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Cohort reconstruction: which infants can be restudied at school age?

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Summary. Longer-term follow-up of infants with specific health concerns, such as low birthweight, is critical to assessing the effect of medical interventions. This report examines the approach of reconstructing previously studied cohorts in terms of the factors discriminating between respondents and non-respondents. Follow-up was attempted during 1987–1988 for 1875 children born during a 6-month period in 1978 in three geographically defined regions in the United States, for whom 1-year assessments of health and developmental status were obtained at 1 year of age as part of a previous study. For a 25% sample, participation involved a clinic visit for developmental assessments; for the remainder an interview by telephone or home visit. Follow-up was obtained for 72.5% of the cohort. Refusal rates were low (7%); most non-response was due to an inability to locate the families. Predictors of non-response reflected primarily low socio-economic status; completion rates were not influenced by mode of assessment. The role of a tracing agency is discussed. We conclude that cohort reconstruction is feasible with response rates comparable to some prospective studies with ongoing cohort maintenance.

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Introduction

To understand the natural history of many health problems and the efficacy of medical interventions, long-term studies are essential. This is particularly true for young children, since assessing the full impact of health events may not be possible until various degrees of physical and psychosocial maturation are achieved.[1] Prospective cohort studies, however, are very expensive for relatively long-term outcomes and/or relatively rare conditions, and impossible when an unanticipated outcome is encountered (i.e. the emergence of vaginal adenocarcinoma in women whose mothers were given diethylstilbestrol during pregnancy).[2]

Longitudinal information in the context of an unexpected outcome or fiscal constraint may be obtained through the return to previously studied populations, termed reconstructing a cohort.[3] Such studies have the advantage of prospectively collected data, but also incur difficulties. The latter arise from the fact that follow-up was not anticipated, and information critical to re-establishing contact with the members of the cohort may be unavailable. This problem may increase the risk of attrition biases often encountered in prospective studies,[4–7] even when special tracing techniques are employed.[8,9] Moreover, recontacting previously studied subjects who were unaware that this might occur may raise human subject concerns[10] which may place added constraints on approaches to tracing, such as prohibitions against validating address information by telephone.[9]

Prospective cohort or cohort reconstruction studies of children, particularly those in the preschool period, present some additional challenges. Names and primary caretakers may change with adoption, foster care placement or alterations of marital status of the parents. Further, unlike adults for whom work histories or credit experience[9] may assist in tracing, systematic information on children is not generally available from any agency until school age when children encounter the educational system.

More specific to our study, the outcomes of low birthweight (LBW) (< 2500 g) infants remain a critical element in discussions of the effectiveness of neonatal intensive care.[11] Despite the apparent wealth of studies on the outcomes of infants experiencing neonatal intensive care, recent reviews[12,13] have revealed significant gaps in the information available. Methodological critique[14] has also raised questions about the generalisability and potential biases of this literature (i.e. from cohorts inadequately described, and biases due to attrition not well identified). Where such information has been reported for prospective studies, losses to the cohorts generally occur disproportionately among children from families characterised by one or more indicators of socio-economic disadvantage, but the association of attrition with birthweight and other measures of neonatal and later health status has varied.[15–18] Almost no information is available on factors influencing the completeness of cohort reconstruction in this population.

We have examined this question as part of a multi-site study of the early school-age outcomes of children of varying birthweight. As part of this analysis, we have also explored the use of a tracing agency, and the effect of two different modes of data collection on response to the study.

Methods

The data in this report were obtained from the reconstruction of a cohort previously studied as part of the evaluation of the Robert Wood Johnson Foundation National Perinatal Regionalization Program. Both the overall results of this study, as well as the methods and descriptive reports on variables to be used in this report, have been published.[19–21] Those methods and results relevant to the current study are summarised briefly below.

The 1978 cohorts in three of the regions funded by the regionalisation study were selected for study. The regions were the 15 counties around Syracuse in upstate New York; Cuyahoga County (Cleveland), Ohio; and Dallas County, Texas. The study cohorts were initially defined as consisting of all births in a 6-month period in 1978 (March to August in Syracuse and Dallas, and May to October in Cleveland); the sample was stratified to include all low birthweight infants and 3% of normal birthweight (NBW) ($\geq$ 2500 g) infants.

Attempts were made to conduct a home visit with all infants not known to be dead or adopted within a 6-week period at 1 year of age corrected for duration of gestation. At that home visit, a trained lay interviewer obtained an interval health and social history and a set of developmental observations based on age-appropriate items from a standardised test of infant development.[22] Information related to the neonatal period was abstracted from the birth certificate and recoded into a common format.

The completion rate at this 1-year follow-up for these three regions was 70.0% or a total of 2538 infants. From this group, all infants born weighing $\leq$ 1500 g and a random sample of heavier birthweight infants were selected for follow-up at elementary school age, with a total of 1875.

The study activities involved contacting eligible families and requesting participation in a follow-up effort which involved an interview for the parents of about an hour's duration, a request for permission to contact the child's teacher, and, for a sub-sample of 25%, a clinic visit for developmental observations. Thus, for 75% of the study population, participation consisted of a telephone interview (or home interview for those with no telephone) and a mailed request for permission to contact the teacher. For the remainder for whom developmental observations were obtained, participation consisted of a 2-hour clinic visit. This latter group was selected to maximise our ability to assess specific subgroups and minimise family travel, and consisted of all Black subjects plus a random sample

of the remaining subjects with 1-year addresses within 1–2 hours' travel of the study site. This second restriction affected primarily the Syracuse site, and even there only moderately as most subjects lived within that travel distance.

Study rosters were prepared by retrieving the computer tapes of the original sample and updating the address information by abstracting contact information from the cards used by the interviewers at 1 year. These cards were used to record the activities used by the interviewer in tracing the family, and could include former addresses and telephone numbers, as well as similar information on other family members or friends. The amount of information varied, however.

Investigators at each site were asked to identify personnel to serve as local study staff administrators, who participated in a 1-week training session in Philadelphia. Included in this session was an introduction to the study and an introduction to tracing procedures found useful in previous studies. On return to their site, these administrators were responsible for identifying, hiring and training the interviewers, and for implementing the procedures.

The specific order and relative utility of various search strategies depended on the characteristics of the individual site and family. The process was initiated with a letter to the most recent address listed on the contact cards with explicit directions not to forward but to return if undeliverable or if a new address was available. If the letter was returned as undeliverable without a new address, a variety of techniques was employed to obtain a new address, ranging from fairly simple procedures such as checking local telephone directories or the local hospital patient information system to searches of public data bases (e.g. vital statistics, postal searches and registry of motor vehicles) as permitted under existing statutes. In addition, contacts of the family who were noted on the home visitor cards such as relatives and neighbours were also recontacted for updated address information. However, such efforts were tied to the local geographic area.

After these prescribed local tracing activities were exhausted, names were returned to the study co-ordinating centre and sent to a national tracing agency for any new address information which could be obtained from more general searches of the same type of record systems for those who may have moved from the area. Data collection continued from May 1987 to July 1988.

Respondents and non-respondents to the survey at school age were compared for variables obtained from the birth certificate and 1-year survey. In addition, the possible effect of inconvenience to the parent was assessed through a comparison of response rates among those scheduled only for the interview with those for whom a clinic visit was scheduled. Statistical methods involved chi-square analyses for categorical variables. The statistical packages used were SAS[22] and EPI INFO[23] for odds ratios and confidence intervals.

Results

Completion rates

Follow-up information was obtained for 1361 children or 72.5% of the 1875 eligible. The follow-up status for nine children, however, was noted to be a death between 1 year and the survey period. For the remaining 1352 children, parental interviews were obtained for 1067, and interviews with developmental observations for 285. The remainder of the analysis will consider these 1352 children (or 72.1% of the total).

The completion rates and reasons for non-completion overall and by site are shown in Figure 1. The completion rates varied among the sites from 82.9% in Cleveland to 66.4% in Syracuse. The major source of variation in completion was the non-location rate as refusal rates were low. The latter was 7.0% overall, ranging from 4.4% in Dallas to 8.7% in Cleveland. Families that could not be located by any combination of local and national tracing activities constituted 15.8% overall (with a high of 23.9% in the Dallas sample).

The 'other' category accounted for 4.7% of the target sample and consisted of families who were located but for whom interviews were not obtained due to repeated failure to keep appointments or to a location some distance from the study team without telephone access. For example, the relatively high rate in the Syracuse region (7%) was due in part to fiscal constraints on study staff travel to rural counties near the Canadian border to do local tracing and/or home interviews.

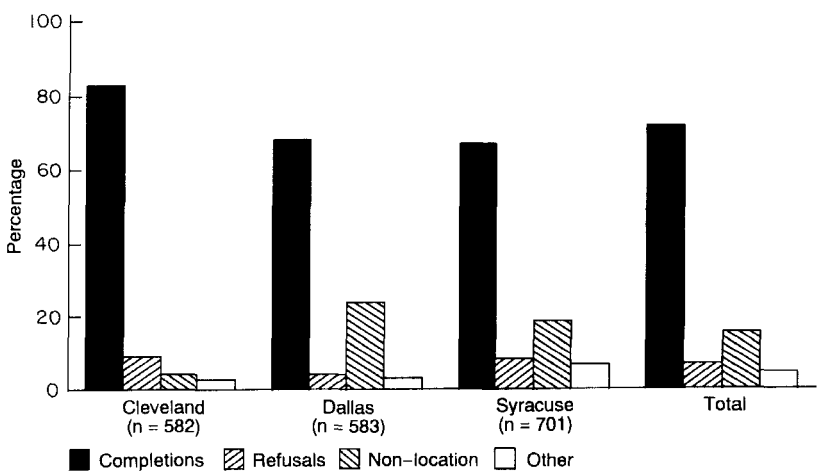

Figure 1. Completion rate and reason for non-completion by site and total.

Comparison of respondents and non-respondents

Sociodemographic factors

No differences between respondents and non-respondents were found for gender or race of child, paternal age at birth of the child, or average number of household members at 1 year of age (Table 1). Differences by maternal educational attainment at birth and 1 year and by family income at 1 year were highly significant, with more disadvantaged families among the non-respondents.

Pregnancy and neonatal events

No differences were seen between respondents and non-respondents for number of pregnancies prior to the birth of the study child, maternal hospitalisation during pregnancy before delivery, plurality, birthweight or neonatal transport (Table 2). Respondents were somewhat more likely to have delivered by Caesarean section. The most striking differences, however, were by the trimester of start of prenatal care and length of neonatal hospitalisation. Non-respondents were more likely to start care later than respondents, but less likely to have experienced a prolonged neonatal hospitalisation relative to their birthweight.

Child health and health care use in first year

Morbidity reported throughout the first year was summarised as a morbidity gradient or index which has been previously described.[21] In brief, each category of the gradient was defined as exclusive, and a child was assigned to the first

Table 1. Comparison of respondents and non-respondents by socio-demographic characteristics at birth and 1 year of age

	Percentage of[a]		
	respondents ($n = 1352$)	non-respondents ($n = 514$)	Odds ratio (CI)[b]
Gender of child (male)	48.5	46.5	0.92 (0.75–1.14)
Race of child (non-White)	26.8	28.5	1.09 (0.86–1.37)
Maternal educational attainment at 1 year (< 12 years)	23.6	39.1*	2.08 (1.66–2.60)
Maternal age at birth (< 19 yr)	17.5	21.2**	1.27 (0.97–1.64)
Family income at 1 year[c] (< $15000/yr)	53.4	64.6*	1.59 (1.27–2.00)
Number in household at 1 year (> 4)	36.0	34.3	0.93 (0.75–1.16)

[a] Chi-square for comparisons: $*P < 0.001$; $**P = 0.06$.

[b] Odds ratio and confidence interval (CI) of non-respondents compared to respondents.

[c] Data unknown for 5.5% of respondents and 10.3% of non-respondents.

Table 2. Comparison of respondents and non-respondents for pregnancy and neonatal events

	Percentage of[a]		
	respondents ($n = 1352$)	non-respondents ($n = 514$)	Odds ratio (CI)[b]
Number of pregnancies prior to study child(0)	39.7	40.7	1.04 (0.84–1.28)
Trimester of start of prenatal care (> first)	20.5	30.9*	1.74 (1.37–2.20)
Maternal hospitalisation during pregnancy before delivery	16.4	18.4	1.15 (0.88–1.52)
Multiple birth	10.8	8.6	0.77 (0.53–1.12)
Birthweight (> 2500 g)	40.0	38.2	0.92 (0.75–1.14)
Gestational age (< 37 weeks)	29.6	27.4	0.90 (0.71–1.14)
Delivered by C-section	22.7	18.3**	0.75 (0.47–1.23)
Neonatal transport	6.3	4.9	0.76 (0.47–1.23)
Duration of neonatal hospitalisation	27.2	20.2**	0.68 (0.53–0.81)

[a] Chi-square: $*P < 0.001$; $**P < 0.05$.
[b] Odds ratio and confidence interval (CI) of non-respondents compared to respondents.

appropriate category, regardless of other morbidity present. In order of assignment, the categories were: severe congenital anomaly and/or severe gross motor developmental delay (developmental quotient (DQ) equivalent to 69 or less), moderate congenital anomaly and/or suspect development (DQ between 70 and 85), mild congenital anomaly, illness requiring hospitalisation other than previous conditions, prolonged illness lasting longer than 30 days, and brief (< 30 days) or no illness. When dichotomised as the presence of any congenital malformation and/or a gross motor performance equivalent to a DQ < 85, respondents did not differ from non-respondents (Table 3). Likewise, they did not differ in the percentage of children who experienced any rehospitalisation in the first year, or in usual source of the child's medical care. However, respondents and non-respondents did differ by type of health insurance for the child's health at 1 year, with non-respondents more likely to rely on Medicaid or other sources.

Multivariate analyses

When all 19 variables were entered into a multiple logistic regression, the goodness-of-fit chi-square for the entire equation was highly significant ($P < 0.0001$). The coefficients for only four variables achieved statistical significance, however. The most powerful predictor of non-response was maternal educational attainment of less than 12 years, yielding an adjusted odds ratio of 2.060 (1.542–2.752).

Table 3. Comparison of respondents and non-respondents for child health and health care use in the first year

	Percentage of[a]		
	respondents ($n = 1352$)	non-respondents ($n = 514$)	Odds ratio (CI)[b]
Morbidity gradient[c] (without congenital malformation or developmental delay)	21.6	21.2	0.98 (0.76–1.26)
Rehospitalised in first year	13.0	16.1	1.29 (0.96–1.72)
Doctors' visits in first year (> 10)	34.5	30.4	0.83 (0.66–1.03)
Well-child visits in first year (> 6)	29.5	25.1	0.80 (0.63–1.01)
Usual source of care other than private doctor/clinic	25.2	28.9	1.21 (0.96–1.53)
Medicaid health coverage[d]	18.4	26.7*	1.61 (1.26–2.06)

[a] Chi-square: $^*P < 0.001$.

[b] Odds ratio and confidence interval (CI) for non-respondents compared to respondents.

[c] See reference 21 for full description. Morbidity gradient is a summary measure of morbidity reported in first year, in this instance dichotomised between those reporting a congenital malformation (regardless of severity) and/or observed to have a gross motor performance equivalent to a developmental quotient less than 85.

[d] Publicly supported health insurance for those qualifying for welfare and other low-income individuals.

A family income at 1 year of < $15,000 (OR 1.324(1.012–1.733)) and start of prenatal care later than the first trimester (OR 1.429(1.074–1.903)) also increased the risk of non-response. A neonatal stay greater than the 75th centile for birthweight decreased the risk of non-response (OR 0.697(0.522–0.929)).

Method of assessment

Of the 1875 in the sample, 461 (24.7%) were selected for physical and developmental observations. The response rate among these 461 was 69.2% as compared to 72.8% among the remaining 1314, a difference which was not statistically different (χ^2 with Yates' correction = 1.88, $P = 0.17$; odds ratio = 0.84(0.66–1.06)).

Use of tracing agency

Of the 1875 in the sample, 679 cases were referred back to the study co-ordinating centre for tracing. Of these, the tracing agency was able to provide new information on 427 (62.9%). Of the 1187 not sent for tracing, 87.4% were completed as opposed to 46.4% among those referred to the tracing agency. However, the information from the agency often proved to be insufficient to accomplish contact.

The completion rate among the 427 with new information was 52.2%, and among those without new information, 36.5%. The latter represented the results of continued local tracing efforts. The completions among those referred for tracing for whom new information was obtained accounted for 16.5% of all completions.

Discussion

These results suggest that the re-construction of cohorts of young children is feasible, and that response rates may be comparable to prospective studies with ongoing cohort maintenance over similar or shorter periods of time. While the Infant Health and Development Program achieved a 93% follow-up rate at age 3 with relatively extensive efforts,[18] the Collaborative Group on Antenatal Steroid Therapy retained 63% at the same age.[16] The follow-up rate for the National Perinatal Collaborative Study was 79% at age 7.[15] The completion rate in our study was comparable to that of the National Perinatal Study without their substantial cohort maintenance activities, and represents a substantial improvement over the 10–15% per year attrition projected in some studies.[16,18] Our completion rate is comparable to the participation rates in some recent cross-sectional studies, as well.[7]

Attrition is not equally likely among all subgroups in the population and is more likely among groups characterised by evidence of socio-economic disadvantage such as low maternal education or low income.[16,17,19] Like Aylward,[16] differences in patterns of attrition among different sites were seen, although maternal education remained the predominant factor across all sites. In contrast to other studies,[7,16] attrition was not associated with measures of infant morbidity including birthweight, gestational age, and subsequent health events during the first year. If anything, sicker infants may be more likely to be located and to participate, as indicated by the effect of longer neonatal hospitalisations.

The results are similar to the experience in the first round of assessment at 1 year, when less well-educated families were less likely to participate, but no difference by birthweight was seen. However, this comparison should be interpreted cautiously as about half the losses at 1 year reflected families that could not be scheduled in the 6-week window so that final response rates with a less restrictive design remain difficult to estimate.

In addition, no differences were noted between those scheduled for a clinic visit as compared to those requiring an interview only. Others have compared telephone, mail and home visit methods for obtaining information, and generally found that the quality of the information is similar but that refusal rates are somewhat higher for telephone interviews.[24–26] No studies to our knowledge have examined the effect of inconvenience comparable to ours. For those selected for the observations, completion required travel time as well as the time of the interview, often involving rearrangements in parental work and child school

schedules, offset by only a very modest travel/parking reimbursement and prepared statement for teachers. Perhaps parents perceived the free, comprehensive battery of cognitive and achievement tests as attractive. The exact factors influencing participation in such efforts warrant further research. Based on our results, however, the mode of data collection is not a factor.

Finally, the usefulness of a tracing agency was examined. Little published literature is available to guide investigators in designing appropriate follow-up strategies, and our experience represents an addition to this literature. As with Nash *et al.*,[8] local tracing activities prove to be the most productive, and depend on a dedicated and persistent field team. We were fortunate to recruit one field supervisor whose previous employment had provided substantial experience in these techniques, and who provided teams at other sites with practical consultation which augmented the methods cited in the manual.[27,28] However, as Nash *et al.* note,[8] tracing beyond the local area increases expenses due to travel time and costs, suggesting the use of agencies with existing capacity to perform this task. However, little information is available to estimate the gain to be realised. In one study comparing two firms, the tracing rate was about 33% for both firms, but one firm appeared to be more limited in its scope, with lower tracing rates among those considered to have moved more distantly than those remaining in the same state.

While our completion rate of 46.4% among those sent to the tracing agency appears to be an improvement over this experience, the interpretation of these numbers should take into account the completion rate among those for whom the agency was unable to provide new information due to continued local tracing efforts. Thus, an estimate of completion rates among cases referred for tracing should be considered a maximum.

This experience illustrates a factor affecting both local and distant efforts, namely the length of time allowed for tracing. The productivity of our survey teams might have been enhanced if all names had been sent for tracing before interviewing began. However, the costs of tracing are calculated on a per-case basis and not on the results. Further, telephone contact by the tracing agency to verify address information was precluded by restrictions on who might contact families placed by one funding agency. Since, in addition, this first contact remains an important entry point of data collection, we elected to start with local efforts, and provide the tracing agency with difficult-to-locate cases. This strategy, however, places limits on the time over which the agency can pursue various sources of information, and such searches proved to take at least 3 months. By providing more detail in the logistics of tracing, our study does not suggest an optimal strategy, but does provide information for developing more successful tracing strategies for future studies.

Among the considerations in using an external tracing agency is assuring the confidentiality of participant information and the voluntary nature of their

participation. In this regard, it should be noted that, in the previous round of survey work, other contacts were not specifically precluded, but the use of the data was restricted to the study team as part of the permission signed by the parents. Both the principal investigator and project director of the assessment at 1 year are intimately involved in this project.

Second, the objective of the tracing is to obtain an adequate address to describe the selection and follow-up procedures, and to invite the family's participation. This letter and procedures have been reviewed by the institutional review boards at each site and at the Children's Hospital of Philadelphia to assure compliance with federal regulations for the protection of human research subjects.

Third, all personal identifier files are maintained separately from any assessment data on the child and family. All contacts with the family are by the research team with permission to access the data; as noted above, the tracing agency is not permitted to contact the family even to verify the address.

Finally, the local assessment teams are carefully trained to respect the right of refusal of the family. As with other studies,[10] our low refusal rate and high participation rate among located families suggests that such procedures are acceptable.

The interpretation and generalisability of the results must take into account some of the features of the study sites and previous experience of the cohort. Thus, the subjects eligible for this study are those who could be located at 1 year of age within a brief period of time. Further, the study population does not include high proportions of families likely to be very difficult to trace such as undocumented workers. Thus, the study population to some extent has already been selected for those who might be more readily located and willing to participate in a study, although it is similar to most prospective studies of LBW (or, more generically, biologically vulnerable) infants in terms of socio-demographic characteristics.

In addition, the losses to the cohort will require careful analytical attention in reports covering the results of the school-age assessment, but the exact approaches may vary according to the type of outcome being described. We have already begun comparisons of those assessed at 1 year with the subset of those assessed at school age in terms of distribution of these cohorts over the wide range of variables available at 1 year, and in terms of correlations among variables especially with birthweight as suggested by other investigators.[29] Despite the statistical significance reported here, the two groups appear highly similar. We recognise that these comparisons provide only partial reassurance, and plan to assess the effect of cohort losses, especially losses among more disadvantaged families, through sensitivity analyses and multivariate modelling, where such techniques appear appropriate.

Despite these concerns, we conclude that cohort reconstruction of samples of

young children is feasible, with completion rates comparable to prospectively followed cohorts. This study supports the recommendations of the Select Panel for obtaining important longitudinal information from previously studied cohorts.[3]

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