studied.
When analysis is done for cohorts the choice is neutral because the two kinds of analysis go, under some conditions, to same result.

Some demographers reject use of period *incidence rates* for *exposure rates*. But two different types of critics are addressed to incidence rates, depending on the type of analyse done, period analysis or cohort analysis:

- The first concerns period analysis. The distortion resulting in the transversal sum of incidence rates when cohorts are affected by changes in timing of the observed events[1].
- The later comes when, dealing with cohorts, researchers use the incidence rates of the last observation year in order to forecast the intensity of the phenomenon in some few cohorts whose all the events are not yet completely observed[2]. This concerns also mainly periods with timing changes.

This two critics do not aim at the core of the computation of incidence rates but only to some specific uses of these indexes. These rates give, indeed, a very robust measure, without bias, of cohort patterns.

I – Critics addressed to use of incidence rates in period analysis

What is under first critic is the meaning of sum of incidence rates on a period basis. Is the interpretation of a total period rate as a quantum index, as it is mainly done, really justified? For example, G. Calot[3] considered total period fertility rate not as a fertility measure but a replacement measure.
What is considered as crazy for some is the capability for total period rate to overestimate/underestimate real level of a phenomenon, when tempo is changing over

[1] For example, Toulemon, L., *Marriage intensity has to be computed from occurrence-exposures rates*, 24th Conference, UISSP, Bahia, 2001.
[2] For example, Sobotka, T., *Childless Societies? Trends and Projections of Childlessness in Europe and the United States*, PAA, Philadelphia, 2005.
[3] Calot, G., "Mais qu'est-ce donc qu'un indicateur conjoncturel de fécondité?", *Population*, n° 3, mai-juin 2001, p. 325-327

cohorts. Especially, for non repeatable events, the index ca go beyond one, that is considered as unacceptable by some demographers.
For others, at the opposite, this is an helping capacity to see that something is changing in cohorts, because all total period rates are mixed measure of cohort tempo and cohort quantum.

1) Availability of data
One of the first limitations in the choice of the index comes from the availability of the necessary data. As we saw previously, if for incidence rates only events classified by duration since the event origin and the population classified according to the same duration are required, for exposure rates (or probabilities) an additional classification of the population is needed, classification according to the status towards the phenomenon studied: already lived or not.
Even if, for taking first marriages as example, we need annual population classified, by marital status, sex and age. But this tabulation is less frequent than the simple population by age and sex. So even if number of national statistical institutes publish, on a regular basis, this table, it is not the case for every country. If we look at the Eurostat demographic database, we could see that single population classified by sex and age is available for 17 countries for year 2007, 18 for 2000 and only 7 for 1990 (Table 1), even if for some countries data could be available in national statistical institutes.

Table 1. European countries for which population by marital status are available on the Eurostat website

1990	2000	2007
Finland	Belgium	Belgium
France	Czech republic	Czech republic
Hungary	Denmark	Denmark
Iceland	Finland	Finland
Norway	France	Germany
Romania	Germany	Hungary
Sweden	Hungary	Iceland
	Iceland	Latvia
	Italy	Lithuania
	Liechtenstein	Liechtenstein
	Netherlands	Netherlands
	Norway	Norway
	Romania	Romania
	Slovakia	Slovakia
	Slovenia	Slovenia
	Sweden	Sweden
	Switzerland	Switzerland
	United kingdom	

So, for any of these years we have data for Albania, Bulgaria, Croatia, Ireland, Luxembourg, Poland or Portugal. Even if, these countries could be omitted, this is a severe restriction when you are interested in comparative studies.

2) Quality of data on specific populations
When the necessary data are available a double interrogation arises:

- What is the completeness of census? Particularly, is there an under coverage of single, especially young people?
- What is the quality of the distribution by marital status produced by the statistical office? This question could seem strange nowdays, nevertheless few decades ago in western Europe, quality of answer to the census question about the marital status of interviewed persons was not so good. Indeed, at that time, people living in couple and single mothers living alone with children were often tempted to declare themselves in

an other marital status. In the same way, divorced people declared themselves as married or single instead of divorced because divorced people were a little bit rejected by society.

Wrong declarations could introduce bias in the measure of marriage of single, only if, at each age, the number of persons who declare wrongfully themselves as *single* is not equal to number of single who declare themselves as an other status. Indeed, difference between this two numbers affects the size of population at risk[4].

In an article on approximations and errors in computing cohort marriage tables Louis Henry, the most important French demographer wrote as conclusion: "as it is not possible to appropriately correct numbers of singles at young ages, we use age-specific first marriage incidence rates; the easiest way is to distribute the total number of marriages from the table, which is equal to complement to one of the definitive celibacy frequency which was just computed, as it is for the sum of incidence rates. It is fruitless to research a better approximation as long as census errors do exist[5]."

Even if this analysis was done forty-five years ago, is pertinence is still valuable. Indeed quality of census did not improve very much during last decades, and completeness could be worst now. European society could be nowdays more tolerant to what was considered yesterday as a deviant behaviour, but for comparative studies across Europe we need to be sure of the quality of data we are using. So, as a precaution principle, we think that use of incidence rates is preferable.

From some events there is no choice. For example, for analysis of fertility by birth order, national statistical institutes do not compute every year female populations classified by age and number of previous born children. So, only family surveys can give opportunity to compute exposure rates. An other possibility is to use the now quite common information coming from censuses: classification of women according to age and number of ever-born live children, and to update this information by cohorts with incidence rates. By this way, it is possible to convert incidence rates into exposure rates.

The problem with this procedure is: the new repartition by age and parity, given by the next census, will differ, quite certainly, from that coming from the previous census and annually updated. The discrepancy come essentially from migration, if there is no selective under registration according to the number of children ever-born. The implicit hypothesis of same fertility for migrants as for native people, which is classic, is not very realistic.

For divorce the problem is worst; we do not have surviving marriages at each duration by marriage cohort, only the original number of marriages of the cohort is available, which is considered as the population at risk. So the computed rates are *net* incidence rates, because they include effects of mortality and migration.

3) Problems in interpretation

It seems that critics addressed to use of incidence rates are due mainly to the interpretation generally done to the cross sectional synthesis of age specific rates. Indeed, this synthesis is most often presented as the proportion of the people who would experience the studied phenomenon in a hypothetical, or synthetic, cohort of persons who, at each age, experienced the relevant age-specific incidence rates applying in the year in question. Clearly, the set of rates may not apply to any real people. Nevertheless, this indicator can exceed the maximum

[4] For detailed analysis on the measurement problem caused by these uncertainty in census, see Henry L, 1963, "Approximations et erreurs dans les tables de nuptialité de générations", *Population*, 1963, n°4, oct.-déc., p 737-776..

[5] "Comme on ne peut pas corriger convenablement les effectifs de célibataires aux jeunes âges, on a recours à la série des mariages réduits ; le plus simple est de répartir la somme des mariages de la table, égales au complément à un de la fréquence du célibat définitif qu'on vient de calculer, comme l'est la somme des mariages réduits. Il est vain de chercher une meilleure approximation tant qu'il subsiste des erreurs de recensement. »

possible. So, the total first marriage rate can exceed 1, i.e. more than a first marriage by never married person in the hypothetical cohort. Because of this possibility, which express the sensitivity to changes in the timing of the phenomenon over cohorts (due to the fact that incidence rates do not take into account the previous experience of the population[6]), some demographers reject this indicator.

If we move to fertility, as example, we will see more easily that this rejection, even if it is rare for fertility, come from the misinterpretation of this indicator. Generally, as for other phenomenon, TPFR (total period fertility rate) is interpreted using the model of the hypothetical cohort. Nevertheless, it is not the only way of interpreting TFR. One other interpretation corresponding exactly to the nature of the computation done for obtaining it, is to say that it is only a standardisation. TFR is a standardised fertility index, using as standard population a female population where, at each age, number of women will be equal to 1000. This interpretation is the origin of the French designation of induced rates as *reduced events*, reduction to a standard structure. And this standardisation can only be interpreted as an hypothetical cohort, in a very specific situation: when quantum and tempo of the phenomenon is stable over cohorts.

A more useful interpretation of what is exactly TFR was given by Gérard Calot[7]: TFR is the fraction of the absolute number of live births by the average size of female birth cohorts (mean cohort of generations subject to the risk of fertility). This mean cohort is the weighted average size of the different female generations which, in that year, are in fertile ages, whereby the weighting coefficient of the cohort at a given age is the fertility rate at that age.

Despite the unit used to express it: *number of live births by woman*, i.e. the same unit as for cohort *completed fertility*, TFR is not a measure of fertility level, but a measure of replacement level.

Indeed, for example, a TFR equal to 1.4 birth by woman, i.e. one third under the replacement threshold (2.1 births by woman), means that the number of female live births during year *y* is lower by one third to the mean size of female population of fertile age during the same year. In an other way, we can say that fraction between TFR and 2.1 is also fraction, for female population, between size of cohort born during the year to mean size of young adults cohorts (around age 30, i.e. period mean age at birth).

Nevertheless, it is an imperfect measure of replacement because it compares two sizes of population at different ages: size *at birth* of the cohort born year *y*, size *around age 30* for cohort of young adults.

Application to mortality

Fifteen years ago, looking for the best way to show limits and even misleading of the interpretation of a total period rate as a quantum index by using the concept of hypothetical cohort, I thought that I should find an irrefutable example to show that this interpretation can be absurd, when the very specific conditions are not realised. This example does exist with mortality whose intensity is always equal to one. So, if the total period mortality rate differs from one, nobody would explain that quantum of mortality is changing[8].

It is because of this unavoidable character of death that there is not any index of mortality quantum, but only tempo indices. By transposing to mortality indices used for non unavoidable phenomena, I wondered what could be the level of a period quantum measure of mortality, knowing that cohort quantum is equal to one. This question could sound strange

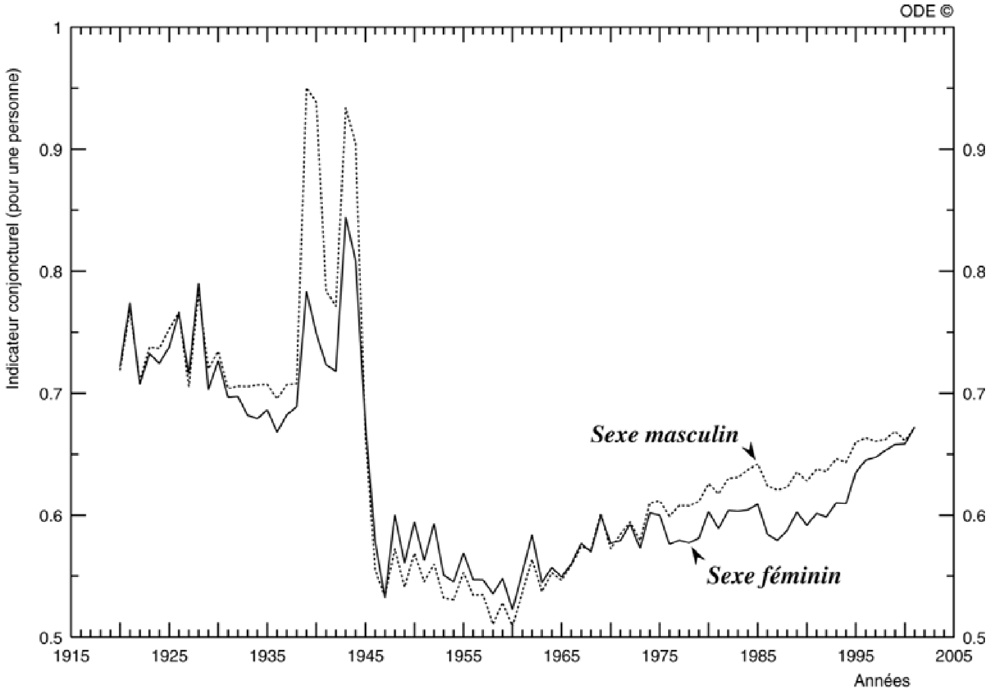
[6] C.f. Péron Y., « Les indices du moment de la nuptialité des célibataires », *Population*, 6, 1991, p. 1429-1440.

[7] C.f. Calot G., « Mais qu'est-ce donc qu'un indicateur conjoncturel de fécondité ? », *Population*, 3, 2001, p. 325-327.

[8] Sardon J.-P., « A period measure of mortality. The example of France », *Population : An english selection*, 6, 1994, p. 131-150.

because, with the classical period life table, number of deaths equal the radix of the table. But what could be the result when using incidence rates, as for first marriages with denominator defined as all the people having, or not, already lived the studied event; i.e. for mortality, survivors and persons, of the same birth cohort, already dead?

Figure 1. France, 1920-2001. Total period mortality rate, by sex



The level of this index, always under one, prevents us to consider it, as we could be tempted for first marriages, as a period quantum measure, as the value that cohorts would observe if incidence mortality rates would not change for a long time.
This total period mortality rate, despite its odd values which might lead us to reject it instinctively, contains information which is simple to display and may prove useful for analysis.
Indeed, the well-known relationship between cohort and period demographic indicators are simplified in the case of mortality, because quantum is constant across cohorts. So what this indicator displays is only tempo variations over birth cohorts.
Thus, when TPMR underestimates the cohort quantum by *x* %, the mean age at dying is rising over cohorts by *x* hundredths of a year. So, from the trend of TPMR between the two world wars, we can conclude that, in France, life expectancy at birth was increasing by 30 hundredths of a year by birth cohort, since TPMR was underestimating by around 30% mortality quantum.
Until the beginning of 1960s, the increasing underestimation of cohort quantum reflected the growing increase of expectation of life at birth. Afterwards, its trend shows a slowing down of increase in life expectancy along cohorts.

This period measure of mortality which is much more responsive to variation in the age distribution of deaths correspond exactly to what Roland Pressat[9] has called the “sum of tempo elements”, which he was using for dividing TFR in order to point out what could be TFR without changes in cohort tempo. This was rediscovered ten years later as the Timing Index[10] used to compute average cohort fertility (ACP).

[9] Pressat R., 1969, L’analyse démographique : concepts, méthodes, résultats, PUF, 323 p. (p. 122-124).
[10] Developed independently by Butz and Ward (1979) and Ryder (1980)

4) Conversion between two kinds of rates

It is always easier to compute incidence rates. Nevertheless, if for a precise purpose it is nevessary to get exposure rates or to construct tables, it is possible to derive occurrence-exposure rates from these incidence rates.

If we compute, according to the most common formula, the incidence rate as:

$$p_i^n = \frac{E_i^n}{\left(P_i^n + P_i^{n+1}\right)/2}$$

The exposure rate is computed by the same formula except that the denominator is limited to population at risk. If E is the number of first births and the denominator C = number of childless women, instead of P = number of women irrespective of their previous births:

$$\pi_i^n = \frac{E_i^n}{\left(C_i^n + C_i^{n+1}\right)/2}$$

It is useful to be able to convert – both ways- cohort rates of one kind into corresponding rates of the other kind. Consider therefore a non-repeatable event like first birth, and let p_i^n be its incidence rate and π_i^n the exposure rate, both observed at age i during year n.. Suppose that the population is closed and that there is no death and no migration in the age interval [α,ω], α being the minimum age at birth and ω being age 50 as before.

The proportion of "never mother" at the beginning of year n among women at completed age i-1 can be computed in two equivalent ways, and we get

$$1 - \sum_{j=\alpha}^{i-1} p_j^{n-i+j} = \prod_{j=\alpha}^{i-1}\left(1 - \pi_j^{n-i+j}\right) \quad (1)$$

If we write down the same relation for a year later and subtract the latter from (1) we obtain the following formula to convert π rates into p rates:

$$p_i^n = \prod_{j=\alpha}^{i-1}\left[\pi_i^n\left(1 - \pi_j^{n-1+j}\right)\right] \qquad (2)$$

If we divide the initial identity for year n+1 by that for year n and reorganize, we get, conversely, the following formula to transform p rates into π rates[11]:

$$\pi_i^n = \frac{p_i^n}{1 - \sum_{j=\alpha}^{i-1} p_j^{n-i+j}} \qquad (3)$$

This formula can be used for all events except for unrepeatable events for which the order is a determinant to define the population at risk. It is why, for birth orders higher than 1, this formula must be adapted:

$$\pi_{k,i}^n = \frac{p_{k,i}^n}{\sum_{j=\alpha}^{i-1} p_{k-1,j}^{n-i+j} - \sum_{j=\alpha}^{i-1} p_{k,j}^{n-i+j}} \qquad (4)$$

where k is the birth order and the population at risk of a k+1 birth is:

[11] Shryock H. S. and Siegel J. S., 1971, *The methods and materials of demography*, US Department of commerce, Bureau of the census, 2 vol.

$$\sum_{j=\alpha}^{i-1} p_{k-1,j}^{n-i+j} - \sum_{j=\alpha}^{i-1} p_{k,j}^{n-i+j} \qquad (5)$$

Because of the computing of the proportion women by parity through cohort, the computation of rates of the first kind is only possible from the eldest cohort under observation from age 15.

5) Exposure rates are they free of tempo effects?

Since several year, there is debate about cross sectional synthesis, and how to get indices free of tempo effects, in order to measure only period quantum. This debate reinitiated by John Bongaarts and Griffith Feeney[12] has shown that tempo distortions are not only present in *quantum* measures based on the incidence rates synthesis but also in period *tempo* measures based on exposure tables, such as life expectancy[13].

Tempo effects result from an increase or a decrease of the numbers of events which affects numerators of rates. It is why it concerns every type of rates, occurrence rates as well incidence rates, because in both case numerators are the same. Nevertheless, tempo distortions are much weaker for occurrence rates and less noticeable because, by the multiplicative way of computation, it is not possible to get quantum measures which exceed one. It is why, for a long time, these tempo effects on occurrence synthesis were not considered.

Now, these tempo distortions in period quantum measures using occurrence rates are well established and accepted for fertility and first marriages. For mortality, the problem is a little bit different because even if mortality rates (or probabilities) are also affected by tempo effects, its does not prevent to gent an intensity equal to one.

If we examine now tempo effects on tempo measures, we know, as result of the research done by John Bongaarts and Griffith Feeney[14], that if mean age at event computed from incidence rates is not affected by tempo distortions (because distortion applies both on numerator and denominator of the fraction), it is not the case for exposure/occurrence rates. It is why, even a so popular index as expectancy at birth, is also effected by tempo distortions, and could excess the real mean age at death by something between one and two years.

So, if we decide to use occurrence rates, instead of incidence rates, in order to measure period quantum of a phenomenon and even if this kind of rates is less sensitive to change in the timing of cohorts, this quantum measure will continue to be affected by tempo distortion, but of smaller size. More, the mean age will be affected. So if we are interested in trend in the mean age at event, we should compute also incidence rates in order to get mean ages without distortions.

II – Critics addressed to use of incidence rates for extrapolating cohort quantum

As we already know, use of incidences rates to describe quantum and tempo of any demographic event in cohorts give same results as use of exposure/occurrence rates, under some hypothesis. We will not discuss here if the underlying conditions of independence and continuity[15] are really realized, but a very specific use of incidence rates in estimation of the

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[12] Bongaarts J. and Feeney G., "On the Quantum and Tempo of Fertility", (1998), *Population and Development Review*, 24(2), 271-291.

[13] Bongaarts J. and Feeney G., "How long do we live?", (2002), *Population and Development Review*, 28(1), 13-29.

[14] Bongaarts J. and Feeney G., "The Quantum and Tempo of Life-Cycle Events", (2005), *Population Council*, Policy Research Division Working Papern°207, 52 p.
Reprint in *Vienna Yearbook of Population Research*, 2006, 115-151.

[15] Independence between level of the studied phenomenon and perturbing events (mortality and migration); continuity of risk of perturbing events before and after the realisation of the studied event.

total cohort completed rates for cohorts which have not yet reach the end of period during which the specific event could be lived.

Indeed, the second kind of critics addressed to incidence rates stress on the way commonly used to complete distribution of age/duration specific incidence rates of cohort which have not yet reach the end of the risk period. Most often, hypothesis done is to keep constant, for the coming years, the set of age-specific incidence rates computed for last observation year. If this method give a very good approximation of cohort quantum when timing of events is quite stable over cohorts, it is not the case when tempo distortions affect behaviour of cohorts contributing to period quantum, even though estimation is limited to cohort over a certain age (for example, 30 or 35 for fertility or first marriages), or to a defined amount of the cohort quantum (for example, 15% of the total cohort rate estimated).
So, when cohorts are anticipating the event this method overestimates cohort quantum and underestimates it when they are catching up some delay, as it is nowdays generally the case in all industrialised countries for first marriages and fertility.

Nevertheless, if this method is commonly used even when timing effects are not negligible, there are several others competing methods available, using occurrence-exposures rates, as proposed by T. Sobotka, as well as incidence rates.
The reason for choosing exposure rates instead of incidence rates hold to the fact that the former take into account the past trend of the studied phenomenon, so if less and less people have already lived the event, with a constant probability the number of events at a specific age will grow up from one cohort to the following one. If the incidence rate is kept constant the number of events will remain at the same level, since incidence rates are equivalent to the events of the table.

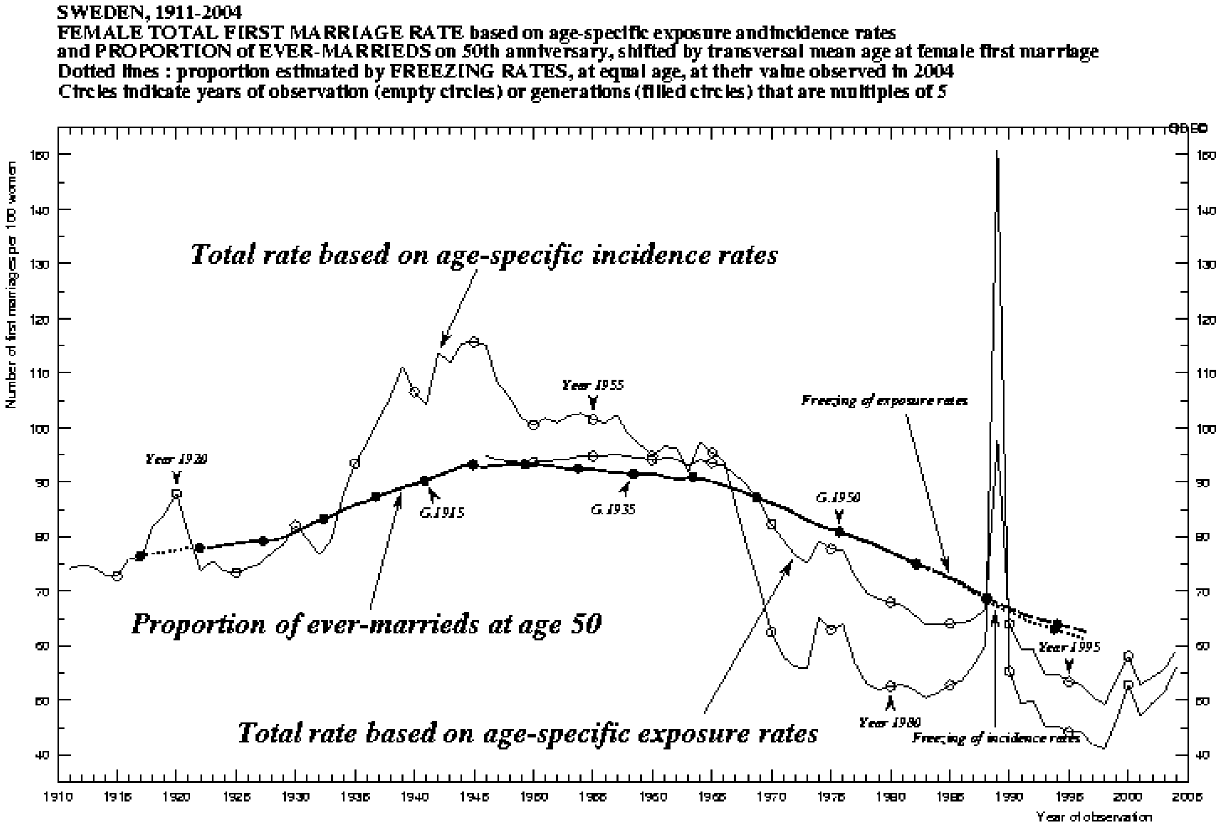
But the choice in front of us in such a situation is not necessary between the two kind of rate, but also the choice of the hypothesis done. When there is a catching up of events previously postponed, that is to say that number of events are increasing over time at older ages, it appears that, especially for incidence rates, the hypothesis done of constant rates is far from the most appropriate.
In such situation, it would be better to make a linear adjustment of the age specific rates of the last *x* years of observation. It could be also more realistic, in case of use of exposure rates to proceed also to a linear adjustment of rates over the last years.

Analysing fertility of first birth order Tomas Sobotka[16] wrote: "The poor performance of the period incidence rates in projecting childlessness is not due to their distortion by the postponement of childbearing, but also to the fact that they are related to all women in a given age and do not reflect the real exposure. Even if any "catching up effects" were absent and first probabilities therefore remained constant among women at later ages, the incidence rates would increase simply because of increasing number of childless women at these ages". The author give himself the answer and a simple look at the trend of incidence rates over time confirms: what is in question is not the use of incidence rates, but the way they are used.
In a period where incidence rates change rapidly from one year to the following one, rate freeze at the last observed value can only lead to underestimate cohort quantum.
Taking as example female first marriages in Sweden, are represented on **figure 2** period and cohort quantum based on the two kind of rates, incidence rates and exposure rates. Nevertheless, here exposure rates are not computed using single female population estimates, but are derived from incidence rates. As expected, variations around the cohort quantum are

[16] Sobotka T., *Childless Societies? Trends and Projections of Childlessness in Europe and the United States*, PAA, Philadelphia, 2005.

lesser for the exposure rates measure and if, for the past, the two cohorts quantum measures are equal, for cohorts, which are still bellow 50, estimation done using exposure rates freeze is a little bit higher than with incidence rates.

Figure 2. Sweden, Period and Cohort quantum based on each kind of rates



Looking at the age specific incidence rates shows the origin of the growing difference between the two kinds of estimates (**Figure 3**). In the recent period, that is to say, since the end of 1990s, there is a significant increase of incidence rates over 30. So, because this change will continue, first marriages will be concluded to older and older ages, when rates are frozen at the value of the last observation year this leads to an underestimation of proportion of ever-married at age 50.

In such a case it would be preferable to make a linear adjustment on incidence rates, the number of years used for the adjustment being defined by the form of the trend during recent period. Increase of incidence rates during recent period covers as well the increase in population at risk as changes in intensity at the considered age.

We applied this new method on the same set of data for Sweden. More precisely we used a linear adjustment on the last ten years of observation (1995-2004) to foresee values of incidence rates for the coming years. Estimation of cohort quantum done with this new hypothesis give results close to those obtained using exposure rates (**Figure 4**). Estimation of proportion of ever-married are even a little bit higher when a linear adjustment of incidence rates is used.

It appears so that if critiques formulated are well founded, they are not addressed to the right responsible. Indeed, instead of criticizing use of incidence rates, it should be more appropriate to limit critiques to the way of using, i.e. only to the freeze of rates, when trend is not to stability.

Figure 3. Sweden, 1911-2004. Female first marriage age specific incidence rates

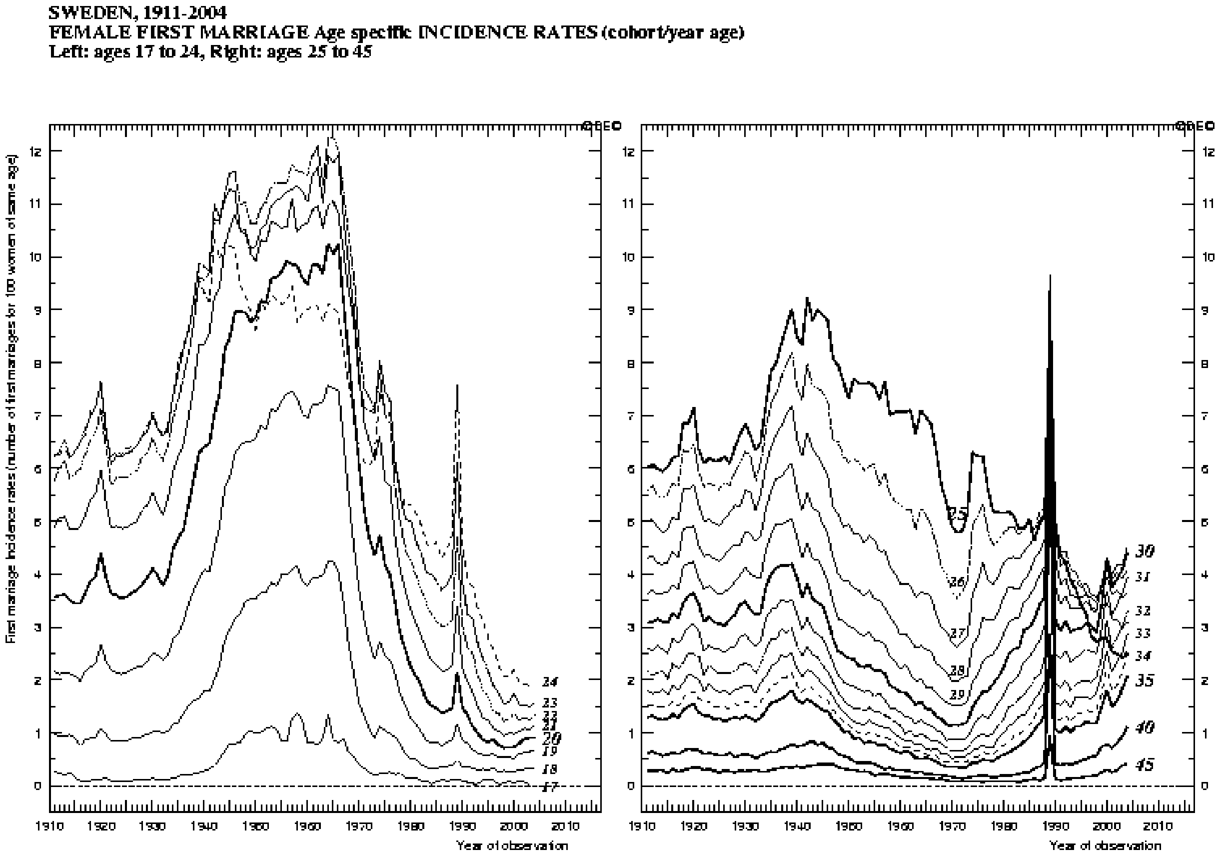
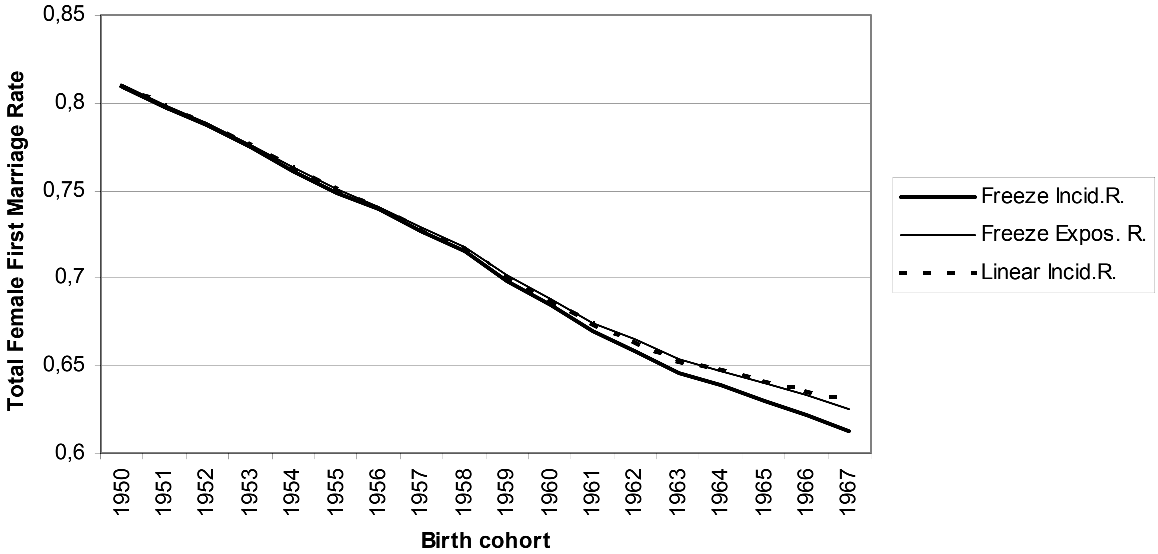
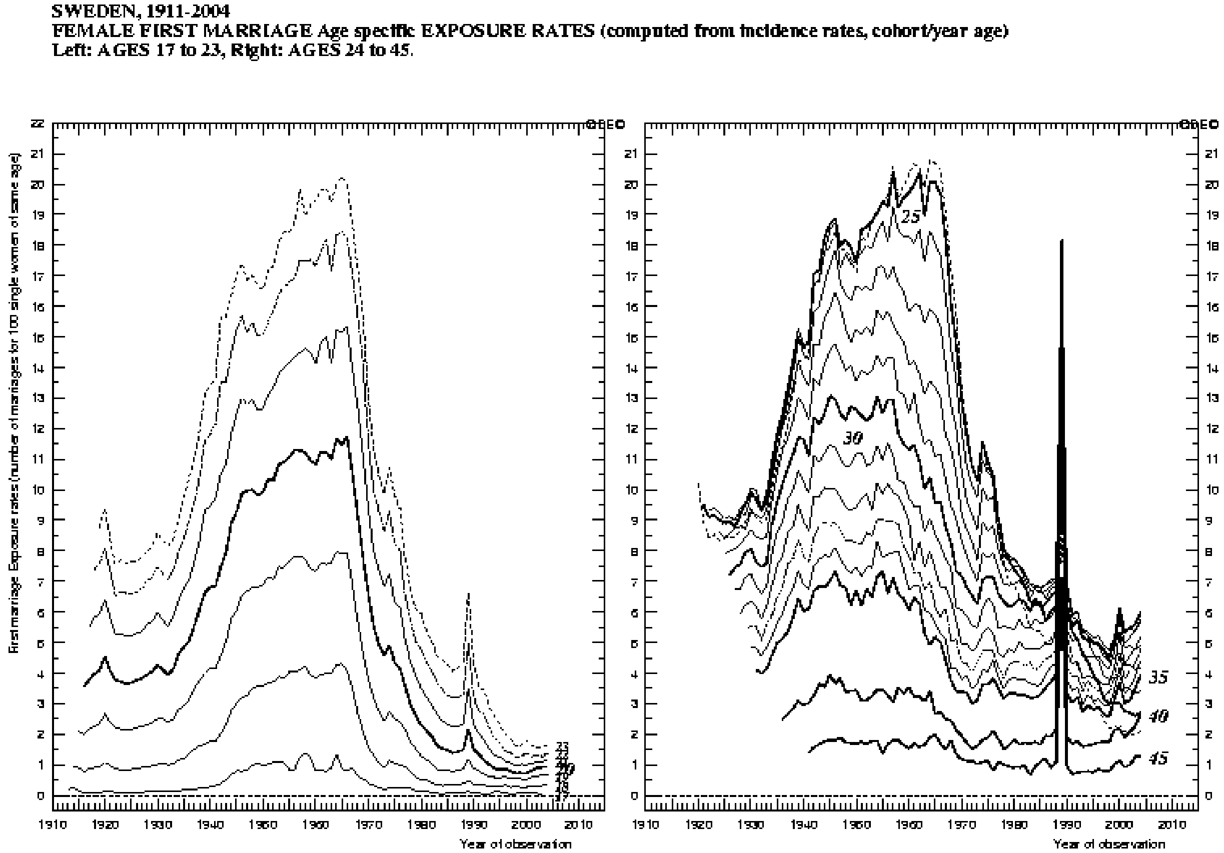


Figure 4. Sweden, Estimation of Total Cohort Female First Marriage Rate Comparison between several methods for extrapolating rates



I think that even with exposure rates it is possible to get the same kind of observation, but at a lesser extend. Indeed, if exposure rates take into consideration changes in numbers of population at risk, variations over years at a specific age are very large, and as shown by age specific female first marriage exposure rates for Sweden, there is also a significant increase since the late 1990s over age 30 (**Figure 5**).

Figure 5. Sweden, 1911-2004. Female first marriage age specific exposure rates



So, in order to take into account the last changes of intensity of a phenomenon at each age over years, it would be better to use a linear adjustment of these occurrence rates, following by this way the procedure we referred to for incidence rates. This forecast should be done for a limited number of years, in order to keep some robustness to the estimation. Indeed, it is necessary to keep in mind that in case of trend reversal in the near future, a linear adjustment can lead the estimate to be far from what will be observed. In this domain, invariability, defined by keeping constant age specific rates, is, on average, a conservative method. It is why this method is so popular.

We should be careful about robustness of test done on the several projection methods available. Indeed, results can be under dependence of the specific configuration of data during the test period. So, freeze methods will give better results when changes are limited, or when there is a reversal during test period, while linear adjustments will be more efficient when changes will be stronger.

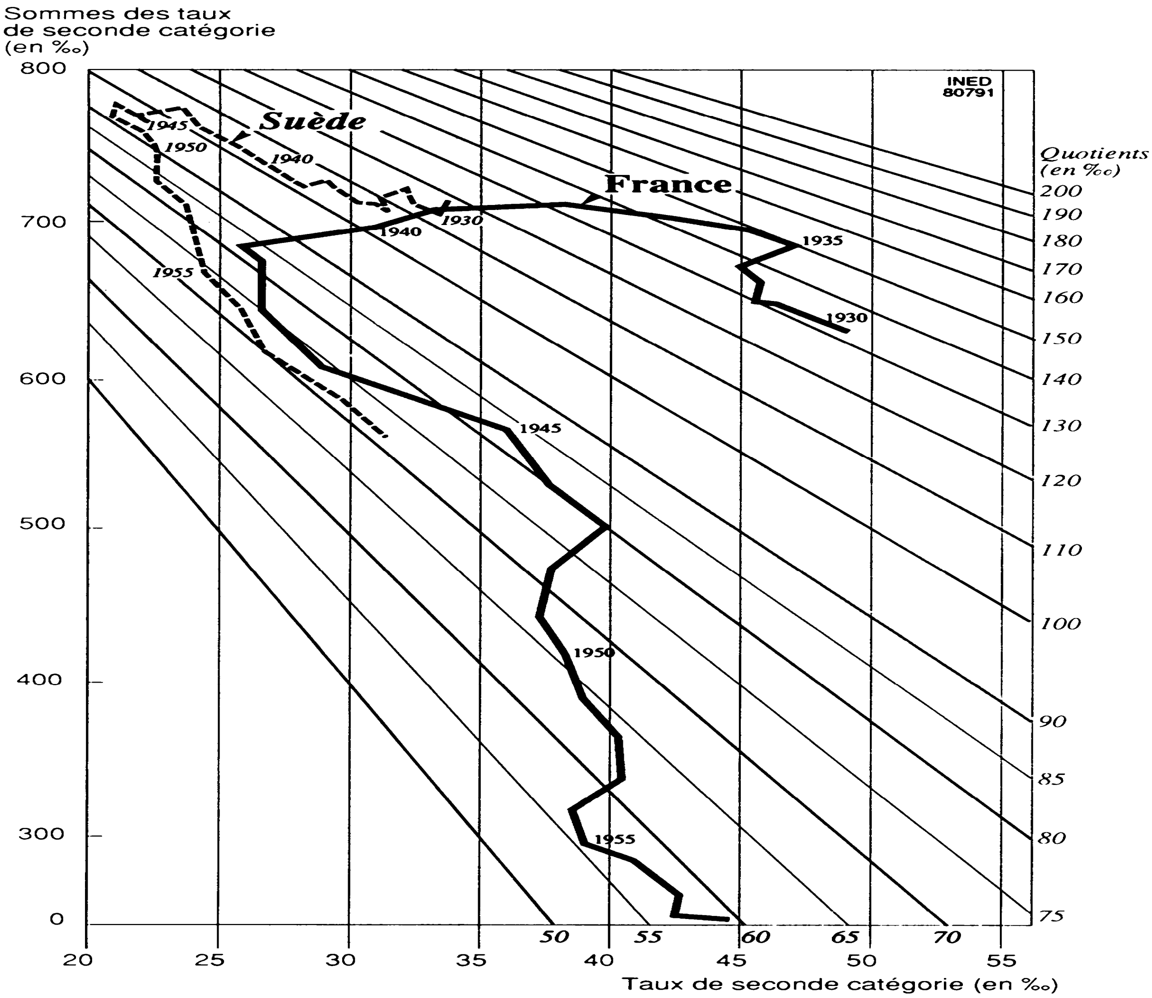
Conclusion

As conclusion I would say that it is not useful to oppose these to kind of indices between them. They are no competing. Each of them has advantages and disadvantages. There are not bad indices, there are only bad uses and very often bad interpretation of what they are.
In an article[17] written in 1991, devoted to marriages of celibacy, but conclusions are valuable for any kind of pair of incidence and exposure rates, I showed the advantages of an analyse using both kind of rates.

[17] Sardon J.-P., 1991, Une aide à l'analyse : les courbes d'iso-quotients. L'exemple de la nuptialité. *Population*, n° 6, 1991, pp. 1405-1428.

An example of such an analysis is done on **figure 6**, where for Swedish and French male birth cohorts, is represented, at the same time, trend of incidence and occurrence rates at age 29.

Figure 6. France and Sweden, male first marriages.
Trend in incidence and exposure rates at age 29[18]



From this type of graph representing the relationship between the two kind of rates, it is possible to list the meaning of each crossed variations, as you can see on the below table coming from the same article:

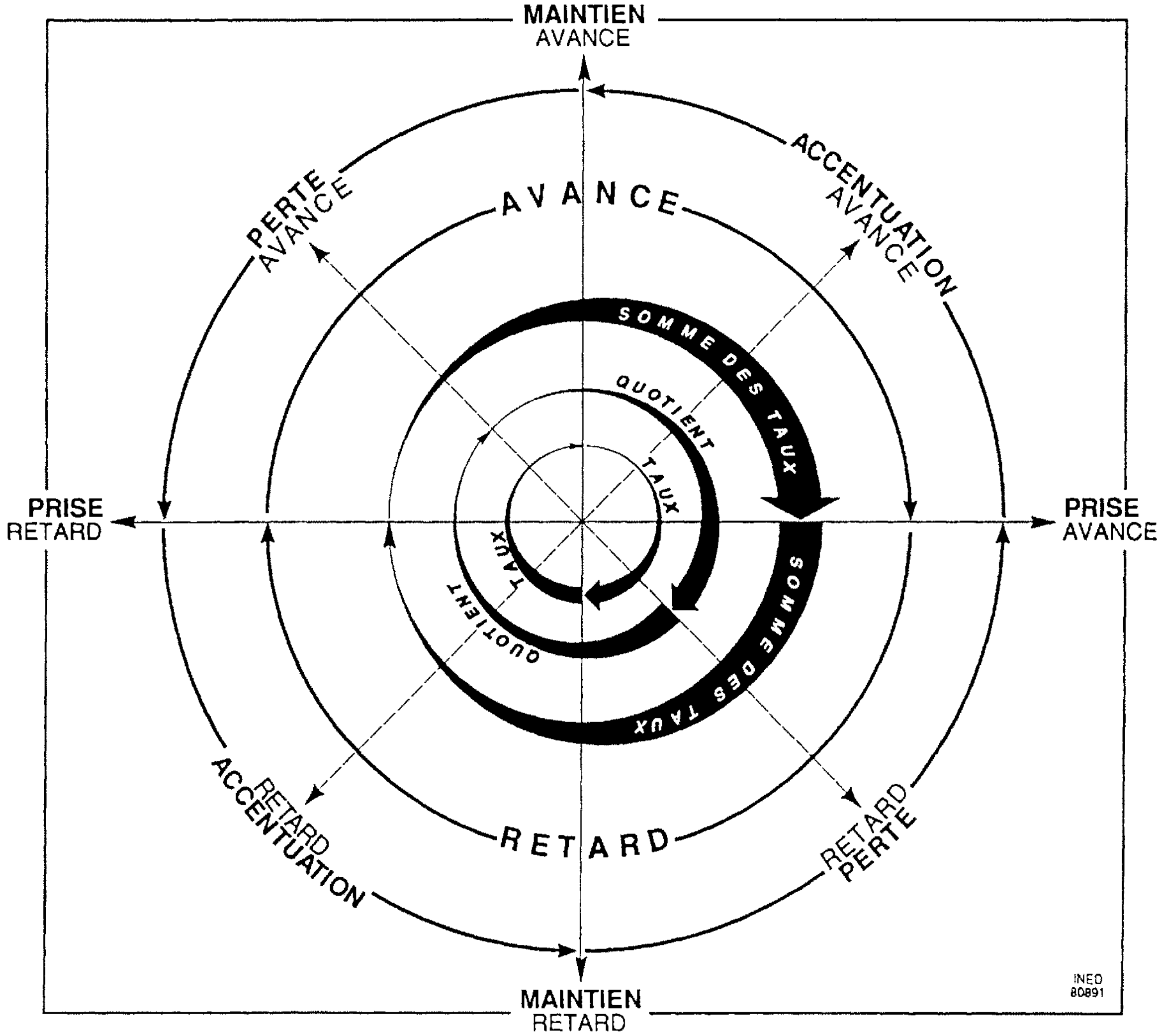
Table 2. Meaning of crossed variations of incidence and occurrence rates

Trend of incidence rate	Trend of occurrence rate		
	Increase	Stabilisation	Decrease
Increase	• Advance • Increase of advance • Recovery of delay	• Recovery of delay	• Recovery of delay
Stabilisation	• Keeping of advance	• No change	• Keeping of delay
Decrease	• Lost of advance	• Lost of advance	• Increase of delay • Lost of advance • Delay

[18] On figure, solid line refers to Sweden and dotted line to France

These results can be presented in more aesthetical way in the following graph.

Figure 7. Scheme of crossed variations between successive cohorts



An other reason to compute both sets of rates comes from the fact that mean ages at event are affected by tempo distortion when they are founded on exposure/occurrence rates and are free of such effects when incidence rates are used.