

Addison, John T., Douglas A. Fox, and [Christopher J. Ruhm](#). "The Impact of Advance Notification: A Comment On a Study By Nord and Ting," *Industrial and Labor Relations Review*, Vol. 45, No. 4, July 1992, 665-673.

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## THE IMPACT OF ADVANCE NOTICE: A COMMENT ON A STUDY BY NORD AND TING

JOHN T. ADDISON, DOUGLAS A. FOX, and [CHRISTOPHER J. RUHM](#)\*

IN their analysis of the effects of advance notification of plant closings, Nord and

Ting (*ILR Review*, July 1991) claim that much can be expected of current legislation that mandates a 60-day prenotification standard because the outcomes of voluntary arrangements, in an interval preceding implementation of the legislation, point to substantial reductions in both joblessness and earnings losses for workers given over two months' warning of their impending layoff. They also suggest that many of the disparate results reported in the earlier plant closing literature reflect the inability to distinguish between formal and informal notice on the basis of the 1984 and 1986 Displaced Worker Surveys.<sup>1</sup>

The empirical analysis presented in this comment focuses exclusively on the authors' "unemployment" results.<sup>2</sup> Using a

\* John T. Addison is Professor of Economics, University of South Carolina, and Visiting Professor, Westfälische Wilhelms-Universität Münster; Douglas A. Fox is Assistant Professor of Economics, Penn State University—Shenango; and Christopher J. Ruhm is Associate Professor of Economics, University of North Carolina—Greensboro.

1. Two other studies, Addison and Portugal (this *Review*) and Ruhm (1992), have also analyzed the relation between advance notice and post-displacement joblessness, using the 1988 Displaced Worker Survey. Earlier work is summarized in Addison and Portugal (this *Review*, and 1991).

2 We deliberately restricted our focus in this manner because of additional problems attaching to the authors' wage change analysis. First, there is no clear behavioral link between the unemployment analysis and the earnings development analysis. To the extent that the avoidance of unemployment finding underpins the beneficial earnings result, it would appear logical to have tested, among other things, whether those notified workers without any intervening spell of joblessness "outperformed" their notified counterparts with positive spells. Second, it

similar sample and reduced form probit model (see next section), we do not obtain confirmation of Nord and Ting's (N&T's) finding that written notice exceeding two months reduces the likelihood of experiencing a positive spell of joblessness. Once we restore to the sample two groups that we believe must be included in it (non-notified workers and non-employed workers) and introduce informal notice as a separate regressor, we do obtain the N&T result for plant closings, but not for other sources of layoff. Disaggregation of the samples by gender and broad occupation produces further changes in the pattern of results. These findings indicate that the estimated notification effects are extremely sensitive to small changes in the sample and model specification and that considerable caution is required in drawing policy conclusions.

We also find that once a worker experiences some unemployment, extended intervals of written notice do not "produce" reductions in the spell length of joblessness. For a sample that closely approximates that used by N&T, we do observe an insignificant negative impact of extended notice on joblessness; but for a sample that includes non-notified and non-employed workers the coefficients are positive, significantly so in the case of the 1990 DWS. This result does not, of course, mean that long notice causes higher

may be misleading to focus on wage changes (rather than wage levels) if notice is systematically provided to workers with high or low wages. Third, selection problems may be particularly severe because of the inclusion of recently displaced workers in the sample, a relatively small proportion of whom will have secured reemployment at the survey date.

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unemployment; rather, it hints at the endogeneity of notice, a phenomenon N&T fail to consider.

N&T's suggestion that the information (on written notice) contained in the latest Displaced Worker Surveys may help clarify divergent findings from past surveys does not receive support in the results reported here, which point to notification effects that depend critically on sample stratification. As before, the task remains one of identifying and explaining the disparate effects of notice.

### Methodology

We deploy essentially the same probit model as do N&T to ensure maximum conformity between the two sets of reduced form regressions.<sup>3</sup> Thus, twenty of the independent variables deployed here are identical to those used by N&T. Changes were made to just three of the variables. Specifically, the dummies for broad occupational status (BLUE COLLAR) and industry (MANUFACTURING) were defined at the point of displacement rather than at the survey date, and instead of using the displaced worker's state unemployment rate averaged from the date of displacement we substituted the relevant state unemployment rate obtaining at the time of displacement.<sup>4</sup> Otherwise, the form of the estimating equation is unchanged. It follows that the principal difference between our analysis and N&T's has to do with sample construction.

We estimated the probit equation for six separate samples. First, we use the N&T

<sup>3</sup> We are indebted to Nord and Ting for supplying us with copies of their programs.

<sup>4</sup> To the extent that occupational status influences post-displacement outcomes (for example, via its effect on reservation wages and specific human capital), it is the pre-displacement, rather than the post-separation, occupation that is of key importance. The authors' use of the unemployment rate averaged from the date of displacement is also dubious because the typical worker was displaced 2.5 years before the survey date but was out of work for less than one year. Thus, the initial, rather than the averaged, unemployment rate is a more meaningful indicator of economic conditions confronting the displaced worker during the period of job search.

sample [model (0)], which includes only those workers who responded in the affirmative to the broad notice question in the DWS and who were reemployed in January 1988. This sample thus includes reemployed workers who received either informal notice (INFONOT) or one of the three lengths of written notice (WRITNOT, WRITNOT2, WRITNOT3) but excludes the

most logical control group, namely, those who did not learn of or otherwise anticipate their displacement. Thus, in N&T's estimating equation the informally notified constitute the omitted category.

Second, we add back to this sample all those respondents who never found work in the wake of displacement [model (ii)], thereby including both the economically active and inactive. Third, we add in those who were not notified of their impending displacement [model (iii)]. This third sample is our preferred one for the obvious reason that in seeking to assess the impact of notice on the likelihood of observing a positive spell of joblessness, the most logical reference category is the non-notified rather than the informally notified. Accordingly, the probit now includes, in addition to the three formal notice variables, informal notice as a separate regressor, the non-notified being the reference category. Fourth, workers displaced from construction jobs and the armed forces were excluded [model (iv)], to ascertain the sensitivity of the results to the inclusion of certain groups for whom the meaning of displacement is opaque. In what follows, the results from our preferred specification [model (iii)] will receive emphasis.

The sample in models (i) through (iv) is made up of workers displaced by reason of plant closings. This source of displacement accounts for around 50% of displaced workers, the balance comprising those permanently laid off either because of slack work (analogous to mass layoffs) or because of abolition of shift or position. We see no reason to examine plant closings alone—each group receives the various types of notice and each is covered by current legislation mandating 60 days' notice, subject to a basic employment size

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threshold and magnitude of layoff constraint—and so our two final specifications apply the sample inclusion criteria of models (iii) and (iv) to workers displaced by slack work and abolition of shift or position.

Each model is estimated for both the 1988 and the 1990 DWS, the latter having recently become available. In addition, a number of disaggregations of the data are attempted to ascertain the sensitivity of our basic results to sample construction.

Finally, although N&T do not provide results for the duration of post-displacement joblessness, simply noting that "written advance notification had *no* significant impact on the length of unemployment spells of dislocated workers" (fn. 10, p. 688) (emphasis added), we also estimate a parametric jobless duration equation using the flexible, three-parameter extended generalized gamma variant of the accelerated failure time model (Addison and Portugal 1987).<sup>5</sup>

#### Findings

Table 1 provides summary findings for the reduced form probit equations. (Full results are available from the authors on request.) The upper panel of the table gives results for specifications that identify the three categories of written notice alone, the reference category being the

<sup>5</sup> The dependent variable in this exercise requires modest elaboration. The duration measure is derived from responses to the question in the DWS asking displaced workers how many weeks passed before they found a job. Replies to this question, in addition to providing information on the (completed) spell of unemployment, also encompass a "never found work" response. Such workers are included, and their joblessness top coded at 99 weeks, if they were displaced within the first three years of the survey (1983 to 1985 in the case of the 1988 DWS and 1985 to 1987 for the 1990 DWS). Workers displaced in the last two years of each survey, however, were included only if they were reported as currently unemployed on the basis of the labor force status question in the parent CPS. If so, their unemployment duration, necessarily incomplete spells, was used in the duration analysis. We experimented with various ways of treating persons who reported that they had never found work, but the results were insensitive to the alternatives considered.

informally notified in models (i) and (ii) and the informally notified plus the non-notified in models (iii) through (iv). The lower panel of the table includes informal notice as a separate regressor, as is (we argue) more appropriate.

The most obvious finding is that the basic N&T result does not obtain for the 1988 DWS, using a similar sample and their specification of the notice variables. Indeed, we obtain results indicating that notice intervals of greater than two months' duration "work" for the 1988 DWS only when we add in the non-employed and the informally notified. There is even less evidence of a beneficial effect of lengthy notice in the 1990 DWS: the coefficients on WRITNOT3 are never statistically significant and are frequently positive rather than negative. In fact, as can be seen from the lower panel of the table, the coefficients on WRITNOT3 are better determined for plant closings, in the case of the 1988 DWS, once informal notice is included as a separate regressor. Informal notice is also highly significant, and remains so for the 1990 DWS, for which WRITNOT3 is no longer significant.

What is true for plant closings does not carry over to other layoffs. For workers dislocated by slack work and abolition of shift or position, the longest notice interval is nowhere significant. Rather, it is notice of between one and two months' duration that is associated with a reduced likelihood of observing positive spells of joblessness. As noted above, informal notice is always significant at conventional levels. These findings provide strong evidence that the estimated notice coefficients are sensitive to plausible changes in the model specification and sample inclusion criteria.

We next consider the relationship between advance notice and the duration of post-displacement joblessness. Table 2 shows the results of estimating the extended generalized gamma accelerated failure time model. As suggested by N&T, extended notice fails to significantly mitigate the subsequent joblessness of workers displaced by plant closings when the non-notified and non-employed are ex-

Table 1. The Effects of the Various Types of Advance Notice on the Probability of Positive Post-displacement Joblessness.  
(Asymptotic Standard Errors in Parentheses)

Type of Notice	1988 DWS						1990 DWS					
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(i)	(ii)	(iii)	(iv)	(v)	(vi)
WRITNOT1	.151 (.193)	.159 (.172)	.102 (.167)	.121 (.172)	.178 (.166)	.153 (.180)	.069 (.194)	.100 (.178)	-.022 (.170)	-.097 (.173)	-.148 (.165)	-.116 (.181)
WRITNOT2	.062 (.169)	-.022 (.129)	-.124 (.151)	-.089 (.157)	-.459** (.193)	-.502*** (.196)	.339* (.191)	.391** (.181)	.261 (.173)	.330* (.185)	-.446** (.179)	-.392** (.184)
WRITNOT3	-.201 (.146)	-.218* (.129)	-.284** (.122)	-.237* (.128)	-.182 (.222)	-.247 (.228)	.132 (.168)	.153 (.148)	.023 (.138)	-.000 (.141)	-.198 (.226)	-.165 (.230)
WRITNOT1			.005 (.173)	.000 (.178)	.100 (.171)	.085 (.184)			-.208 (.176)	-.314* (.180)	-.300* (.171)	-.284 (.187)
WRITNOT2			-.222 (.157)	-.209 (.163)	-.531*** (.197)	-.565** (.200)			.068 (.179)	.102 (.191)	-.589*** (.184)	-.554*** (.191)
WRITNOT3			-.386*** (.130)	-.363*** (.136)	-.249 (.225)	-.304 (.231)			-.172 (.145)	-.228 (.149)	-.330 (.231)	-.315 (.235)
INFONOT			-.185** (.076)	-.226*** (.081)	-.181** (.085)	-.163* (.095)			-.346*** (.078)	-.406*** (.083)	-.347*** (.094)	-.396*** (.105)
n	1007	1333	2298	2061	2206	1872	948	1232	2128	1944	1915	1601
Mean of Dependent Variable	.806	.843	.867	.868	.910	.911	.802	.832	.855	.861	.912	.913
$F\left(\frac{X\beta}{\sigma}\right)$	.826	.862	.880	.882	.821	.924	.829	.862	.873	.881	.923	.925

*Model Specifications:*

- (i) Broad Nord and Ting model and sample (workers displaced by plant closings).  
(ii) Model (i) but with sample extended to include non-employed workers.  
(iii) Model (ii) but with sample further extended to include non-notified workers.  
(iv) Model (iii) but sample excludes construction and armed forces.  
(v) Model (iii) but estimated over layoffs (abolition of shift or position plus slack work) rather than plant closings.  
(vi) Model (iv) but estimated over layoffs (abolition of shift or position plus slack work) rather than plant closings.

\* Statistically significant at the .10 level; \*\* the .05 level; \*\*\* the .01 level.

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NCE NOTICE: A COMMENT

*Table 2. Effects of the Various Types of Advance Notice on Postdisplacement Joblessness: Accelerated Failure Time Model, Extended Generalized Gamma Distribution.*

Type of Notice	1988 DWS						1990 DWS					
	(i)	(iii)	(v)	(i)	(iii)	(v)	(i)	(iii)	(v)	(i)	(iii)	(v)
WRITNOT1	.052 (.167)	[.350*] (.189)	.252 (.164)	[.161] (.195)	-.044 (.126)	[.087] (.145)	-.101 (.174)	[.129] (.198)	-.217 (.171)	[-.000] (.207)	.220 (.139)	[.232] (.163)
WRITNOT2	.064 (.159)	[.090] (.163)	.134 (.168)	[.057] (.187)	.055 (.210)	[.058] (.207)	-.067 (.150)	[-.151] (.166)	-.189 (.146)	[-.115] (.178)	.233 (.193)	[-.102] (.200)
WRITNOT3	-.029 (.153)	[-.212] (.150)	.197 (.151)	[.014] (.170)	.350 (.220)	[-.030] (.230)	.368*** (.148)	[.435***] (.162)	.280*** (.143)	[.054] (.168)	.146 (.204)	[.171] (.218)
INFONOT		.067 (.076)	[-.046] (.086)	-.041 (.071)	[-.080] (.079)				-.079 (.075)	[-.258***] (.088)	-.141* (.075)	[-.140*] (.086)
n	796 [988]	1888 [2191]	1899 [2098]	748 [934]	1723 [2029]	1654 [1819]						

*Notes:* Figures in square brackets are the corresponding values for a duration equation estimated over all spells of unemployment. Model specifications are for (i), (iii), and (v) as given in Table 1.

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cluded. But note that, as the sample is widened, we find that the coefficients on WRITNOT3 become positive, quite large, and—in the case of the 1990 DWS—statistically significant. This does not imply that the longest interval of written notice *extends* the unemployment duration of workers displaced by plant closings. Rather, the results hint at the presence of unobserved characteristics that are positively correlated with both receipt of extended notice and lengthened jobless spells, and caution against assigning a causal interpretation to reduced form estimates. In any event, the findings weaken N&T's conclusion that mandatory notice would substantially reduce post-displacement joblessness.

Note, too, the uniformly negative coefficients on informal notice when the duration equation is estimated over all spells of joblessness, including direct transitions from employment to employment (zero spells).<sup>6</sup> In general, no such relation is reported for the longest interval of notice.

Further evidence of the fragility of the estimated notification effects is evident in disaggregation of the data by broad occupational status and gender. Results for the 1988 DWS are given in Table 3. The N&T result appears to hold for blue-collar workers and for both genders in the case of plant closings. But for other layoffs the longest interval of written notice is not associated with a significant reduction in the probability of observing some positive spell of joblessness, with the exception of white-collar workers, for whom WRITNOT2 is both stronger and better determined. On the other hand, the coefficients on informal notice are significant for white-collar workers and men for both sources of displacement, but not for blue-collar workers or women.

Further disaggregations of the data jointly by occupation and gender were also estimated. Because of the small number of observations on lengthy notice, WRITNOT2

<sup>6</sup> Zero weeks of unemployment were assigned a value of .01 week.

and WRITNOT3 were merged.<sup>7</sup> The results are provided in the appendix table. Note that differences in sample sizes, specification of the reduced form probits, and grouping of layoff categories limit the comparisons that may be drawn between the results given in the appendix table and those supplied by Addison and Portugal (this volume), despite the superficial correspondence between the results for all other layoffs in the former and for all displaced workers in the latter.

It can be seen that a further differentiated pattern of results obtains. Longer intervals of written notice "work" for blue-collar men but not for white-collar men in the case of plant closings, whereas exactly opposite results are reported for all other layoffs. Only for female white-collar workers are the results for longer intervals of notice "consistent" across reasons for displacement. Any suggestion that simple compositional effects explain the results for blue-collar workers (all workers are affected in the case of plant closings, but only a subset thereof in cases of slack work and abolition of shift or position) confronts the obvious difficulty that the same does not hold for white-collar men. The safest inference, therefore, is that the endogeneity of notice underpins these very disparate results. Unobserved factors are likely to figure larger in data disaggregation of this type. The principal *finding*, however, is again that the effects of formal notice vary greatly across population subgroups.

### Endogeneity of Notice

N&T treat their reduced form estimates of the association between advance notification and the probability of experiencing post-displacement joblessness as if they imply a causal relationship. If voluntary notice is provided on a nonrandom basis,

<sup>7</sup> The probit equations could not be estimated for blue-collar women despite grouping observations on the two longest notice intervals. Specifically, the number of (negative) observations (6 for layoffs and 15 for plant closings) fell below the number of independent variables (22, including the constant) for this subgroup.

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**Table 3. The Effects of the Various Types of Notice on the Probability of Experiencing Positive Post-displacement Joblessness: Separate Estimates by Broad Occupation and Gender, 1988 Displaced Worker Survey.**

Type of Notice	Blue-Collar		White-Collar		Men		Women	
	(iii)	(v)	(iii)	(v)	(iii)	(v)	(iii)	(v)
WRITNOT1	.148 (.261)	.228 (.260)	-.113 (.237)	-.084 (.235)	-.019 (.226)	.042 (.198)	.014 (.278)	.178 (.348)
WRITNOT2	-.090 (.254)	.184 (.506)	-.336* (.203)	-.748*** (.230)	-.217 (.199)	-.217 (.285)	-.247 (.263)	-.881*** (.305)
WRITNOT3	-.535*** (.196)	5.151 (4772.7)	-.273 (.178)	-.450* (.243)	-.378** (.169)	-.211 (.323)	-.375* (.210)	-.191 (.335)
INFONOT	-.040 (.120)	-.0614 (.125)	-.292** (.101)	-.277** (.118)	-.246*** (.098)	-.265*** (.101)	-.118 (.128)	.101 (.176)
n	1070	1120	1228	1086	1383	1465	915	741
Mean of Dependent Variable	.891	.933	.846	.887	.858	.904	.880	.922
$F\left(\frac{\sum \beta}{\sigma}\right)$	.907	.949	.861	.901	.874	.914	.901	.950

Note: model specifications (iii) and (v) are given in Table 1.

however, such a causal interpretation is likely to be incorrect. Indeed, the fragility of estimated notification "effects" strongly suggests that advance notice may be correlated with unobserved (to the econometrician) factors that influence joblessness. If so, limited information is provided by the reduced form models.

Recent research suggests that lengthy written notice is provided endogenously. Most pertinent to this comment are studies by Ruhm (1992) and Jones and Kuhn (1991). Ruhm provides evidence indicating that although persons receiving substantial written notice have higher probabilities of avoiding joblessness than their counterparts surprised by the displacement event, they are also more likely to experience *lengthy* spells of joblessness. This result, which is consistent with the findings reported here, implies that formally notified workers have lower reemployment hazards after entering unemployment than their non-notified counterparts. It is difficult to reconcile this result with any conclusion other than that the provision of advance notice is endogenous.<sup>8</sup>

Using the Ontario Ministry of Labour data set, Jones and Kuhn similarly test for and uncover significant evidence of endogenous advance notice for their sample

<sup>8</sup> Ruhm also implements a more technical test that provides further evidence of endogeneity.

of Canadian firms. The key identifying information used in their test is the variation across firms in the legal minimum notification period, which is a function of the size of the layoff. Durations beyond the legal minimum, however, are provided at the discretion of the employer. That is to say, inter-firm differences in the minimum length of notice are largely exogenous, whereas intra-firm variations are endogenous and likely to depend on unobserved characteristics.

Although this comment has not focused on N&T's wage regressions, the nonrandom provision of notice is likely to create bias in these results as well. In addition, earnings and unemployment could interact in important ways that receive little attention in N&T's analysis. For example, firms voluntarily providing lengthy written notice may be more likely than other firms to offer additional kinds of displacement assistance in the form of skills training and supplemental unemployment benefits, inter alia. Workers receiving this package of benefits may be quite likely to stay unemployed longer, particularly if their initial efforts to find new employment fail, but receive higher reemployment wages than their counterparts receiving neither advance notice nor other forms of assistance. Yet, however beneficial the voluntary provision of notice in such circumstances, without further infor-

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mation (on the package) one is patently unable to rule on the efficacy of mandatory notification.

#### Conclusions

The regression estimates obtained by Nord and Ting are extremely sensitive to plausible (and necessary) changes in the model specification. Furthermore, Nord and Ting pay inadequate attention to the correlation between advance notice and unemployment once the jobless spell is under way and fail to consider how their results might be biased by endogenous provision of advance notice.

Our main findings are as follows. First, extended intervals of written notice have no discernible effect on the probability of avoiding a spell of joblessness for those displaced by reason of slack work or abolition of a shift or position, and the favorable effects observed for plant closings do not carry over to the 1990 DWS. Second, written notice does not reduce the length of positive spells of joblessness in any of the single equation models and is often correlated with increased jobless durations. This finding suggests the importance of unobserved characteristics associated with the receipt of extended notice and underscores the need for caution when formulating policies relating to advance notice and job security. Third, the more detailed information on notice provided in the latest Displaced Worker Surveys does not provide a "solution" to an earlier literature drawing on the 1984 and 1986 surveys and, in particular, does not explain the strong showing of informal notice in a number of the models.

We do not argue that the provision of extended notice in private contracts is without benefit, merely that the variation

in observed outcomes clouds any simplistic interpretation of its efficacy. The regime shift associated with legislating mandated notice further muddies the waters, although it is not obvious that the current legislation has significantly increased the frequency with which notice is provided (Addison and Blackburn 1991).

Nord and Ting are correct in noting that the impact of advance notice extends beyond jobless duration to encompass earnings development. Pending improvements in our understanding of the mechanisms through which prenotification facilitates job finding, however, the link between the two parts of their study remains tenuous and any (implicit) suggestion that they have provided an integrated model of unemployment and earnings changes should be resisted. Detailed treatments of the effects of notice on earnings (for example, Addison and Fox 1991; Ruhm 1991) provide some supporting evidence that the longest interval of written notice is correlated with higher post-displacement wages but show that the causal connections leading to this result are far more complicated than those implied in Nord and Ting's simple reduced form estimates. An integrated treatment of earnings and joblessness awaits improvement to our understanding of the process through which workers obtain reemployment.

At a minimum, considerable caution must be exercised in attributing strong policy implications to the results of a single study. Thus, we consider Nord and Ting's concluding statement—"These results confirm the appropriateness of the 60-day notification period required by the Worker Adjustment and Retaining Notification Act of 1988" ( p. 691)—to be both premature and overstated.

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**Appendix**  
**The Effect of the Various Types of Notice on the Probability of Experiencing Positive Post-displacement Joblessness, by Broad Occupation and Gender, 1988 Displaced Worker Survey**

Type of Notice	(iii) Blue-Collar		(v) Blue-Collar		(iii) White-Collar		(v) White-Collar	
	Men	Women <sup>a</sup>	Men	Women <sup>a</sup>	Men	Women	Men	Women
WRITNOT1	.137 (.297)		.178 (.270)		-.185 (.379)	-.119 (.325)	-.276 (.308)	-.117 (.385)
WRITNOT2 + WRITNOT3	-.394** (.186)		.435 (.475)		-.220 (.225)	-.379* (.201)	-.694** (.280)	-.556** (.244)
INFONOT	-.163 (.133)		.222 (.133)		-.395*** (.151)	-.275* (.144)	-.629*** (.161)	.218 (.202)
n	851		905		532	696	560	526
Mean of Dependent Variable	.880		.924		.823	.864	.873	.901
$F\left(\frac{X\beta}{\sigma}\right)$	.901		.932		.840	.891	.899	.928

<sup>a</sup> Sample size too small to permit estimation of probit.

Note: model specification (iii) is as given in Table 1.

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