Letters to the Editor

The Journal welcomes letters to the editor. If found suitable, they will be published as space allows. Letters should be typed double-spaced, should not exceed 400 words, and are subject to abridgment and other editorial changes in accordance with Journal style. All letters that reference a recently published Journal article are sent to the original authors for their reply. If no reply is published, the authors have not responded by date of publication. Send letters to Paul M. Fischer, Editor, The Journal of Family Practice, Department of Family Medicine, Medical College of Georgia, Augusta, GA 30912, or Fax (706) 855-1107.

ANALYSIS OF DATA

To the Editor:

The study by Bluestein and Rutledge on delayed pregnancy testing among adolescents[1] raised an important question. The authors were careful in selecting potential and known confounders related to delayed pregnancy testing among teenagers. The authors correctly noted that causality cannot be based on this prevalence study. The determinants of delayed pregnancy testing will require additional research using a different study design with adequate sample size and control for confounders.

The authors used Pearson correlations to evaluate the initial associations between delay in pregnancy testing and the many independent variables. This is incorrect since the Pearson product-moment correlation coefficient requires that variables be measured on a numerical scale, have a bivariate normal distribution and do not have outliers.[2,3] Most of the independent variables are categorical (nominal or ordinal) measures. Spearman's rank correlation could be used if the dependent and independent variables are both ordinal.[2] The correlation coefficient between two dichotomous variables is the phi coefficient, and can be used in some circumstances.[3]

In their multiple linear regression the authors used ostensibly "significant" results; however, the actual associations between delay in pregnancy testing and the independent variables cannot be known from their initial "correlations." Very different results will probably occur when proper methods are applied to these variables. ANOVA, Mantel-Haenzel chi-squares using stratification, and other methods are available that can better estimate association. The problem of "multiple comparisons" must also be addressed to ensure the proper level of significance.

P. Anthony Chapdelaine, Jr, MD, MSPH
Sharon A. Maxwell, MS
Meharry Medical College
Nashville, Tennessee

References

1. Bluestein D, Rutledge CM. Determinants of delayed pregnancy testing among adolescents. J Fam Pract 1992; 35:406–10.
2. Dawson-Saunders B, Trapp RG. Basic and clinical biostatistics. Norwalk, Conn: Appleton & Lange, 1990; 133:162–6.
3. Kleinbaum DG, Kupper LL, Morgenstern H. Epidemiologic research. New York: Van Nostrand Rheinhold Co, 1982: 152–3.

The preceding letter was referred to authors Rutledge and Bluestein, who respond as follows:

We would like to thank Chapdelaine and Maxwell for their comments on the statistical methodology used in our recent paper.[1] Per their suggestion, we reran our analyses using Spearman rank correlations, but found no difference in our final results. The variables that were intercorrelated at statistically significant levels using Pearson correlations remained significant using Spearman correlations, while two additional variables (ability to pay and regular menses) became significantly associated with the outcome, but only marginally so. A new regression analysis including these additional variables resulted in no change in our findings: difficulty in admitting to pregnancy was still the only significant predictor of delay in pregnancy testing.

Chapdelaine and Maxwell stated that rerunning our analyses using Spearman coefficients would be technically correct and likely lead to different results. They also suggested that, in lieu of multiple regression, we consider instead analysis of variance, a Mantel-Haenzel-stratified chi-square test, or other methods to estimate associations. Based on the robustness of assumptions underlying regression analysis and practical experience with multivariate statistics in analyzing multifactorial models based on survey data, we believed that our analyses were satisfactory.

When using a collection of variables representing a mix of measurement levels (eg, ordinal and interval, as in this study), Pearson correlations are fairly robust. At worst, they provide a more conservative estimate of association since standard errors may be slightly high. As for Spearman correlations, this statistic is of questionable value. A Monte Carlo simulation by the authors of the popular PRELIS statistical program,[2] used for covariance-structure modeling, found that Spearman correlations provided an inferior estimate of the true correlation, rho—almost as bad an estimate as obtained through Kendall's coefficient. As a result, they did not even include the Spearman correlation matrix as an option in the most recent version of PRELIS.

Evidence from both simulations and actual covariance-structure models incorporating ordinal data suggests that with a large enough sample, one's choice of Pearson, tetrachoric, or polyserial correlations, and estimation by maximum likelihood, generalized least squares, or a weighted estimator, ultimately makes little if any difference in the value of parameters of association (eg, betas or *r*'s).[3,4] Dr Levin recently conducted an analysis using a mix of interval- and ordinal-level variables to predict dimensions of a scale composed of solely ordinal indicators.[5] He first did things "correctly" (polyserial and asymptotic correlations, weighted least squares, etc), and then reran his analysis using defaults (Pearson correlations and maximum likelihood). With the exception of lower standard errors for the former analyses, there were virtually no differences in the parameter estimates, so for purposes of simplicity and clarity he reported the latter analyses. The moral is that, in many circumstances, regression and correlation analysis is quite robust with respect to violations of assumptions regarding the distribution of variables. For these reasons, we believe our approach to the data analysis is justified.

Carolyn M. Rutledge, MS, CFNP
Daniel Bluestein, MD, MS
Jeffrey S. Levin, PhD, MPH
Department of Family and Community Medicine
East Virginia Medical School
Norfolk

References

1. Bluestein D, Rutledge CM. Determinants of delayed pregnancy testing among adolescents. J Fam Pract 1992; 35:406–10.
2. Joreskog KG, Sorbom D. PRELIS: a program for multivariate data screening. 2nd ed. Mooresville, IN: Scientific Software, Inc, 1988.
3. Huba GJ, Harlow LL. Robust structural equation models: implications for developmental psychology. Child Dev 1987; 58: 147–66.

continued on page 263

continued from page 259

4. Johnson DR, Creech JC. Ordinal measures in multiple indicator models: a simulation study of categorization error. Am Sociol Rev 1983; 48:398–407.
5. Levin JS, Liang J. A measurement model of the general well-being (GWB) scale. Presented at the 44th annual scientific meeting of the Gerontological Society of America, San Francisco, November 24, 1991.

CRYOSURGERY

To the Editor:

With regard to the Technology Review on cryosurgical equipment (*Ferris DG, Ho JJ. Cryosurgical equipment: a critical review. J Fam Pract 1992; 35:185–93*) I'd like to make some comments. In the family practice program where I am a third-year resident, we have available both nitrous oxide (Cabot) and liquid nitrogen (Brymill) guns. There is a unanimity in our practice that, for skin lesions, the liquid nitrogen spray is more convenient and effective and has a wider range of dermatological uses than nitrous oxide.

We have found that the liquid nitrogen gives a faster freeze, which cuts down on the time of irritation or pain; allows a painting of various conditions (eg, acne, melasma) for which a probe is not suited; and yields a much better cure rate on common verrucae as well as penile or anal lesions with the liquid N_2 spray as opposed to the N_2O probe.

Every dermatologist I know has a liquid nitrogen system. I recommend anyone interested in this to look at *Cryosurgery in Dermatology: A Useful Office Technique*. Video tape #435, Network for Continuing Medical Education Series. (Available from the Network for Continuing Medical Education, 1111 Secaucus Rd, Secaucus, NJ 07094. Also available in most medical college libraries.)

Colin Elliott, MD
Saginaw Cooperative Hospitals, Inc
Saginaw, Michigan

The preceding letter was referred to Dr Ferris, who responds as follows:

I appreciate the comments made by Dr Elliott concerning cryosurgical equipment. Our article focused on gynecologic applications of cryosurgical equipment and was not intended to fully describe dermatologic applications. Both liquid nitrogen and nitrous oxide cryosurgical units are frequently used for dermatologic treatment of various skin conditions. Liquid nitrogen is certainly an excellent cryogen; however, it must be kept in a double-walled, vacuum-insulated container. Liquid nitrogen evaporates at a rate of between 1.5% and 8% per day depending on the size and quality of the container (*Rand RW, Rinfret AP, von Leden H, eds. Cryosurgery. Springfield, Ill: Charles C. Thomas, 1968*). In contrast, nitrous oxide can be kept in a closed container system, which minimizes continual gas depletion. Therefore, nitrous oxide units may be preferable for family physicians, particularly in settings where cryosurgical treatments are performed infrequently.

Daron G. Ferris, MD
Department of Family Medicine
Medical College of Georgia
Augusta

COMPUTER-BASED MEDICAL RECORDS

To the Editor:

The article on computer-based medical records (*Ornstein SM, Oates RB, Fox GN. The computer-based medical record: current status. J Fam Pract 1992; 35:556–65*) was a great introduction to the subject. Using WordPerfect on a laptop computer, I have been typing my progress notes in the examination room since February.

One tremendous unknown in this move to the computerized patient record (CPR) is the impact on the patient-physician relationship. Even with future technology, the CPR will require us to interact with the computer in front of patients. Even if we dictate for later entry, we must still consult the system for previous entries and results while in the patient's presence.

In my practice, patients have complained that I seem to be paying more attention to the computer than to them, in spite of typing the entire encounter without once losing eye contact.

I was glad to read that Dr Oates is in the process of studying patient acceptance of this technology. We are also in the middle of a study on patient reactions to the computer. I am hoping to enlist others who may be using a computer in the examination room to join us in this research. With all of the changes occurring in medicine, we do not need any additional factors interfering with the physician-patient relationship.

Gilbert L. Solomon, MD
Canoga Park, California

The preceding letter was referred to Dr Ornstein, who responds as follows:

I appreciated the comments of Dr Solomon and his interest in studying patient acceptance of the computerized patient record (CPR). A recent study by Rethans et al (*Rethans JJ, Hoppener P, Wolfs G, Diedriks J. Do personal computers make doctors less personal? BMJ 1988; 296: 1446–9*) has addressed this subject among patients in a group practice in the Netherlands. The vast majority of respondents had favorable opinions about the CPR and its impact on the doctor-patient relationship. Additional studies among patient populations in this country are needed. Qualitative research methods may be well suited to this type of investigation.

Steven M. Ornstein, MD
Department of Family Medicine
Medical University of South Carolina
Charleston

DENTAL CARE

To the Editor:

I read with interest the recent articles on dental care in family practice[1,2] by Park et al.

I think the articles should have included the American Academy of Pediatrics nutritional guidelines on fluoride[3] in Part I of this series as the readers need to be reminded once again of the importance of fluoride use in the prevention of dental caries.

These guidelines indicate that infants from birth until 1 year of age should be consuming approximately 0.25 mg of fluoride each day; from ages 1 to 3 years, 0.5 mg; and after 3 until 16 years, 1 mg per day. If the water is fluoridated at 1 part per million, that will provide 1 mg of fluoride per liter of water. This means that infants up to age 1 year who are breast-feeding or consuming undiluted liquid formula should consume at least 8 oz of fluoridated water per day to obtain the 0.25 mg of the required amount of fluoride.

We know from experience that most infants do not take in 8 oz of fluoridated water daily unless they drink powdered

milk mixed with fluoridated water. Furthermore, what we have learned from our own practice is that the amount of fluoride in fluoridated municipal water supplies changes from season to season, being less during the summer months. We also have discovered here in Lancaster that even in the winter months, the amount of fluoride in the water is less than 1 part per million. We suspect that this is a common occurrence in other communities around the country. We think it is wise to have the amount of fluoride in the water supply checked from time to time.

Mineralization begins in utero; therefore, it is not an uncommon practice to start prescribing fluoride in the middle or last trimester of pregnancy. It should be remembered that most of the teeth are completely mineralized by age 8 years, and the wisdom teeth are not completely mineralized until the early teenage years. It is for this reason that we must keep in mind that our patients should be encouraged to maintain the 1 mg of fluoride intake up until age 16 years, when the second molars have erupted. Fluorosis or staining of the teeth is unlikely to occur unless the fluoride consumption is consistently greater than 3 to 4 mg per day.

Nikitas J. Zervanos, MD
Lancaster General Hospital
Lancaster, Pennsylvania

References

1. Park BZ, Kinney MB, Steffensen JE. Putting teeth into your physical exam. Part 1. Children and adolescents. J Fam Pract 1992; 35:459–62.
2. Park BZ, Kinney MB, Steffensen JE. Putting teeth into your physical exam. Part II. Adults. J Fam Pract 1992; 35:585–7.
3. Pediatric nutrition handbook. 2nd ed. American Academy of Pediatrics, Elk Grove, Ill, 1986:169–71.

A response to the preceding letter was received from the Council on Dental Therapeutics of the American Dental Association:

Dr Zervanos makes some interesting observations and statements regarding the effects of systemic fluorides. I believe that it would be informative to review some of the Council on Dental Therapeutics recommendations and some current thinking on this topic.

First, and this was not mentioned in the article by Park et al, the fluoride supplementation schedules of the American Academy of Pediatrics and the Council on Dental Therapeutics are almost identical. The difference is that the AAP guidelines indicate that supplements should be given until age 16 years, whereas the Council recommends until age 13 years. The Council's rationale for age 13 years is that by 13 years of age, all secondary teeth except third molars have erupted. Third molars erupt somewhere between 17 and 21 years of age.

An important point to remember is that the recommended amount of fluoride supplement depends on both the age of the child and the fluoride level of the drinking water. The AAP and Council guidelines indicate that the recommended dosage of fluoride supplement varies for water containing less than 0.3 ppm, 0.3 to 0.7 ppm, and more than 0.7 ppm fluoride.

Dr Zervanos states that it is not uncommon to start prescribing fluoride in the middle or last trimester of pregnancy. The Council is aware of several studies that have evaluated this practice. Although the collective findings of these studies indicate a possible benefit to the primary teeth of the offspring, the Council feels that the evidence is not sufficiently conclusive to warrant recommending the procedure. The equivocal benefit is probably due to the fact that there is no consistent pattern of fluoride concentration between maternal and fetal circulation.

Another question that often arises is whether infants who are exclusively breast-fed need fluoride supplements. Fluoride levels in human breast milk have been found to be less than 0.05 ppm. This concentration remains relatively constant regardless of drinking water and maternal plasma levels of fluoride. Therefore, infants who are solely breast-fed ingest considerably less fluoride than those receiving formula mixed in 1 ppm of fluoridated water, and supplementation should be considered.

Finally, Dr Zervanos states that fluorosis is unlikely to occur unless the fluoride consumption is consistently greater than 3 to 4 mg per day. In actuality, human and animal studies have shown that mild fluorosis can also occur with *consistent* consumption of less than 3 to 4 mg per day, and animal studies have shown that *occasional* consumption of fluoride above an equivalent human dose of 3 to 4 mg per day can cause mild fluorosis.

There are two take-away messages from this information. First, physicians and dentists need to know the level of fluoride in the child's drinking water in order to be able to prescribe appropriate levels of fluoride supplements for consistent consumption, with the age/dose guidelines being closely adhered to. Second, and just as important, parents need to be instructed not to make up for missed supplement doses by giving multiple doses the next time, because of the possibility of causing mild fluorosis due to the occasional consumption of higher than optimal levels of fluoride.

Clifford W. Whall, Jr, PhD
Council on Dental Therapeutics
American Dental Association
Chicago, Illinois