We strongly disagree with the distortion of our conclusions as reported in *Time* magazine.[4] However, Jelliffe and Jelliffe, well known for their outstanding contributions to the promotion of breast-feeding, accuse us of alarmism and sensationalism that are substantiated neither by our data nor by our conclusions. We consider ourselves "breast is best" supporters. However, we do not feel that this conviction implies that the issue of HIV transmissibility during nursing is an untouchable field of investigation. In addition, we consider the claim of Jelliffe and Jelliffe that the role of breast-feeding in HIV transmission is "at worst minuscule" deserves discussion. Table 1 shows the estimated number of HIV-infected infants born during 1990 in the United States and in Kigali, Rwanda. With conservative estimates in the Rwandan calculations, the table shows a grossly similar number of infected infants in the United States and in an African city with a population 625 times smaller (Kigali had a population of 400,430 in December 1990).[7] A substantial proportion of the HIV-infected infants in Rwanda (13 percent in our model) are likely to have acquired HIV infection postnatally from their seroconverting mothers. What could perhaps be viewed as "minuscule" in the American context is certainly not so in populations where HIV is spreading by heterosexual transmission at a rapid pace.

Table 1. Estimates of the Number of Newborns and Infants Maternally Infected by HIV in 1990 in the United States and in Kigali, Rwanda.

Location	Prenatal or Intrapartum Infections*	Postpartum Infections from Seroconverting Mothers	Total
United States	1800†	Minuscule?	1800
Kigali	1350‡	210§	1560

*Assuming a 30 percent transmission rate from mother to fetus or newborn.

†According to Gwinn et al.[5]

‡Assuming a 29 percent rate of HIV seroprevalence in pregnant women[6] and according to age-adjusted estimates of fertility rates.[7,8]

§Assuming the minimal estimated annual postpartum seroconversion rate of 5 percent in mothers from Kigali and the lower estimate of the postnatal transmission rate from seroconverting mothers (36 percent).[9]

We fully agree with Williamson et al. that in situations where no safe alternative to breast-feeding can be recommended, maximal efforts should be made to promote safer sex. Indeed, the realization that there is a real risk of transmission of HIV to their infants should they seroconvert might stimulate mothers to adopt safer sexual behavior. In some African cities, such as Kigali, counseling and testing are now a widely used strategy, although not mandatory for pregnant women.

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PSYCHOLOGICAL STRESS AND THE COMMON COLD

To the Editor: The conclusion of Cohen et al. (Aug. 29 issue)[1] that stress increases infection rates after an experimental rhinovirus challenge cannot be accepted on the basis of the results presented. Persons with low or absent titers (≤2) of serum neutralizing antibody almost always become infected after an intranasal rhinovirus challenge. In our trials over the past 13 years, 321 of 343 susceptible (i.e., with titers ≤2) control subjects (94 percent) became infected after a challenge.[2-15] In the study of Cohen et al., infection rates in the antibody-free subjects with high and low scores on the psychological-stress index were comparable (86 and 92 percent, respectively). That infection was not documented in a small percentage of susceptible subjects from the two laboratories could be due in part to technical reasons, such as the inaccuracy of screening antibody tests, poor mixing of viral inoculums, and failure to detect viral shedding in persons who were actually infected. The very high infection rate indicates that nonimmune persons, regardless of their level of psychological stress, lack protective mechanisms in the nose for dealing with rhinovirus.

Also, increasing levels of serum antibody are associated with increasing degrees of protection, as evidenced by a direct relation between prechallenge antibody titers and resistance to infection after inoculation with increasing concentrations of virus.[16-18] At the Common Cold Unit in England, it was the practice to enroll volunteers who had all levels of serum antibody before a challenge. A limitation of this study is that the important confounding effect of different prechallenge antibody levels was treated as a dichotomous variable. No attempt was made to stratify subjects according to these levels. It is possible that prechallenge stress levels influence infection rates in partially immune subjects, but this can only be addressed by reanalyzing the data and taking into account the different levels of prechallenge antibody. This would best be done for the rhinovirus group alone. Immunity to coronavirus and respiratory syncytial virus may be different.

In the susceptible volunteers we studied, only 74 percent of the persons who became infected after a rhinovirus challenge met the criteria[19,20] for a cold; the remainder had subclinical infections. If prechallenge stress has an important effect on rhinovirus colds, we believe it is more likely that it influences the development of illness rather than the acquisition of infection.

Also, were these experiments done as a part of studies conducted for other purposes? This may not necessarily bias the results, but if it is the case, it should be indicated.

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To the Editor: Cohen et al. report an increased incidence of infection after the nasal administration of respiratory viruses in subjects with psychological stress. Although the cause of this effect was undetermined, both the authors and Swartz in an accompanying editorial[1] suggest that the mechanism is probably a systemic immunologic abnormality.

A more likely explanation, I think, is the cholinergically mediated nasal response to stress. Unlike the simple emotional response to a threat, stress is usually characterized by conflict, with feelings of hostility, guilt, resentment, humiliation, and the like. Wolf examined and performed biopsies of the nasal mucosa of persons in emotional conflict and documented hyperemia, edema, hypersecretion, and nasal obstruction.[2] The opposite findings, vasoconstriction with shrinkage of the mucosa and clear nasal breathing, were noted with strong emotions unassociated with conflict[3] — the "fight or flight" response mentioned by Swartz.[1]

The relation of stress to depression and unexplained fatigue is well known. Of 297 consecutive patients from my practice of general internal medicine, 49 percent noted a sense of nasal obstruction in either nostril after the occlusion of the contralateral nares. One or more of the criteria for depression listed in the *Diagnostic and Statistical Manual of Mental Disorders* (third edition) were noted in 43 percent of all the patients.[4] Nasal obstruction, however, was reported by 57 percent of these patients, as compared with 25 percent of those without symptoms of depression ($P<0.02$). Similarly, 75 percent of those with unexplained fatigue had nasal obstruction, as compared with 32 percent without unexplained fatigue ($P<0.001$).

Stress is clearly associated with changes in the nasal mucosa. The swollen nose is more vulnerable to infection.[5] The resultant changes in air flow, ciliary movement, and local membrane defenses may explain the findings of Cohen et al. better than a systemic immunologic abnormality.

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The authors reply:

To the Editor: Drs. Gwaltney and Hayden indicate that they do not believe that the relation between psychological stress and colds reported in our article is attributable to infection. We think that they are misinterpreting the data.

First, they speculate that anyone without viral-specific antibody should become infected when experimentally challenged with a rhinovirus and that failure (in both their work and ours) to identify infections in persons who are seronegative before a challenge is probably attributable to technical inadequacies. This is certainly a reasonable speculation. However, it is equally plausible (and supported by our data) that there are individual characteristics that predict whether identifiable infections will develop in persons, even seronegative persons.

Second, they propose that the difference in rates of infection between those below and those above the median for stress in the seronegative group (86 percent and 92 percent, respectively) was small. However, there was no reliable interaction between stress and serologic status. This indicates that the relation between stress and infection was similar in seropositive and seronegative subjects. Let us assume (as Drs. Gwaltney and Hayden suggest) that there was no real association between stress and infection in seronegative persons. Even if this were so, the relation between stress and infection among seropositive persons remains substantial (infection rate, 67 percent for low stress and 80 percent for high stress), as does the association when seropositive and seronegative subjects are combined. In sum, contrary to their argument, there is convincing evidence for the importance of infection in the relation between stress and colds. We do agree, however, that there are plausible mechanisms through which stress might influence the development of symptoms among infected persons, and we continue to pursue this possibility in our own work.

Drs. Gwaltney and Hayden are also concerned that when the prechallenge viral-specific antibody level is measured continuously, it might be associated with psychological stress. We examined correlations between the psychological-stress index and each of the specific antibody measures (serum IgG, IgA, and neutralizing antibodies for rhinoviruses), both for the three rhinoviruses combined and separately for each of the five viruses. None of the 16 resulting correlations even approached statistical reliability. The average ($\pm$SD) correlation was -0.06 (± 0.07).

Finally, Drs. Gwaltney and Hayden ask whether our studies were

part of studies conducted for other purposes. The answer is yes, but the subjects in our analysis were control subjects — i.e., those receiving placebo or no treatment.

Dr. Chester suggests that our results may be attributable to stress-induced changes in the nasal mucosa rather than to systemic changes in immunity. We agree that mucosal changes provide a plausible alternative explanation, and we thank Dr. Chester for documenting this work.

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EMERGENCY BALLOON VALVULOPLASTY AS INITIAL TREATMENT OF PATIENTS WITH AORTIC STENOSIS AND CARDIOGENIC SHOCK

To the Editor: Patients who have aortic stenosis with severe heart failure and cardiogenic shock usually do not survive. Since it has been shown that balloon aortic valvuloplasty may transiently improve severe left ventricular dysfunction in patients with aortic stenosis,[1-4] we performed emergency balloon valvuloplasty in 10 patients with aortic stenosis (8 men and 2 women) who had cardiogenic shock refractory to intensive medical therapy. Valve replacement was considered to have a prohibitive risk.

The mean (±SD) age of the patients was 64±9 years (range, 54 to 79). Seven patients had coexisting cardiac or noncardiac illnesses: previous myocardial infarction in two patients and chronic lung disease, cirrhosis of the liver, psychiatric disorder, pulmonary infection, and sepsis in one patient each. Valvuloplasty catheters (Boston Scientific) with a maximal balloon size of 20 mm (three patients) or 23 mm (six patients) were used. The double-balloon technique[5] (balloon sizes of 18 and 20 mm) was used in one patient. Three full inflations were performed and maintained for 10 to 20 seconds.

The hemodynamic results of balloon valvuloplasty are shown in Table 1. The mean (±SD) left ventricular ejection fraction, as assessed by angiography, was 25±6 percent before the procedure. Transient electromechanical dissociation developed in one patient during the first balloon inflation, requiring a few seconds of cardiac massage. There were no other complications, and there were no deaths during this procedure. Dyspnea improved immediately. Signs of shock disappeared within hours.

One patient had restenosis four days after the procedure, with recurrence of shock. He died during a repeat catheterization. A second patient died three weeks later of gastrointestinal bleeding. Two patients, 68 and 79 years old, refused valve replacement and were alive without symptoms of heart failure at 48 and 24 months, respectively. Six patients had uneventful aortic-valve replacement an average of 5 months after valvuloplasty (range, 6 days to 18 months). Repeat catheterization was performed an average of 5.6 months after valvuloplasty in the two patients who refused surgery and before surgery in five of the six patients who subsequently had aortic-valve replacement. The results consistently showed sustained hemodynamic improvement: the left ventricular ejection fraction had increased to 46±16 percent, pulmonary wedge pressure to 17±10 mm Hg, cardiac index to 3.4±0.7 liters per minute per square meter, mean aortic-valve gradient to 47±16 mm Hg, and aortic-valve area to 0.9±0.2 cm^2.

Thus, balloon valvuloplasty can be lifesaving in patients with aortic stenosis and cardiogenic shock. When there is hemodynamic and clinical improvement and the risk of surgery has been lowered, aortic-valve replacement can be performed and offers a chance of long-term survival for these severely ill patients.

Table 1. Hemodynamic Measurements before and Immediately after Valvuloplasty in 10 Patients.

MEASURE	VALVULOPLASTY		P VALUE*
	BEFORE	AFTER	
	mean ±SD		
Aortic blood pressure (mm Hg)	71±8	80±14	NS
Pulmonary-capillary wedge pressure (mm Hg)	33±6	25±7	<0.02
Cardiac index (liters/min/m^2)	1.90±0.34	2.30±0.40	<0.05
Aortic transvalvular gradient (mm Hg)	54±19	28±14	<0.01
Aortic-valve area (cm^2)	0.47±0.10	0.95±0.30	<0.001

*NS denotes not significant.

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INSULIN RESISTANCE

To the Editor: I compliment Drs. Moller and Flier (Sept. 26 issue)[1] on their clear review of the mechanisms underlying insulin resistance. However, they failed to mention the possibility that insulin resistance may also have a hemodynamic origin. Insulin flux across the capillaries may be a rate-limiting step in the action of insulin.[2] The amount of glucose and insulin that reaches the target tissue (i.e., muscle cell) is dependent on the intercapillary distance or the capillary density of the tissue, and glucose oxidation correlates negatively with capillary density.[3] Furthermore, insulin increases blood flow to muscles in a dose-dependent manner.[4] In patients with either non-insulin-dependent or insulin-dependent diabetes mellitus,[5,6] as well as in obese subjects,[4] the increase in blood flow after insulin administration is less than in normal subjects. This smaller vasodilative response to insulin could be due to a smaller amount of glucose being metabolized by the cells (as a result of receptor or postreceptor defects) and thus to a diminished autoregulatory signal from the cells to their nourishing capillaries. Alternatively, the smaller response could be due to resistance to a direct vasodilative action of insulin, so that the amount of insulin and glucose delivered to the target-tissue cells is decreased.

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