avoid prescribing the high-dosage preparations which are most likely to cause troubles, anyway.

Incidentally, can anyone explain why it should be the oestrogen component that is thought to be responsible for thrombotic and embolic phenomena? It is common clinical knowledge that women are less liable than men to coronary thromboses while their oestrogen-levels are high in the reproductive phase of life, and that embolic episodes are most commonly associated with pregnancy and childbirth, i.e. high progestogen-levels. It therefore seems paradoxical to blame oestogens in oral contraceptives. I know about the statistical evidence, but where statistics and common-sense contradict each other I have no faith in statistics and want proper biochemical evidence to convince me.

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SCHIZOPHRENIA AND SEASON OF BIRTH

DEAR SIR,

Hare and Price (1969) report that there is a significant difference between the seasonality exemplified in the month of birth of 3,596 schizophrenic patients and of 14,076 neurotics admitted to the Maudsley Hospital during the period 1951–63. They suggest that the neurotic patients may be regarded as controls, and that therefore the month of birth of schizophrenic differs from that of the general population on the average.

The Registrar General (1961) has given quarterly live birth ratios expressed as percentages of the yearly ratios for each decade from 1841 to 1930 and each quinquennium since. Hare and Price give the age distributions of their neurotics and schizophrenics by five-year intervals. These patients were admited over a period of years centred on 1957. So one can calculate rough distributions by five-year intervals of the year of birth for these schizophrenics and neurotics. It is thus possible to compute the frequencies in each diagnostic category that would be expected to have been born in each quarter of the year on the hypothesis that there is no seasonality in the births of patients as compared with the population. For instance, during the decade 1891–1900 the ratios for the four quarters were 102, 102, 99 and 97. The numbers of days in the four quarters are 90·25, 91, 92 and 92. So the numbers of births in these quarters in that decade were in the ratios of $102 \times 90{\cdot}25 : 102 \times 91 : 99 \times 92 : 97 \times 92$. The four values shown in the top row of Table I distribute the 180 births for that decade in the ratio of these four numbers. The bottom rows of that Table give the numbers of schizophrenics observed to have been born in the four quarters and the numbers expected on the hypothesis that their births are distributed like those of the general population.

TABLE I

Frequencies of Schizophrenic Births in Each Quarter among Hare and Price's Sample of 3596, Observed and Expected on the Hypothesis that Schizophrenic Births are Distributed like the Births of the General Population

Interval	N	Quarter 1	2	3	4
1891–1900	180	45·37	45·75	44·89	43·98
1901–1910	360	90·75	92·40	90·69	86·16
1911–1920	647	164·69	164·45	161·37	156·48
1921–1930	1115	281·08	291·75	280·91	261·26
1931–1935	647	161·50	170·91	164·64	149·96
1936–1940	467	115·41	123·35	120·00	108·24
1941–1945	180	44·48	46·65	44·89	43·98
Expected	3596	903·28	935·26	907·39	850·06
Observed	3596	925	938	840	893

It will be seen that this technique too suggests that there may be a winter excess of schizophrenics—but it suggests that the peak incidence of schizophrenic births is in the fourth quarter rather than the first, as suggested by Hare and Price. It is not easy to interpret these data. If one tests the frequency of schizophrenic births in the fourth quarter against the sum of the frequencies in the other three, it is not significant. A χ^2 test of the frequencies in the two winter quarters (4 and 1) against the frequencies in the two summer quarters (2 and 3) is just significant at the ·05 level. However, there are reasons for questioning the mild suggestion provided by this result. As far as I know, only one set of published data (one of the distributions offered by Huntington, 1938) agrees with Hare and Price's material in indicating an autumn peak. Instead, most of the previous studies (Barry and Barry, 1961; Dalen, 1968; de Sauvage Nolting, 1934, 1954; four of the five distributions of Huntington, 1938; Laestadius, 1949; Lang, 1931; Norris and Chowning, 1962; Tramer, 1929) conclude that there is a preponderance of schizophrenic births in the spring; while a few others (Barry and Barry, 1964; Petersen, 1934; Pasamanick and Knobloch, 1960) have failed to detect seasonality.

I would conclude that the present data do not give as much support to the hypothesis of seasonality

in schizophrenic births as Hare and Price's presentation might seem to suggest.

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FLUPENTHIXOL (FLUANXOL)

DEAR SIR,

In 1969, in this *Journal* (Vol. 115, pp. 1399–1402), Reiter reported on his uncontrolled impression of this drug in the treatment of affective illness. He considered that it was liable to cause only minimal side effects and that it had an antidepressant action which was very quickly apparent. I have tested these assumptions in 59 patients from May to December, 1970. Fifty-three of the patients have diagnoses of an affective disorder, and in 12 of them the illness was regarded as an endogenous pattern of depression. I have also given the drug to many more patients since these initial 59. I found a worthwhile sustained improvement in 24 of the 53 patients. I should emphasize that all these patients had relatively chronic illnesses and had had previous treatment with tricyclic antidepressants and in some instances MAOIs and ECT as well.

Side effects were minimal, as Reiter claims, the main ones being occasional constipation and mild drowsiness. My results are so similar to those of Reiter that it would not be worth while to describe them further in any detail. I came to the same conclusions as he did and regard flupenthixol as a most interesting and potentially useful antidepressant. In a series not selected for chronicity I would anticipate a better response rate than here, and I venture to predict that controlled trials, when undertaken, will show it to be an active drug. The main difficulty in organizing a controlled trial for this substance is that it acts so quickly that it is unsuitable to make a direct comparison of it with drugs which require a month to work.

I wish to thank Dr. W. T. Simpson of Lundbeck Research for supplies of flupenthixol.

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CHLORIMIPRAMINE IN THE TREATMENT OF SEVERE DEPRESSION

DEAR SIR,

In your issue for August 1970, (Vol. 117, p. 211) Collins reports few side effects in the treatment of depression by intravenous chlorimipramine, given by drip infusion. I wish to comment on a hitherto unreported side effect in connection with this relatively new anti-depressant.

Case. Female, age 52 in depressive phase of manic-depressive psychosis. She responded well to intravenous drip infusion of chlorimipramine and was discharged after five days. Whilst returning home by taxi, her whole body began to shake so on arrival home she immediately called her general practitioner. He came within minutes and found her almost completely paralysed, while there was intense coarse shaking of all her limbs. He contacted me and I suggested she be given benzhexol 5 mg. orally. After 15 minutes the paralysis and shaking had disappeared. She completed her recovery on oral chlorimipramine and benzhexol.

Since this time I have seen several patients who while receiving oral chlorimipramine have reported uneasiness and shaking of the hands. These side effects have been alleviated by benzhexol. These symptoms and their alleviation suggest an extra-