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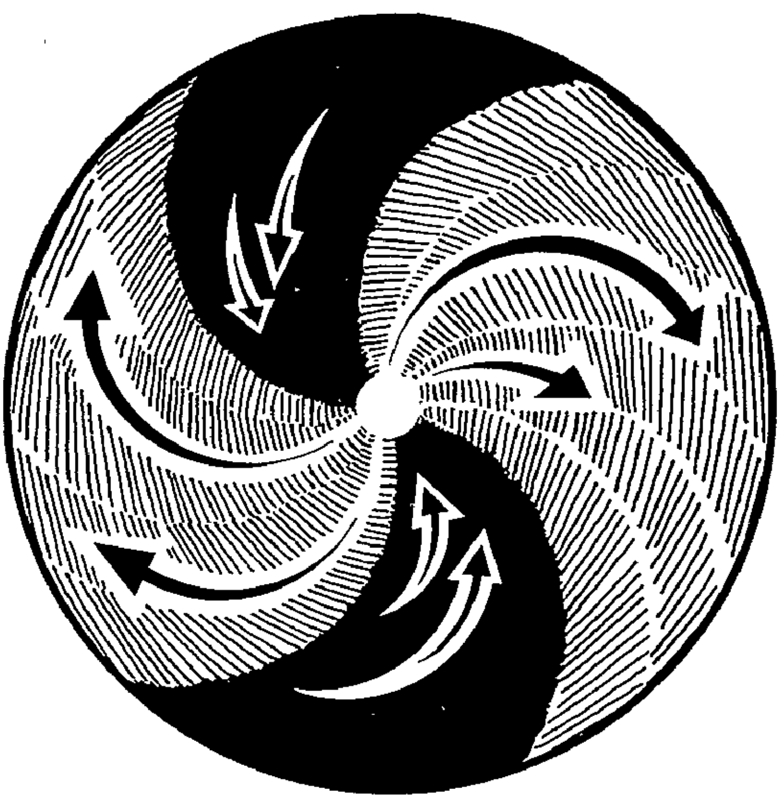
Interplanetary Medium Data Book



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Interplanetary Medium Data Book

by

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September 1977

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INTRODUCTION

Many unresolved questions on the physics of the solar wind and its effects on magnetospheric processes and cosmic ray propagation can be addressed with hourly averaged interplanetary plasma and magnetic field data. A wealth of such data has been accumulated for almost two decades. Recently, much of these data have been assembled onto a single magnetic tape available from NSSDC.

The purposes of this report are: (1) to describe this composite data set - its content and extent, its sources, its limits of validity, and the mutual consistency studies and normalizations to which the input data were subjected, and (2) to present in the form of digital listings and 27-day plots hourly (or 3-hourly) averaged parameters. The listings are contained in the separately bound Appendix to this Data Book.

DATA CONTENTS AND COVERAGE

The composite data set contains: (1) interplanetary magnetic field (IMF) vector data in geocentric solar ecliptic (GSE) and geocentric solar magnetospheric (GSM) coordinate systems, (2) interplanetary plasma parameters, and (3) geomagnetic (K_P and C_9) and solar (sunspot number R) activity indexes. The interplanetary field and plasma data were all obtained by spacecraft in geocentric or selenocentric orbit when those spacecraft were outside the Earth's bow shock. The identifications of interplanetary periods for these spacecraft were made by the experimenters who supplied the data to NSSDC; these identifications are occasionally difficult to make. The geomagnetic and solar activity indexes were taken from a compilation prepared and periodically updated by the European Space Agency's European Space Operations Center and are described in *Lenhart* (1968).

The field parameters consist of field magnitudes, cartesian components, direction angles, and certain standard deviations. The plasma parameters consist of bulk flow speed (V), proton density (N), proton temperature (T), flow direction longitude (ϕ_V) and latitude (θ_V), and certain standard deviations (σ). As is detailed below, not all the plasma parameters were contained in each source data set. Thus, for some hours of the composite data set, only a subset of the identified plasma parameters are given.

The basic unit of time for the composite data set is 1 hour. All field data and much plasma data were available in the form of hourly averages. For those source plasma data sets, identified below, in which only 3-hour values are available, the 3-hour values were assigned to each of the 3 hours of the averaging interval. The 3-hour K_P index and the daily C_9 and R indexes were treated similarly. For example, a given value of C_9 is repeated in 24 successive hourly records on the composite magnetic tape.

Although the details of data merging are given later, it is useful to note here the general outline of the procedure followed. All the source plasma data sets were combined onto a single, time-ordered magnetic tape. For any hour, there were data given separately from up to five sources. A similar composite IMF tape was also generated, with separate data from up to three spacecraft for each hour. A normalized composite plasma tape was generated from a slightly edited version of the tape with the unnormalized experimenter-supplied data. The composite IMF tape, the normalized composite plasma tape, and the tape containing the solar and geomagnetic activity indexes were merged to yield the final composite tape. The plasma parameters contained on the final tape for a given hour were selected from one of the possibly several sources available for that hour. Field parameters were selected in a similar manner. Each of the tapes involved in the preparation of the final composite tape is available from NSSDC.

The percent of coverage of the composite data set over the 1963 to 1975 time period is shown in Figure 1 for each Bartels' solar rotation number. Of the 106,920 hours included in Bartels' solar rotation 1783 to 1947, there are 45,399 hours with field and plasma data of which 23,613 hours have field and plasma data from a common spacecraft, 19,755 hours with field data only, 15,779 hours with plasma data only, and 25,987 hours with no interplanetary plasma or field data. The time intervals of field and plasma data are Nov. 27, 1963, to Oct. 28, 1975, and Nov. 27, 1963, to Dec. 30, 1975, respectively. Of the 61,178 hourly records with plasma data, 29,160 records actually contain 3-hour averages. It is contemplated that this composite data set will be updated as additional data become available.

DATA SOURCES

General

All the source spacecraft used in compiling this composite data set are identified in Table 1 in chronological sequence. Each spacecraft is assigned a numeric and an alphabetic identifier. The numeric identifier represents how a given spacecraft is specified on the magnetic tape and in tables and figures in this document. The alphabetic identifiers are used in the listing found in The Data Book Appendix. In Table 1, it is indicated whether plasma and/or field data from a given spacecraft are used in this compilation.

Plasma Data

The 11 source data sets from which the plasma data of the new composite data set were obtained are listed in Table 2. For each source data set, the spacecraft, the principal investigator and his institution, the averaging interval, the time span, the number of hours on the final

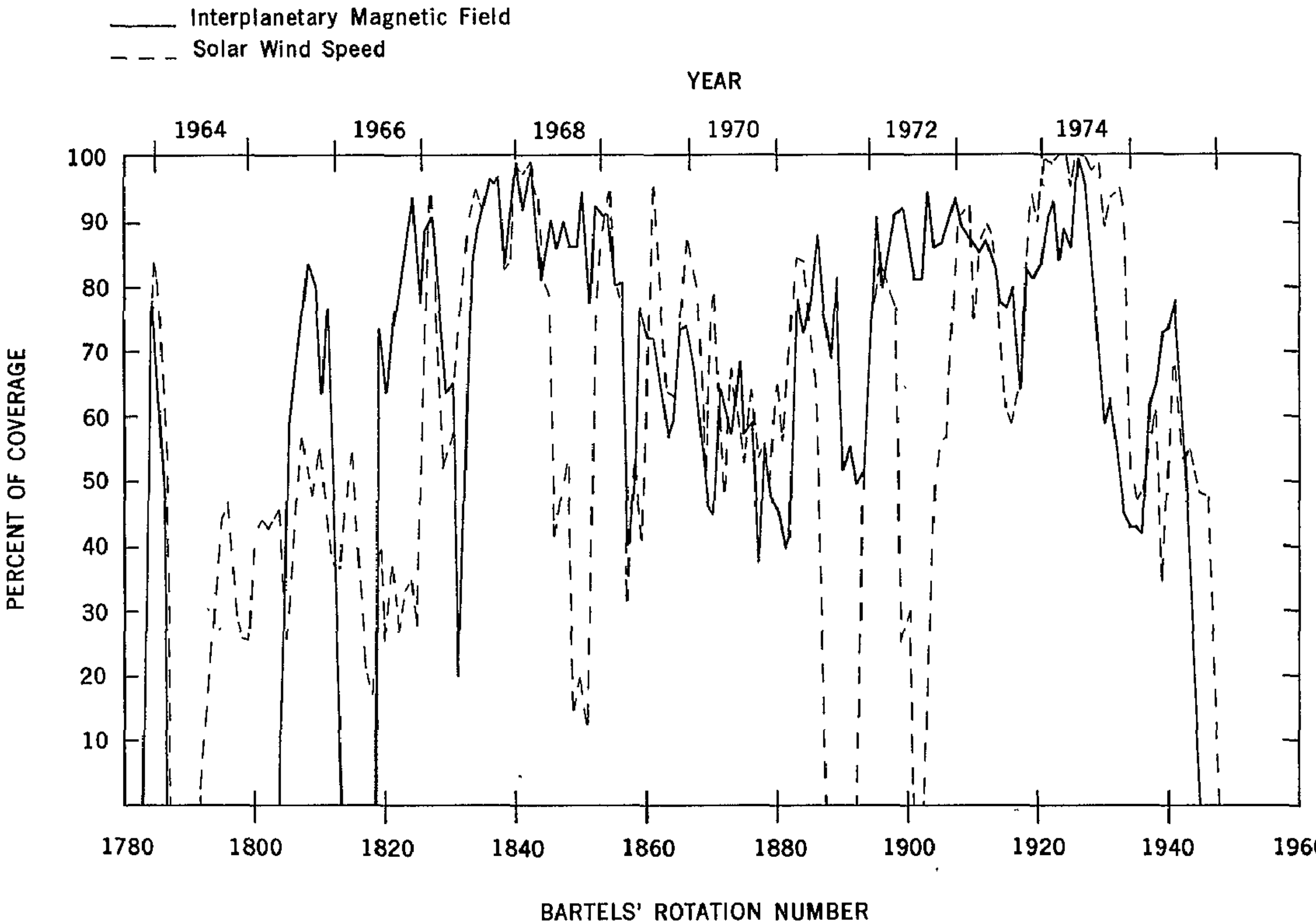

Figure 1. Composite Data Set Temporal Coverage

Table I. Spacecraft Providing Interplanetary Medium Data

Spacecraft	Numeric Identifier	Alphabetic Identifier	Plasma Data	Field Data
Explorer 18 (IMP 1, IMP A)	18	A	X	X
Merged Vela (Vela 2-6)	99	V	X	
Vela 3	3	V	X	
Explorer 28 (IMP 3, IMP C)	28	C		X
Explorer 33 (AIMP 1, IMP D)	33	D	X	X
Explorer 34 (IMP 4, IMP F)	34	F	X	X
Explorer 35 (AIMP 2, IMP E)	35	E	X	X
OGO 5	5	O	X	
HEOS	1	X	X	X
Explorer 41 (IMP 5, IMP G)	41	G		X
Explorer 43 (IMP 6, IMP I)	43	I	X	X
Merged IMP (IMP 6-8)	98	L	X	
Explorer 47 (IMP 7, IMP H)	47	H		X
Explorer 50 (IMP 8, IMP J)	50	J	X	X

Table 2. Source Plasma Data Set Characteristics

Spacecraft	P.I. (Institution)	Averaging Time (Hours)	Time Period	Number of Hours	T	N	V	ϕ_V	θ_V	σ_T	σ_N	σ_V	σ_ϕ	σ_θ
Explorer 18	Bridge (MIT)	3	11/27/63 - 2/22/64	1,485		20%	10%							
Merged Vela	Bame (LASL)	3	7/21/64 - 3/18/71	10,273			X							
Vela 3	Bame (LASL)	3	7/26/65 - 11/13/67	5,721	10%	25%	5%	1.6°		X	X	X	X	
Explorer 33	Bridge (MIT)	1	7/6/66 - 9/23/69	5,637	X	X	X	X	X	X	X	X	X	X
Explorer 34	Ogilvie (GSFC)	1	6/3/67 - 12/16/67	2,282	X	10%	3%			X	X	X		
Explorer 35	Bridge (MIT)	1	7/28/67 - 7/3/68	3,642	X	X	X	1.5°	.75°	X	X	X	X	X
OGO 5	Neugebauer (JPL)	1	3/5/68 - 4/29/71	2,564	15%	8%	1%	X	X					
HEOS 1	Bonetti (CNR, Italy)	3	12/11/68 - 4/15/70	3,142		X	X							
Explorer 43	Bame (LASL)	1	3/18/71 - 3/27/73	7,998	X	X	X							
Merged IMP	Bame (LASL)	3	3/18/71 - 12/31/74	8,539	15%	30%	2%							
Explorer 50	Bridge (MIT)	1	12/1/73 - 12/30/75	9,895	X	X	X	X	X	X	X	X	X	X

composite tape, and an identification of which plasma parameters were available are shown. The Vela 3 data set is a composite of data from the Vela 3A and Vela 3B satellites. The sources listed as Merged Vela and Merged IMP refer to data sets, generated by the LASL plasma physics team, that contain data from Velas 2, 3, 4, 5, and 6, and Explorers 43, 47, and 50 (IMPs 6, 7, and 8), respectively. Because the LASL Explorer 43 data set has a time resolution of 1 hour, it has been used (after normalizing densities and deleting some suspicious hours) rather than the 3-hour resolution Merged IMP data set, when possible. However, LASL personnel have normalized and edited their Explorer 43 data and have folded them into the Merged IMP data set.

Note that an "X" is used in Table 2 to indicate the availability of some parameters, while, for others, estimates of uncertainties found in the literature are given. During a discussion of the mutual consistency studies carried out in assembling this composite data set, questions of the levels of reliability of various parameters will be further discussed.

The number of hours, listed in Table 2, that each source data set contributes to the final composite data set is only a fraction of the available hours for that source data set. The fraction depends on the data selection priority scheme (discussed later) and the availability of simultaneous data. The fraction ranges from 40 percent for the Merged IMP set to 100 percent for the Explorer 18 and 50 sets.

The bulk plasma parameters of each source data set were determined by each experimenter group by averaging over fine-time scale values of these bulk parameters. (The number of such values contributing to each hourly or 3-hourly average is given on the final composite tape for all source data sets except those from Explorers 18 and Merged IMP.) Fine-time scale bulk parameters were derived from the spectral and directional distributions of sensor outputs along with sensor calibration information. An assumption of the nature of the governing particle distribution function (e.g., convected isotropic Maxwellian distribution) was also made. Generally speaking, fine-time scale plasma parameter derivation has improved with time as spacecraft telemetry rates have increased, thereby permitting improved temporal, spectral, and directional resolution of sensor outputs.

Because each experimental group providing data has generally used the same instrumentation repeatedly and the same parameter derivation technique, the following discussion is of the data sources grouped by institution.

All the Massachusetts Institute of Technology (MIT) data have been obtained with modulated grid split-collector Faraday cups. The basic theory of these instruments is discussed in *Bridge et al.* (1960). In the derivation of the parameters, it was assumed that the governing distribution was a convected isotropic Maxwellian or a convected isotropic Kappa distribution. The latter distribution is a variation of the former with a high energy tail. The Explorer 18 measurement sequence and some key results are discussed in *Bridge et al.* (1965), *Olbert* (1968), and *Egidi et al.* (1969). Explorer 33 details are given in *Lyon et al.* (1968), while Explorer 35 details are given in *Lyon et al.* (1967). For a discussion of the flow direction angle determination from Explorers 33 and 35, see *Egidi et al.* (1977).

All the Los Alamos Scientific Laboratory (LASL) data have been obtained using hemispherical electrostatic analyzers for energy-per-charge selection and an electron multiplier for particle counting. Typically, a convected bi-Maxwellian distribution has been assumed in the bulk parameter derivation. The single temperature contained in the LASL-supplied data sets is related to the perpendicular and parallel temperatures according to $T = 1/3(T_{\parallel} + 2\,T_{\perp})$. Further details on the LASL instruments and data are given in *Hundhausen et al.* (1967), *Gosling et al.* (1967), *Bame et al.* (1967) and *Hundhausen et al.* (1970) for Velas 2 and 3; *Montgomery et al.* (1970) and *Hones et al.* (1972) for Vela 4; *Bame et al.* (1971) for Velas 5 and 6; *Feldman et al.* (1973) for Explorer 43; and *Asbridge et al.* (1976) for Explorers 47 and 50. Discussions of the Merged Vela and IMP data sets are found in *Gosling et al.* (1976) and *Feldman et al.* (1976).

The Goddard Space Flight Center (GSFC) Explorer 34 plasma instrumentation consisted of a curved plate electrostatic analyzer for energy-per-charge selection, followed by a crossed electric field/magnetic field device (Wein filter) for velocity selection, followed by a particle counter. Plasma parameters were derived by taking moments of the observed distribution function. Further details on the instrumentation and data analysis are found in *Ogilvie et al.* (1968a), *Oglivie et al.* (1968b), and *Burlaga and Oglivie* (1968).

The Jet Propulsion Laboratory (JPL) OGO 5 plasma instrumentation consisted of a modulated grid Faraday cup and a curved plate electrostatic analyzer. Plasma parameters were determined iteratively by appropriately combining the outputs of the two sunward-looking sensor systems. Details are provided in *Neugebauer* (1970). Because of its greater reliability, the total charge density obtained from the Faraday cup flux, rather than the ion flux inferred from the electrostatic analyzer, is given in the new composite data set. It is of interest to note that, except for the attitude-stabilized OGO 5, all the spacecraft providing plasma data for the new composite data set were spin stabilized.

The Consiglio Nazionale delle Richerche (CNR, Italy) HEOS 1 plasma instrumentation consisted of a hemispherical electrostatic analyzer followed by a Faraday cup. A convected isotropic Maxwellian distribution function was assumed in the plasma parameter derivation. Details are provided in *Bonetti et al.* (1969). *Diodato et al.* (1975) have presented listings of 3-hour averaged bulk speeds and densities from Vela 3, Explorers 33, 34, and 35, and HEOS 1. The listed averages consist of combined data from as many spacecraft as were available for each 3-hour averaging period. Before averaging, the data for each data set were normalized to Vela 3 values, using the results of the *Moreno and Signorini* (1973) regression analysis. As input to our composite data set, only those Diodato-listed, 3-hour averages resulting from HEOS 1 only were taken, and they have been denormalized. That is, the inverse of the previously used normalization equations were applied.

Magnetic Field Data

The 10 IMF source data sets are listed in Table 3. All were provided by N. F. Ness and colleagues at GSFC, except the HEOS data set, which is a merged HEOS 1/HEOS 2 data set provided by P. C. Hedgecock of Imperial College, London. All the source data sets consisted of 1-hour averages obtained from fluxgate magnetometer data. All the magnetometers were triaxial except those on Explorers 18 and 28, which were biaxial. All but the Explorers 18 and 28 and HEOS magnetometers were flippable to assist in sensor zero-level determinations. See *Hedgecock* (1975a) for a discussion of zero-level determination in the absence of sensor flip capability. Sensor signal digitization resolution was typically between 0.1 and 0.2 gamma. Estimated upper limits of spacecraft magnetic fields at magnetometer locations (ends of booms) ranged from .5 gamma for early spacecraft to .1 gamma or less for recent spacecraft.

The parameters available in the source data sets consist of hourly averaged field cartesian components in solar ecliptic coordinates, the magnitude and direction angles of the field vector made up by these three average cartesian components, and the averaged field magnitude. For the HEOS data set, hourly averaged direction angles and the standard deviations in the averaged magnitude and direction angles were also given. For the Explorer data sets, standard deviations in the cartesian component averages and, for all but Explorers 33, 34, and 35, in the field magnitude average were also given. Field components in GSM coordinates were computed at NSSDC from GSE components, as will be discussed.

Hourly averaged values were constructed from fine-time scale field values (obtained either by measurement or by averaging yet finer scale data). The fine-time scale was 327 s for Explorers 18 and 28, 48 and 32 s for HEOS 1 and 2, and between 1 and 5 s for the remaining Explorers. The 327-s resolution field magnitudes are the magnitudes of field vectors made up of 327-s averaged cartesian components. Thus, field directional fluctuations with frequencies between 5 s and 327 s will cause Explorer 18 and 28 hourly averaged magnitudes to be somewhat smaller than corresponding averaged magnitudes based on 1- to 5-s resolution magnitudes.

Much of the IMF data of the new composite data set was already presented, in GSE components only, in *King* (1975). The present compilation supersedes that earlier document.

Table 3. Source Magnetic Field Data Set Characteristics

Spacecraft	Time Period	Number of Hours	Reference
Explorer 18	11/27/63 - 2/15/64	1,215	*Ness et al.*, 1964
Explorer 28	5/30/65 - 1/29/67	6,233	*Ness et al.*, 1964
Explorer 33	7/4/66 - 7/13/68	8,032	*Behannon*, 1968
Explorer 34	5/26/67 - 12/27/68	5,888	*Fairfield*, 1969
Explorer 35	7/26/67 - 11/10/69	2,825	*Ness et al.*, 1967
HEOS	12/11/68 - 10/28/75	15,139	*Hedgecock*, 1975b
Explorer 41	6/21/69 - 10/26/72	7,373	*Fairfield and Ness*, 1972
Explorer 43	3/13/71 - 7/21/74	8,690	*Fairfield*, 1974
Explorer 47	9/26/72 - 4/3/73	1,645	*Mish and Lepping*, 1976
Explorer 50	10/29/73 - 8/26/75	8,114	*Mish and Lepping*, 1976

MUTUAL CONSISTENCY

General

In the creation of the composite interplanetary medium data set, we have examined the mutual consistency of the source data sets. For the plasma data, consideration of regression analysis results and visual inspection of corresponding scatter plots yielded normalization equations that were applied to some of the experimenter-supplied parameter values. In this section, the regression analysis used is described and the results are discussed for plasma data and for field data. A series of sample scatter plots, found in the back sections of this document, are discussed, and the plasma parameter normalizations utilized are listed. The limits of accuracy of the various parameters in this composite data set are also discussed.

A linear regression analysis, in which equal random error is assumed in both variables, was applied to the simultaneously determined data of several pairs of spacecraft. See, for example, *Madansky* (1959) for details. This approach was chosen because both data sets do have random error in fact and because the unavoidable chaining of regression equations for spacecraft A/B and B/C to obtain A/C relations is more legitimate with this approach.

This approach is in contrast to the more often used approach that assumes no error in the "independent variable." See, for example, *Neugebauer* (1976) and *Moreno and Signorini* (1973). (The present data have been run through a no-error-in-the-independent-variable regression analysis, and regression parameter values were found that are more nearly similar to those of Neugebauer and of Moreno and Signorini than are the following parameter values.)

In the present analysis, the regression parameters a and b in the equation

$$P_{s1} = a\ P_{s2} + b$$

(where $s1$ and $s2$ denote the two source data sets, and P identifies the physical parameter) are determined in a way geometrically equivalent to minimizing the sum of squares of perpendicular distances between data points and regression line.

Before proceeding, it should be noted that, although differences between data sets are emphasized in the following discussion, the level of agreement is really very high considering that the data were obtained and processed at different times using differing instrumentation flown on various spacecraft by various principal investigators and their colleagues. The high levels of agreement attest to the skill and care with which the data were acquired and processed.

Plasma Data

The results of the regression runs for the logarithms of temperature and density and for the bulk speed are presented in Figures 2, 3, and 4. Logarithms were chosen for temperature and density because their distributions were more Gaussian than were the linear values of temperature and density. Each figure shows several regression lines and their equations. Also shown for each line is a slope range that would correspond to 95 percent confidence limits in the absence of autocorrelations in the time series being regressed (and to somewhat lower confidence limits in the presence of such autocorrelations). In addition, the root-mean-square perpendicular distance ($\sigma_\perp$) between data points and regression line is listed and is plotted with the center at the position of the average value on the regression line. The number of hours folded into each regression run and the distribution of values found in the composite data set after normalization are also given in these figures.

The slope ranges of these figures make it clear that there is a statistically significant difference from unity in the slope values for many pairs of spacecraft. Further, a regression equation whose slope is consistent with unity, but whose intercept is comparable to or larger than the listed root-mean-square perpendicular distance, is also statistically inconsistent with $y = x$.

The distributions of values in the final composite data set, indicated in Figures 2, 3, and 4, are not identical to the distributions in the determination of any one of the regression lines, but they can be used as measures of the region of parameter space from which the data points were taken and outside of which the regression lines are meaningless. The percentages of hours with temperatures, densities, and bulk speeds lying outside the ranges in Figures 2, 3, and 4 are 0.4, 0.8, and 2.9, respectively.

The hours included in the determination of the regression equations in Figures 2, 3, and 4 were all hours of simultaneous data, regardless of the number of fine-time scale points per hour and regardless of whether a given hourly value was, in fact, a 3-hour average. The difference in regression parameters obtained was examined with the restriction that each hourly average be comprised of at least three fine-time scale values a restriction used by some earlier workers. For most spacecraft pairs there were only negligible changes in the regression parameters; typically 1 to 2 percent in slope and 1 to 10 percent in intercept and in root-mean-square perpendicular distance. However, for regressions of OGO 5 data with Explorers 33 and 35 data, some significant changes resulted. With the three fine-time-scale-points-per-hour restriction, the following equations were obtained

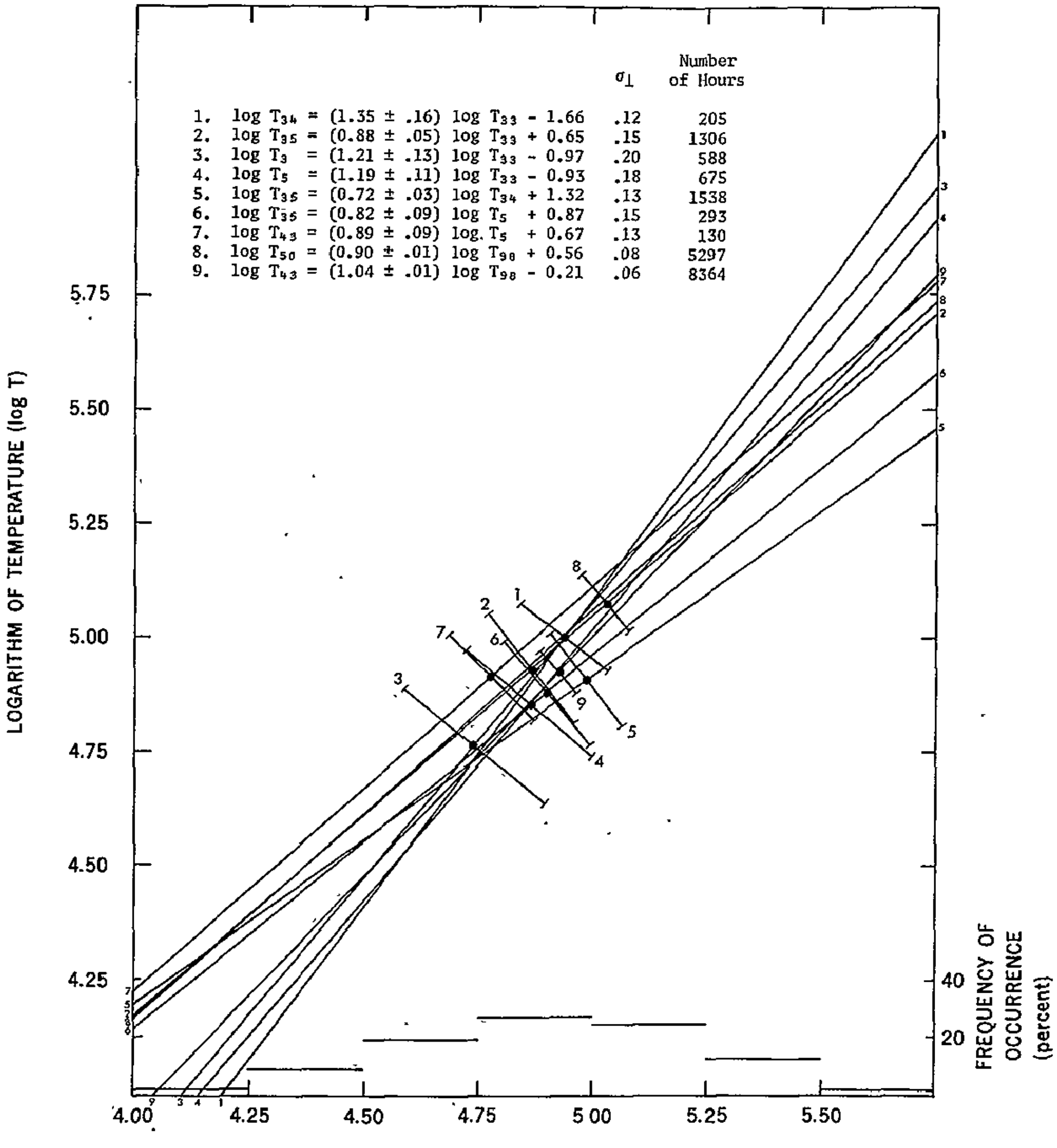

Figure 2. Plasma Temperature Regression Results

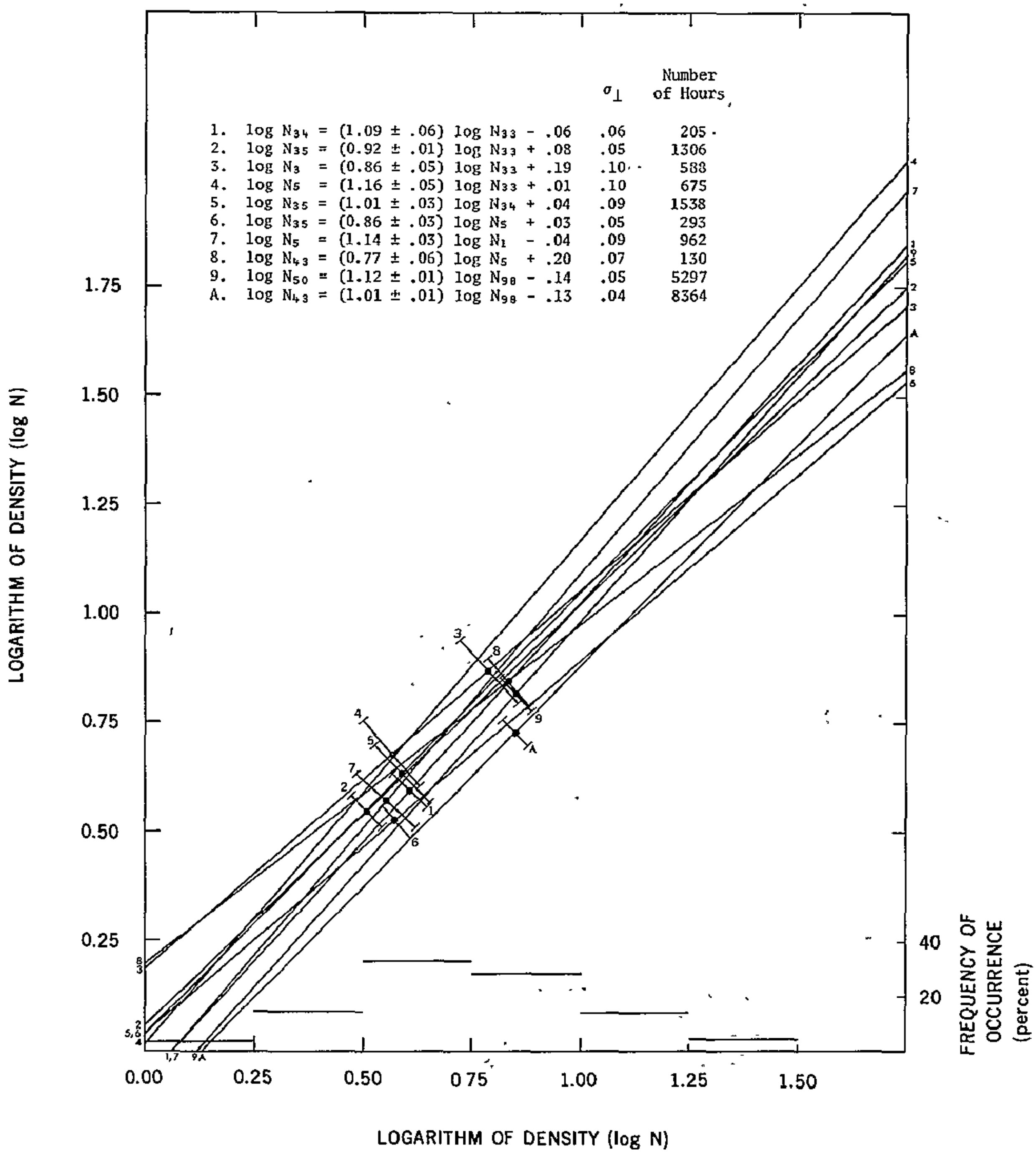

Figure 3. Plasma Density Regression Results

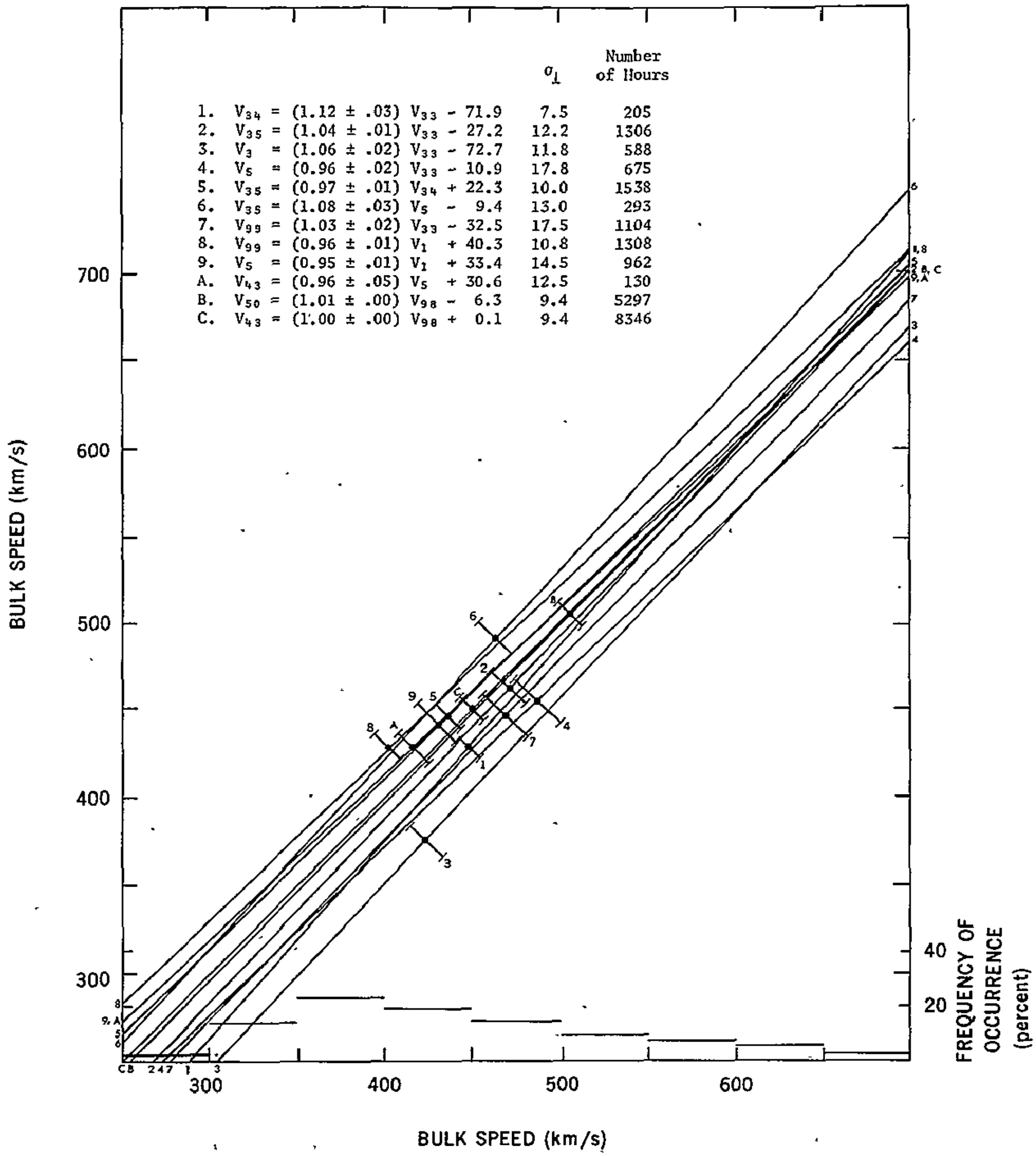

Figure 4. Plasma Bulk Speed Regression Results

	$\sigma_{\perp}$	Number of Hours
$\log T_5 = (1.08 \pm .09) \log T_{33} - 0.37$	0.17	547
$\log T_{35} = (0.91 \pm .10) \log T_5 + 0.45$	0.11	269
$\log N_5 = (1.12 \pm .04) \log N_{33} + 0.01$	0.08	547
$\log N_{35} = (0.86 \pm .03) \log N_5 + 0.03$	0.05	269
$V_5 = (1.00 \pm .02) V_{33} - 31.8$	12.7	547
$V_{35} = (1.08 \pm .03) V_5 - 11.2$	11.4	269

These are to be compared to the appropriate equations in Figures 2, 3, and 4. No significant change in the OGO 5/HEOS regression parameters was found.

Note that no regressions involving flow directions are presented, nor are flow direction angles listed or plotted in this Data Book. This is because such direction angles are given only in a small number of the source data sets (cf. Table 2), and because the potential error of measurement relative to the expected range of flow angles is significantly larger than the relative error in other plasma parameters. Flow direction angles, as received from the experimenters, are found on the magnetic tape from which this Data Book was created.

Scatter plots 1-4, 5-8, and 9-12 are plots for the logarithm of temperature, logarithm of density, and bulk speed, respectively. These correspond to a representative portion of the regression analysis results summarized in Figures 2, 3, and 4. Based on some preliminary scatter plots and other considerations, a modest number (less than 1 percent) of questionable experimenter-supplied data hours from Explorer 34, 35, 43, and 50 were eliminated from the composite data set before the reported regression runs were made.

Note the anomalously low slope in the V_{35} versus V_{34} bulk speed data between about 330 and 380 km/s. This anomaly also occurs in V_{35} versus V_{33} and V_{35} versus V_5 scatter plots (not shown), but in no other scatter plots. The conclusion is that the anomaly is in the Explorer 35 data. No special allowance was made for this apparent Explorer 35 anomaly in creating the composite data set.

Consider the spread of data points about the regression line. This variance may arise from a number of sources related to the instruments, the plasma parameter derivation from sensor outputs, the inadvertent inclusion of averages affected by terrestial or lunar effects, and the solar wind variability itself. This latter effect may be significant insofar as two spacecraft may be measuring at least partly different plasma regimes during a given hour (or 3-hour interval) because of their differing spatial locations and/or their sampling at differing portions of an averaging interval. Recall that two source spacecraft contributing to this composite data set may be separated by a few tens of Earth radii (R_E) in a solar wind flowing at 200 - 300 R_E/h. This spacecraft separation effect was not accounted for in building the composite data set, a fact that yields a slight "fuzziness" in the very concept of the hourly averaged value of an interplanetary parameter for the Earth. That this effect of solar wind variability is not the dominant cause of data point scatter is suggested by the fact that the point spread for the 50/98 (Explorer 50/Merged IMP) regression lines for all 3 plasma parameters is small relative to the spread for other spacecraft pairs, even though the 50/98 regression involves 1-hour (50) and 3-hour (98) averages.

However, whatever the source of the spread of data points, this spread, rather than quoted errors in individually determined plasma parameters, determines the limits of validity of the composite data set created by interspersing normalized hourly (or 3-hourly) averages from many spacecraft. Based on root-mean-square perpendicular distances between data points and regression lines, as listed in Figures 2, 3, and 4, irreducible uncertainties in temperatures are estimated as $\approx$ 40 percent early ($\lesssim$ 1971) and $\approx$ 20 percent late ($\approx$ 1971), in densities as $\approx$ 20 percent early and $\approx$ 10 percent late, and in speeds as 15 km/s early and 10 km/s late.

Thus, for example, it is estimated that the probability that any given (normalized) early-period temperature value is in error by more than 40 percent is $\approx$ 0.32, which is the probability that a sample point taken from a Gaussian distribution lies more than one σ away from the population mean value.

Having examined the irreducible variance in the composite data set, it is desirable to normalize the source data sets to the extent that a significant improvement in mutual consistency may be achieved. A complicating factor in the attempt to find appropriate normalizations is that there are many pairs of overlapping spacecraft. Each of several of these pairs is not independent of combinations of other pairs. For example, regression analyses have been run for Explorers 33/34, 33/35, and 34/35. These runs involved 205 common 33/34 hours between days 236 - 257 of 1967, 1306 common 33/35 hours between day 236 of 1967 and day 98 of 1968, and 1538 common 34/35 hours between days 205 - 344 of 1967. Combining the 33/34 and 33/35 results (cf. Figures 2, 3, and 4) to infer 34/35 relations and comparing these to the directly obtained 34/35 relations yields reasonable consistency in temperature and bulk speed, but a poor measure of consistency in density.

Inferred	Observed
$\log T_{35} = .65 \log T_{34} + 1.73$	$\log T_{35} = .72 \log T_{34} + 1.32$
$\log N_{35} = .84 \log N_{34} + 0.13$	$\log N_{35} = 1.01 \log N_{34} + 0.04$
$V_{35} = .93\ V_{34} + 40.0$	$V_{35} = 0.97\ V_{34} + 22.3$

To some extent, this apparent discrepancy may arise from the different time periods over which these data were taken, combined with some undetected or inadequately treated temporal dependence in sensor characteristics. Although this possibility is examined in more detail in connection with magnetic fields, temporal variation in sensor characteristics has generally been neglected in this data compilation, with some minor exceptions to be noted later. It is of interest that upon comparing inferred and observed 34/35 relations based on only 166 hours of simultaneously available 33, 34, and 35 data, a high level of consistency was found.

The possibility of seasonal variation in the Explorer 33/35 regressions, which might arise from the fact that the Explorer 33 spin vector lay in the ecliptic plane, was examined. (All subsequently launched Explorer spacecraft used in this compilation had spin vectors normal to the ecliptic plane.) For days 236 - 255 of 1967, and then days 18 - 98 of 1968, it was found that

		σ_1	Number of Hours
1967	$\log T_{35} = (0.89 \pm .06) \log T_{33} + 0.59$	0.15	1061
	$\log N_{35} = (0.91 \pm .02) \log N_{33} - 0.08$	0.05	
	$V_{35} = (1.04 \pm .01) V_{33} - 29.3$	12.8	
1968	$\log T_{35} = (0.67 \pm .10) \log T_{33} + 1.62$	0.11	245
	$\log N_{35} = (1.04 \pm .03) \log N_{33} - 0.02$	0.03	
	$V_{35} = (1.08 \pm .04) V_{33} - 42.1$	8.6	

There are apparently significant changes in these two subsets of the Explorer 33/35 data. Nevertheless, because inspection of the appropriate scatter plots revealed that the 245 data points for 1968 populate a region of parameter space (for each of the three parameters) entirely populated by some of the 1061 points for 1967, the choice was made to neglect seasonal variations in performing plasma parameter normalizations. Further, since the 33/35 regression parameters for all 1306 points of 1967 to 1968 are close in value to the corresponding 33/35 parameters for the 1061 points of 1967 only, it appears that seasonal variation is not responsible for the previously discussed discrepancy in observed and inferred 35/34 regression parameters.

Because of many uncertainties in the analysis (dependence of regression results on data subset used, occurrence of spurious points despite attempts to eliminate such points, possible time dependence in spacecraft and/or sensor characteristics, autocorrelations in time series being regressed, etc.), and because of the need to perform normalizations simultaneously and consistently for many overlapping data sets, it was decided to normalize temperatures and densities only when visually better fits to y = x in scatter plots could be achieved. Bulk speed data have been normalized on the basis of the regression equations in Figure 4, as discussed further below.

The normalized parameter values T_n, N_n, and V_n found in the final composite data set are related to the experimenter-supplied values through the normalization equations listed in Table 4. Many points deserve note:

1. The Explorer 18 densities and speeds overlapped with no other available data and could not be normalized. Their limits of validity, relative to the rest of the data set, is uncertain.

2. The "Merged Vela" speeds were separately considered for periods before and after Jan. 1, 1968, and different normalization equations were chosen.

3. The early data (sources 1, 3, 5, 33, 34, 35, and 99) were normalized independently of the later data (sources 43, 50, and 98). In the later period, the 43 and 50 data were normalized to the 98 data. This separation into early and late data follows from the fact that the only overlap between an early period source and a late period source is the set of 130 OGO 5/Explorer 43 common hours obtained in March and April of 1971. (cf. scatter plots 3, 7, and 11.) Given the smallness of this number of hours, and given the fact that the OGO 5 instrumentation was 3 years postlaunch during these hours, it did not seem justifiable to use the OGO 5/Explorer 43 regression to normalize the early and/or late period data to a common standard. This inability to make a dependable early/late normalization will probably not introduce any gross errors into the study of solar cycle variations with this data set; nevertheless, this point should be kept in mind in such studies.

4. Many speeds have been normalized by relatively small amounts, because reliability in absolute speed values is important for studies attempting to link features at 1 AU with solar features. (Note that a 10 to 15 km/s uncertainty in speed for an ≈400 km/s solar wind yields a solar source longitude uncertainty of 1.4° to 2.1° with the frequently used constant radial velocity approximation.) In the early data (≲ 1971), speed normalizations were chosen using the numerous regressions between Explorer 33 and other spacecraft. It was assumed that a weighted average of these equations would yield a relationship between Explorer 33 speeds and "true" speeds and that this relationship could then be used with the Explorer 33/Spacecraft X regression result to yield a relationship between Spacecraft X speeds and "true" speeds.

Table 4. Normalization Equations Used

Spacecraft Identifier	log T_n =	log N_n =	V_n =
18	-	log N_{18}	V_{18}
3	.8 + .83 log T_3	-.222 + 1.16 log N_3	26 + .99 V_3
99 (≤ '67)	-	-	26 + .99 V_{99}
99 (≥ '68)	-	-	V_{99}
33	log T_{33}	log N_{33}	-44 + 1.05 V_{33}
34	1.2 + .75 log T_{34}	log N_{34}	9 + .98 V_{34}
35	log T_{35}	log N_{35}	-8 + V_{35}
5	log T_5	.9 log N_5	-10 + 1.05 V_5
1	-	log N_1	32 + .98 V_1
98	log T_{98}	log N_{98}	V_{98}
43	log T_{43}	.097 + log N_{43}	V_{43}
50	-.62 + 1.1 log T_{50}	.121 + .89 log N_{50}	V_{50}

5. There was a certain amount of arbitrariness in arriving at the normalization equations given in Table 4. However, for the most part, other reasonable choices of normalization parameters would lead to normalized parameter values different from the values obtained by amounts less than the previously discussed intrinsic uncertainties in the composite data set. Nevertheless, the composite unnormalized solar wind tape is available from NSSDC if the reader wishes to make a different normalization.

Magnetic Field Data

The regression analysis results for the interplanetary magnetic field data are given in Tables 5 and 6. The notation follows from an equation of the form

$$P_{s1} = a\ P_{s2} + b,$$

where P denotes the parameter, s1 and s2 identify the two spacecraft, and a is the slope and b is the intercept. Note that slope values are given with limits that would correspond to 95 percent confidence limits in the absence of autocorrelations and which, given the presence of some autocorrelations, in fact correspond to somewhat lower confidence limits. The column labeled $\sigma_\perp$ gives the root-mean-square perpendicular distance between data points and the "best fit" regression line. Table 5 relates to field cartesian components (solar ecliptic coordinates), and Table 6 relates to the average field magnitudes and to direction angles derived from averaged cartesian components.

The units of the b and $\sigma_\perp$ columns are gammas and degrees, as appropriate. In selecting hourly averages for analysis, no restriction on the minimum number of fine-time scale points per hour was imposed. That there are fewer hours in the field longitude regression equation determination than for other parameters results from the exclusion of hours when $|\phi_{s1} - \phi_{s2}| > 180°$. (Such hours of, for example, $\phi_{s1} \approx 10°$ and $\phi_{s2} \approx 350°$ are appropriate for inclusion in regression analysis for other parameters; that such hours were not included in the ϕ regression, with ϕ_{s1} set equal to 370° in the example given, is expected to introduce no bias in the ϕ regression results.)

Scatter plots 13-27 correspond to selected regression runs in Table 5. From inspection of this table and those figures, several points may be made:

1. The slopes are different from unity by several percent in many cases. This implies errors of several percent in effective sensitivity factors in one or both spacecraft involved (*King and Ness*, 1977). There is no unique and con-

Table 5. Regression Results for Field Cartesian Components

$$P_{S1} = a\,P_{S2} + b$$

S1	S2	Number of Points	P	a	b	$\sigma_\perp$
28	33	1183	B_x	1.00 ± .02	0.16	0.99
			B_y	0.93 ± .02	-0.02	1.05
			B_z	1.04 ± .06	-0.05	1.55
34	33	1497	B_x	0.94 ± .02	0.03	0.83
			B_y	0.96 ± .01	-0.03	0.77
			B_z	0.97 ± .03	0.10	0.97
33	35	3040	B_x	1.09 ± .01	0.01	0.75
			B_y	1.09 ± .01	0.03	0.74
			B_z	1.07 ± .02	0.80	0.96
34	35	3145	B_x	0.91 ± .03	-0.17	1.79
			B_y	1.09 ± .01	0.09	0.83
			B_z	0.91 ± .02	0.61	1.14
1	35	1156	B_x	1.08 ± .02	0.17	1.01
			B_y	1.09 ± .02	-0.01	1.07
			B_z	1.01 ± .02	0.72	1.05
41	1	2021	B_x	1.00 ± .01	-0.09	0.72
			B_y	0.99 ± .01	0.00	0.70
			B_z	0.99 ± .02	-0.10	0.66
41	43	1424	B_x	1.03 ± .01	-0.07	0.50
			B_y	1.01 ± .01	0.05	0.52
			B_z	0.98 ± .02	0.05	0.56
47	43	755	B_x	1.05 ± .03	0.00	0.98
			B_y	1.01 ± .03	0.13	0.99
			B_z	1.02 ± .04	0.19	1.40
50	43	1657	B_x	0.89 ± .02	0.15	1.35
			B_y	1.01 ± .02	-0.06	0.82
			B_z	0.94 ± .02	0.08	0.85
1	43	4898	B_x	0.98 ± .01	-0.03	1.17
			B_y	1.00 ± .01	0.03	0.94
			B_z	0.98 ± .01	0.09	0.90
1	47	1675	B_x	0.98 ± .02	0.03	1.00
			B_y	1.01 ± .02	0.03	1.09
			B_z	0.99 ± .03	-0.11	0.91
1	50	3130	B_x	1.04 ± .01	0.00	0.94
			B_y	0.97 ± .02	0.10	1.07
			B_z	1.00 ± .02	0.08	0.88

Table 6. Regression Results for Field Magnitude and Angles

$$P_{S1} \approx a\, P_{S2} + b$$

S1	S2	Number of Points	P	a	b	$\sigma_{\perp}$
28	33	1183	B	0.96 ± .02	-0.07	0.61
		1183	θ	1.13 ± .08	1.28	15.18
		1130	φ	1.01 ± .02	1.52	17.20
34	33	1497	B	0.97 ± .01	-0.01	0.34
		1497	θ	1.01 ± .04	1.18	12.14
		1418	φ	1.00 ± .01	0.19	15.24
33	35	3040	B	1.06 ± .01	0.02	0.40
		3040	θ	1.01 ± .01	8.73	11.92
		2930	φ	1.00 ± .01	-0.19	15.26
34	35	3145	B	0.84 ± .02	0.99	0.94
		3145	θ	0.99 ± .03	7.91	12.87
		2969	φ	0.98 ± .01	5.96	20.09
1	35	1156	B	1.06 ± .03	-0.07	0.97
		1156	θ	0.95 ± .04	7.74	13.06
		1098	φ	1.01 ± .02	-1.13	17.10
41	1	2021	B	1.00 ± .01	0.06	0.35
		2021	θ	1.00 ± .02	-1.10	9.02
		1959	φ	0.99 ± .01	4.88	15.19
41	43	1424	B	1.01 ± .01	-0.03	0.31
		1424	θ	0.98 ± .02	1.05	8.32
		1381	φ	0.99 ± .01	3.47	11.44
47	43	755	B	1.02 ± .01	0.05	0.30
		755	θ	1.01 ± .06	2.70	13.05
		729	φ	1.01 ± .02	-0.76	16.85
50	43	1657	B	0.80 ± .02	1.21	1.17
		1657	θ	1.02 ± .03	1.31	9.11
		1583	φ	1.00 ± .01	-4.05	12.85
1	43	4898	B	0.93 ± .01	0.38	0.96
		4898	θ	1.00 ± .02	1.24	9.47
		4649	φ	0.99 ± .01	1.90	15.02
1	47	1675	B	0.98 ± .01	-0.04	0.41
		1675	θ	0.97 ± .03	-1.93	12.02
		1584	φ	1.01 ± .01	-6.19	18.53
1	50	3130	B	1.01 ± .01	-0.07	0.35
		3130	θ	1.01 ± .02	1.07	11.11
		2927	φ	1.00 ± .01	3.59	19.04

sistent way to determine in which source data sets these apparent errors occur, despite the availability of several different spacecraft pairs.

2. The root-mean-square perpendicular distance between data points and regression line is typically of the order of 0.5 - 1.0 gamma. Variability between spacecraft pairs results from both differing widths of the main clusters of points as well as different numbers of far-outlying points. The factors yielding non-zero root-mean-square perpendicular distances are addressed in the preceding discussion of plasma data. The significance of these point spreads is that they yield the limits of validity of the corresponding parameter. Thus, a listed value of B_x represents the "true" hourly averaged IMF B_x component for Earth to within 0.5 to 1.0 gamma.

3. The regression line intercepts are always less than 0.2 gamma (and often less than 0.1 gamma) except for those involving B_z as measured by Explorer 35. It appears that the Explorer 35 B_z values are too small (too negative) by about 0.7 gamma, and that sensor zero levels for the other spacecraft involved in the regressions have been well determined. It is appropriate to note that in *King* (1975) it was found that, when B_z was averaged over all available hours separately for each source data set, all such averages were within 0.2 γ of zero, except for Explorer 18 (B_z = -1.0 γ based on 1215 hours) and Explorer 35 (B_z = -0.7 γ).

Inspection of Table 6 reveals that typical uncertainties in the "true" hourly averaged IMF magnitude, latitude, and longitude angles for Earth are ≈0.3 to 1.0 γ, ≈10° to 15°, and ≈15° to 20°, respectively. The previously noted ~0.7 γ offset in Explorer 35 B_z is reflected in the ≈8° offset in the θ regression runs involving Explorer 35. Otherwise, intercepts for the θ regressions are all reasonably close to zero. The intercepts for the ϕ regressions exhibit a surprisingly large range of up to ≈6°, although there is no unique and consistent way to assign angle offsets to specific source data sets.

It is apparent from Table 6 that the field magnitude regression slope has an unusually low value (0.80) for Explorer 50/Explorer 43. This is in contrast to the fact that Explorer 50/HEOS and Explorer 43/HEOS field magnitude regression slopes are both much closer to unity (1.01 and 0.93). A similar inconsistency is visible in the 34/33, 35/33, and 34/35 regression runs.

Given the availability of more than 3 years of overlapping HEOS/Explorer 43 data, the possibility of time dependencies in regression parameters that might be at least partially responsible for such inconsistencies has been examined. The results are summarized in Table 7. Note that there is a slight trend for the field component regression line slopes to decrease with time, with a statistically significant decrease of about 10 percent in 1974 relative to 1973. Note the more dramatic variation in the field magnitude regression line slopes. This difference in the character of the temporal changes between field magnitude and field component regression parameters is, at least in part, due to the application of the same regression analysis to dissimilar distributions of field component values (quasi-normal) and field magnitude values (non-normal).

It appears that time variations in sensor characteristics may yield some inconsistencies in comparing results from apparently redundant triads of spacecraft pairs. Nevertheless, it is difficult to uniquely assign time variations to specific source data sets.

Despite the present findings of regression line slopes different from unity, and some intercepts different from zero, no IMF data normalizations have been performed because the line y = x passes through the main cluster of IMF data points on the scatter plots shown (and on those not shown). Equivalently, the changes in parameters brought about by appropriate normalization would be less than the previously discussed uncertainties in these parameters.

These mutual consistency results have been included to give the potential data user both quantitative and qualitative insight into the limits of validity of the composite data set. If the reader believes a specific study would profit from data normalizations, it is advised that normalization be done. The magnetic tape containing up to three sets of IMF data from different spacecraft per hour is available from NSSDC if the reader wishes to test data mutual consistency in some manner other than that employed herein.

DATA SELECTION

The final composite data set was assembled from the composite IMF tape, the normalized composite plasma tape, and a tape with geomagnetic and solar activity indexes. For a given hour, the plasma and field data were each taken from one of possibly several available source data sets according to the following priority scheme.

Plasma data were considered first. If the plasma spacecraft used for the preceding hour was available, and it had a 1-hour resolution, it was chosen. If the plasma spacecraft used for the preceding hour was not available, or if it had a 3-hour resolution, that source having at least three fine-time scale points per hour and having the highest priority was chosen. The priority ordering was (high to low) 33, 35, 34, 3, 50, 43, 5, 1, 99, and 98, determined somewhat arbitrarily on the basis of available

Table 7. Regression Results for HEOS/Explorer 43 Field Data

$$P_1 = a\ P_{43} + b$$

Year	Number of Points	P	a	b	σ_1
1971	102	B_x	1.03 ± .06	0.28	0.64
		B_y	1.06 ± .10	0.03	1.07
		B_z	1.06 ± .13	0.01	0.75
		B	0.95 ± .04	0.47	0.26
1972	1425	B_x	1.04 ± .02	-0.08	0.79
		B_y	1.02 ± .01	0.06	0.78
		B_z	1.02 ± .02	0.02	0.83
		B	1.08 ± .02	0.43	0.75
1973	1592	B_x	1.01 ± .02	-0.08	0.90
		B_y	1.02 ± .02	-0.02	0.98
		B_z	1.02 ± .03	0.02	0.89
		B	1.01 ± .01	-0.08	0.33
1974	1779	B_x	0.90 ± .03	0.14	1.57
		B_y	0.96 ± .02	0.03	1.01
		B_z	0.90 ± .03	0.06	0.96
		B	0.71 ± .05	1.67	1.27

parameters and temporal resolution. If no source was chosen using the just mentioned criteria, the fine-time-scale-points-per-hour criterion was dropped, and the same priority criterion was reapplied.

Then IMF data were taken from the same spacecraft, if available, from which the plasma data were just chosen. However, if this spacecraft was not available, if it was Explorer 35, or if there were no plasma data for the current hour, IMF data were taken from the spacecraft providing IMF data for the previous hour. Again, if this spacecraft was not available or was Explorer 35, IMF data were taken from the highest priority spacecraft available, according to the priority ordering (high to low) 50, 47, 43, 1, 41, 34, 33, 28, and 35. Note that Explorer 35 IMF data appear in the final composite data set only for those 2825 hours when IMF data were available from no other source.

FIELD COMPONENT TRANSFORMATIONS

There are several orthogonal, right-handed coordinate systems in which interplanetary vector quantities are usefully expressed. In geocentric solar ecliptic (GSE) coordinates, the X-axis points from the Earth to the Sun and the Z-axis is normal to the ecliptic plane, positive northward. Geocentric solar equatorial coordinates also have an X-axis pointing from the Earth to the Sun, but have a Y-axis lying in a plane parallel to the solar equatorial plane, positive in a direction roughly opposite that of planetary motion. In this system, which differs from the GSE system by 7.25° at most, the ideal spiral magnetic field (*Parker*, 1958) has no Z component. In geocentric solar magnetospheric (GSM) coordinates, the X-axis again points from the Earth to the Sun, while the Z-axis lies in a plane containing the X-axis and the Earth's magnetic dipole axis and is positive northward. The GSM system is appropriate for studies of magnetospheric effects of IMF variations. See *Russell* (1971) for a more detailed discussion of these and other coordinate systems and the transformations among them.

The solar wind flow direction angles were provided in GSE coordinates and are contained on the composite tape in these coordinates only. The IMF data, given only in GSE coordinates in the source data sets and in the predecessor to this Data Book (*King*, 1975), are given in the present composite data set in both GSE and GSM coordinate systems. The required transformations were performed at NSSDC.

IMF VECTOR STANDARD DEVIATION

As indicated previously, standard deviations for hourly averages of various IMF parameters were made available in various source data sets. However, there was no parameter for which a standard deviation was given in all source data sets.

In order to have a consistent measure of field fluctuations for the composite data in the predecessor to this Data Book, a "vector standard deviation" was computed as $(\sigma_{B_X}{}^2 + \sigma_{B_Y}{}^2 + \sigma_{B_Z}{}^2)^{1/2}$ for Explorer data sets and as $(\sigma_B{}^2 + B^2 \sigma_\theta{}^2 + B^2 \cos^2\theta\, \sigma_\phi{}^2)^{1/2}$ for HEOS records. In so far as these expressions represented the lengths of the diagonals of "uncertainty elements" at the tips of the hourly averaged field vectors, they were taken to yield a quasi-homogeneous set of data when interspersed.

However, it has subsequently been pointed out (*Svalgaard*, 1976) that the expression $\sigma_{B_X}{}^2 + \sigma_{B_Y}{}^2 + \sigma_{B_Z}{}^2$ is analytically equivalent to the expression $\sigma_B{}^2 + B^2 - F^2$, where B is the average field magnitude, σ_B its standard deviation, and F the length of the vector constituted by the averaged cartesian components. Accordingly, the vector standard deviation contained in the new composite data set (tape and listings of this Data Book) is $(\sigma_{B_X}{}^2 + \sigma_{B_Y}{}^2 + \sigma_{B_Z}{}^2)^{1/2}$ for Explorer records and $(\sigma_B{}^2 + B^2 - F^2)^{1/2}$for HEOS records.

DATA PRESENTATION

The composite interplanetary plasma/magnetic field data set has been assembled onto a single magnetic tape with one record for each hour of Bartels' solar rotations 1783 through 1947 (Nov. 2, 1963 to Jan. 12, 1976). The data found in a given record consist of a flag to indicate whether there are plasma and/or field data (or neither) for that hour, time information and Bartels' rotation number, identifiers for the plasma and field source spacecraft, numbers of fine-time scale points in the plasma and field averages, average field magnitude and GSE and GSM cartesian components, magnitude and latitude and longitude angles of the vector comprised by the GSE cartesian components, standard deviations in the average magnitude and in cartesian component averages (Explorer IMF data) or in field angle averages (HEOS IMF data), field vector standard deviation (see previous section for discussion of this parameter), proton temperature, proton density, bulk flow speed and direction angles, standard deviations in the plasma parameters, geomagnetic activity indexes K_p and C_9, and the sunspot number R. The initial flag, the time and solar rotation words, and the geomagnetic activity indexes and sunspot number words have meaningful values for all hours. Plasma (field) words are filled with zeros for hours when no plasma (field) data were available. In addition, individual words corresponding to parameters not provided in the source data set are also filled with zeros. This tape (which may be updated as warranted) is available from NSSDC with a detailed format statement.

The Data Book consists of graphical and tabular presentations of some of the parameters of the composite data set. There are two plots for each solar rotation in which any plasma or field data were obtained. On facing pages, for a convenience in lining up features in the data, are found a plot of plasma data (temperature, density, and bulk speed) and a plot of field data (average magnitude, GSM B_Z component, and GSE latitude and longitude angles of the average field vector). Note the 450° range in the cyclic field longitude angle, employed to decrease the number of times the trace crosses the plot in response to small excursions in the field direction. Note that on those rare occasions when the parameter values exceed the allowed range, a heavy mark is placed near the edge of the plot. For such cases, the reader is advised to consult the data listings in the Appendix for appropriate numerical values.

In a separately bound Appendix to this *Interplanetary Medium Data Book* are found listings of selected hourly parameters, which include plasma temperature (in units of 1000°K), proton density (cm^{-3}), bulk speed (km/s), and an identifier of the spacecraft from which the plasma data were taken. Also found with the plasma data are the field parameters: average magnitude, GSM cartesian components, latitude and longitude angles of the vector made up of the average GSE field components, the previously discussed vector standard deviation, and an identifier of the IMF spacecraft. Note that to economize space, one-character alphabetic spacecraft identifiers have been used (as in this document's predecessor, *Interplanetary Magnetic Field Data Book*) although numeric identifiers are used on the magnetic tape for convenience. (See Table 1 for definitions of the identifiers.) Also note that the data are listed in 1-day blocks and that days with no field or plasma data are omitted from the listings.

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SCATTER PLOTS

The following pages contain plasma and magnetic field scatter plots for selected pairs of spacecraft.

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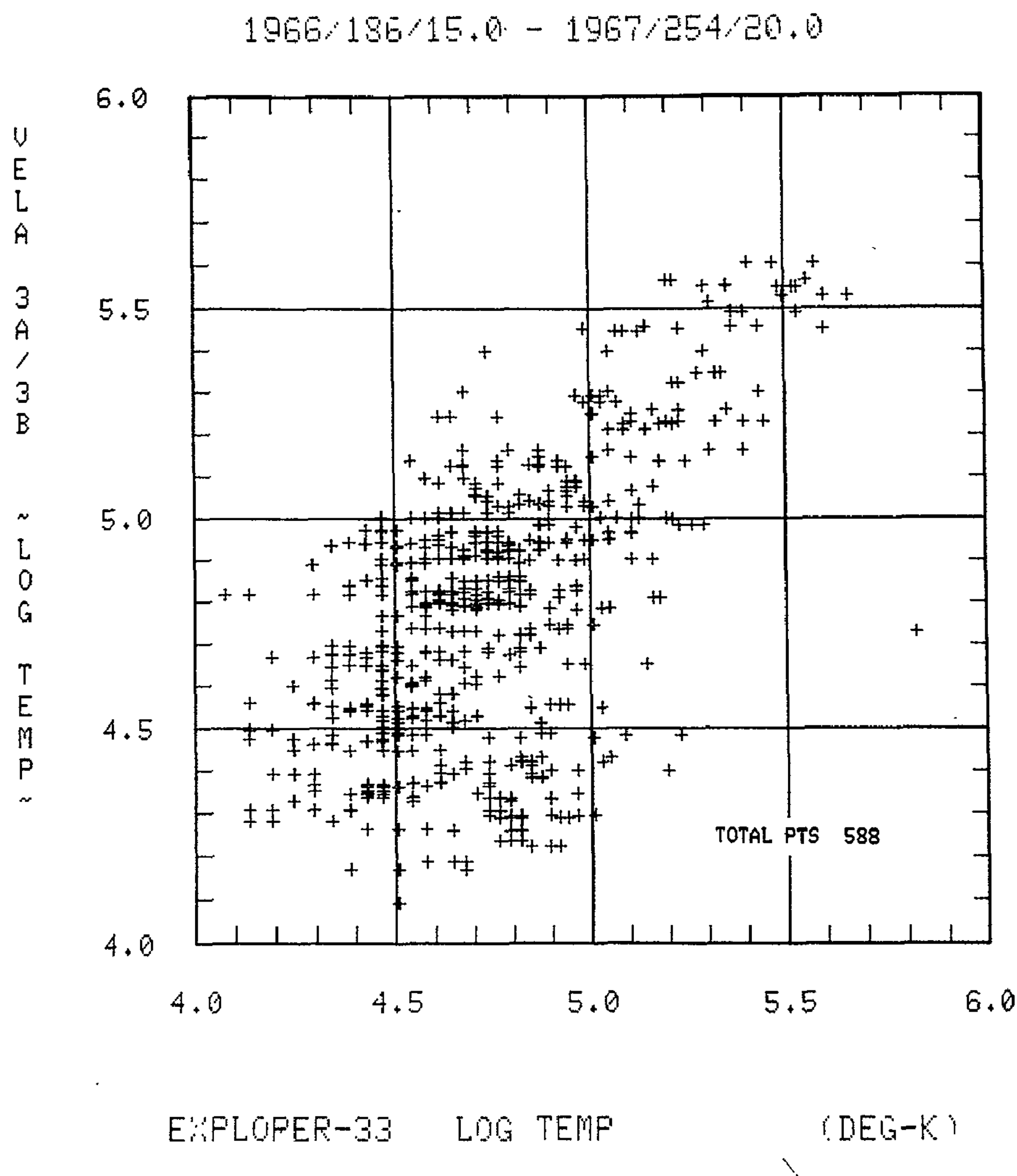

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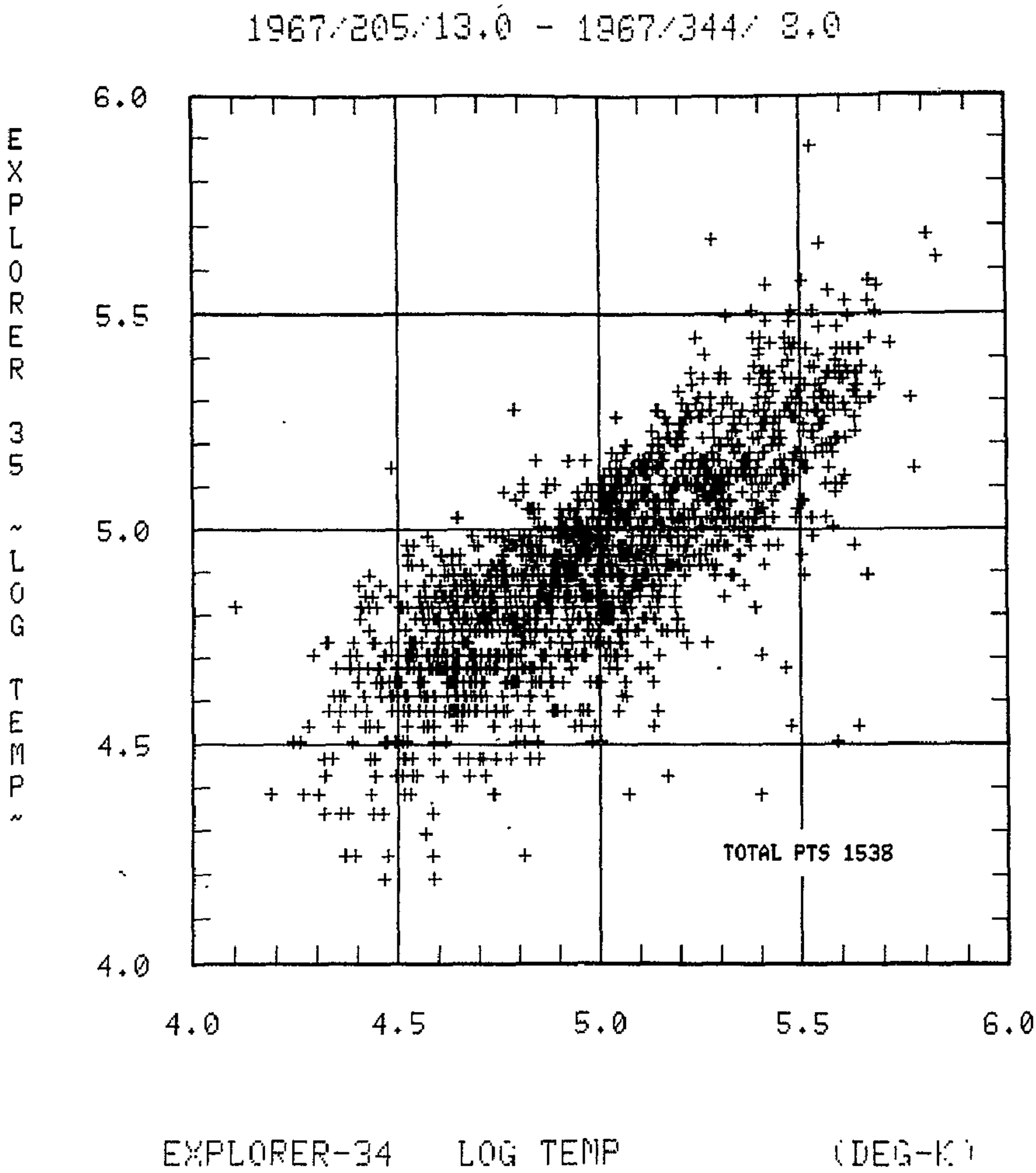

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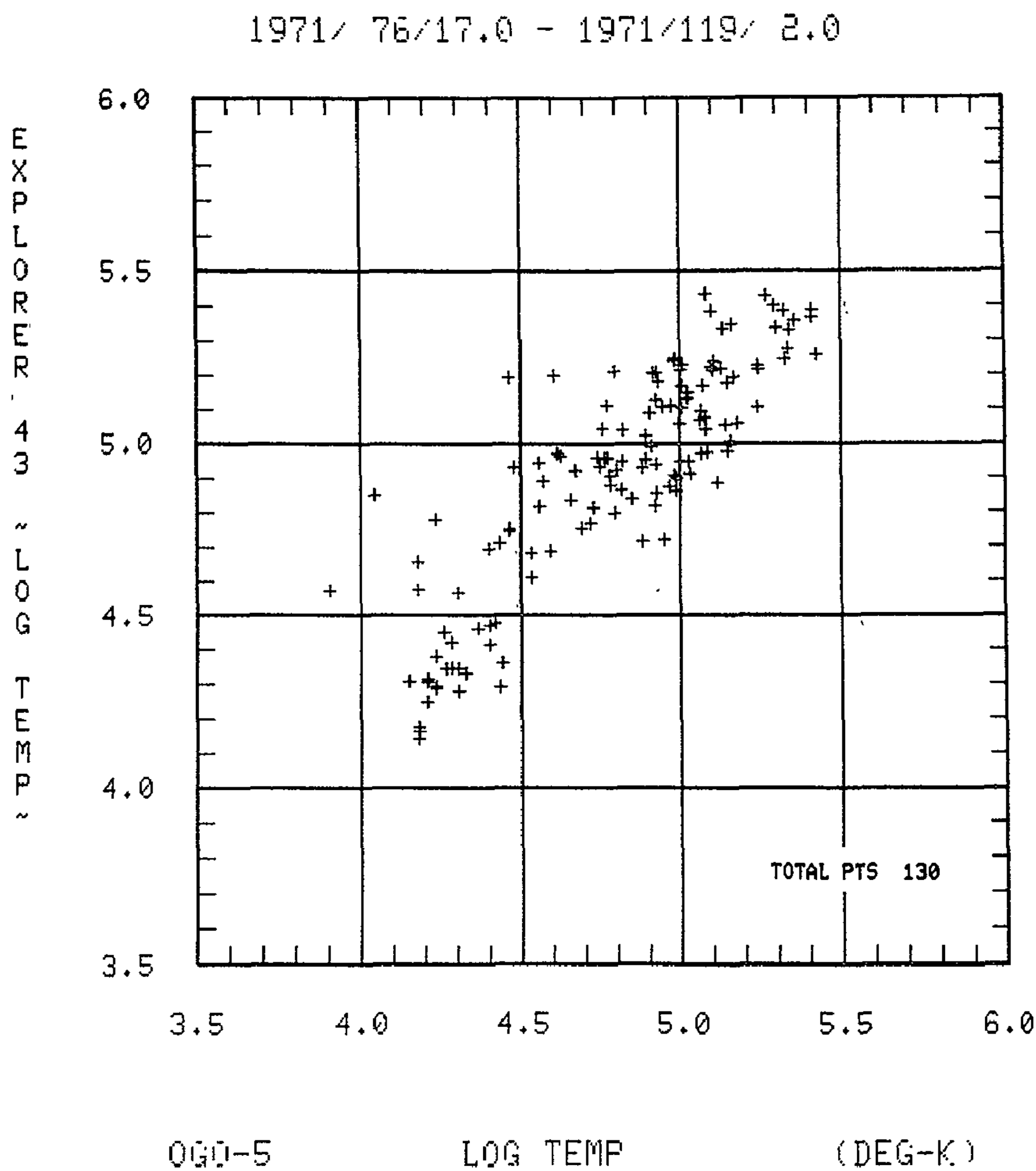

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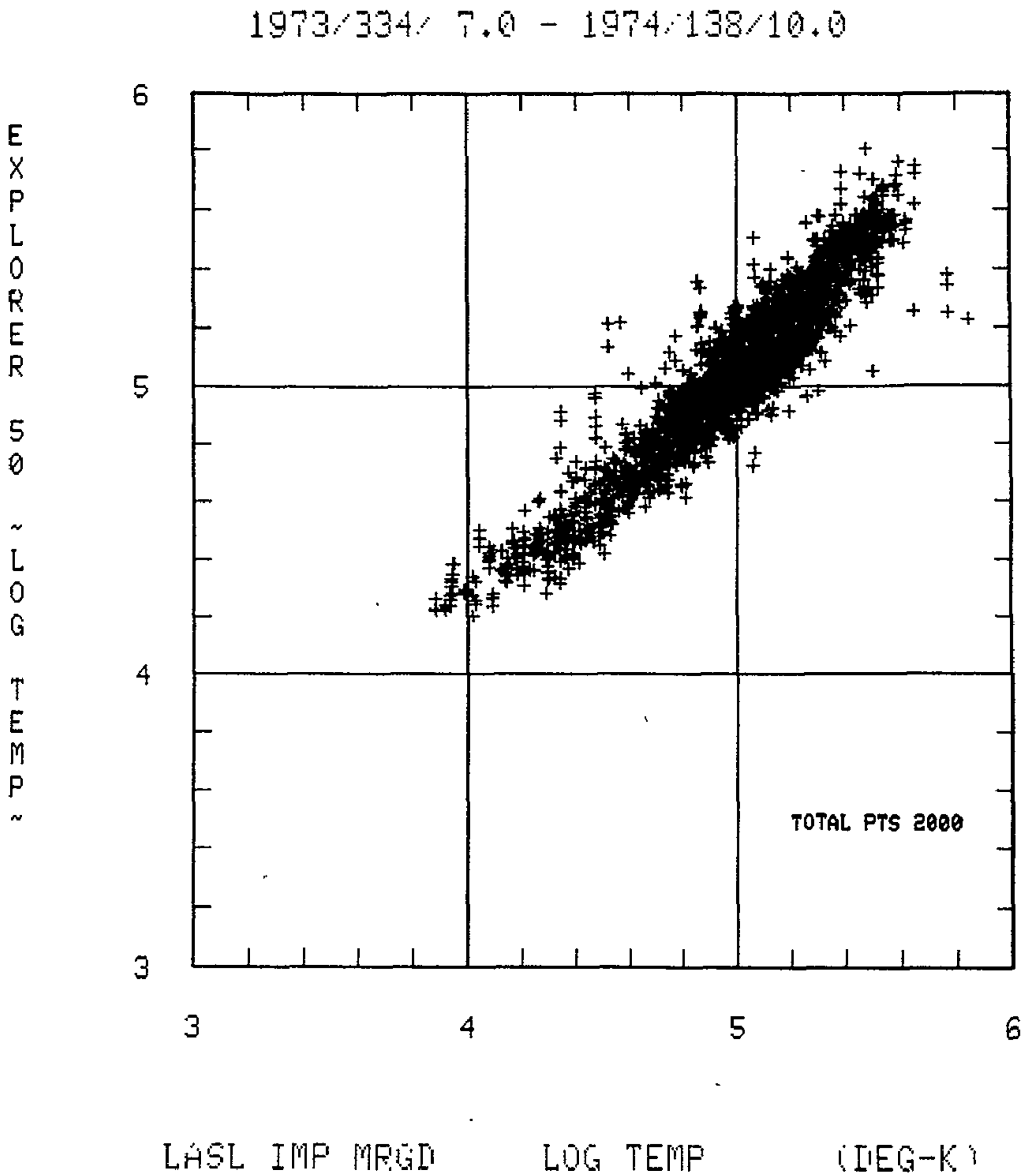

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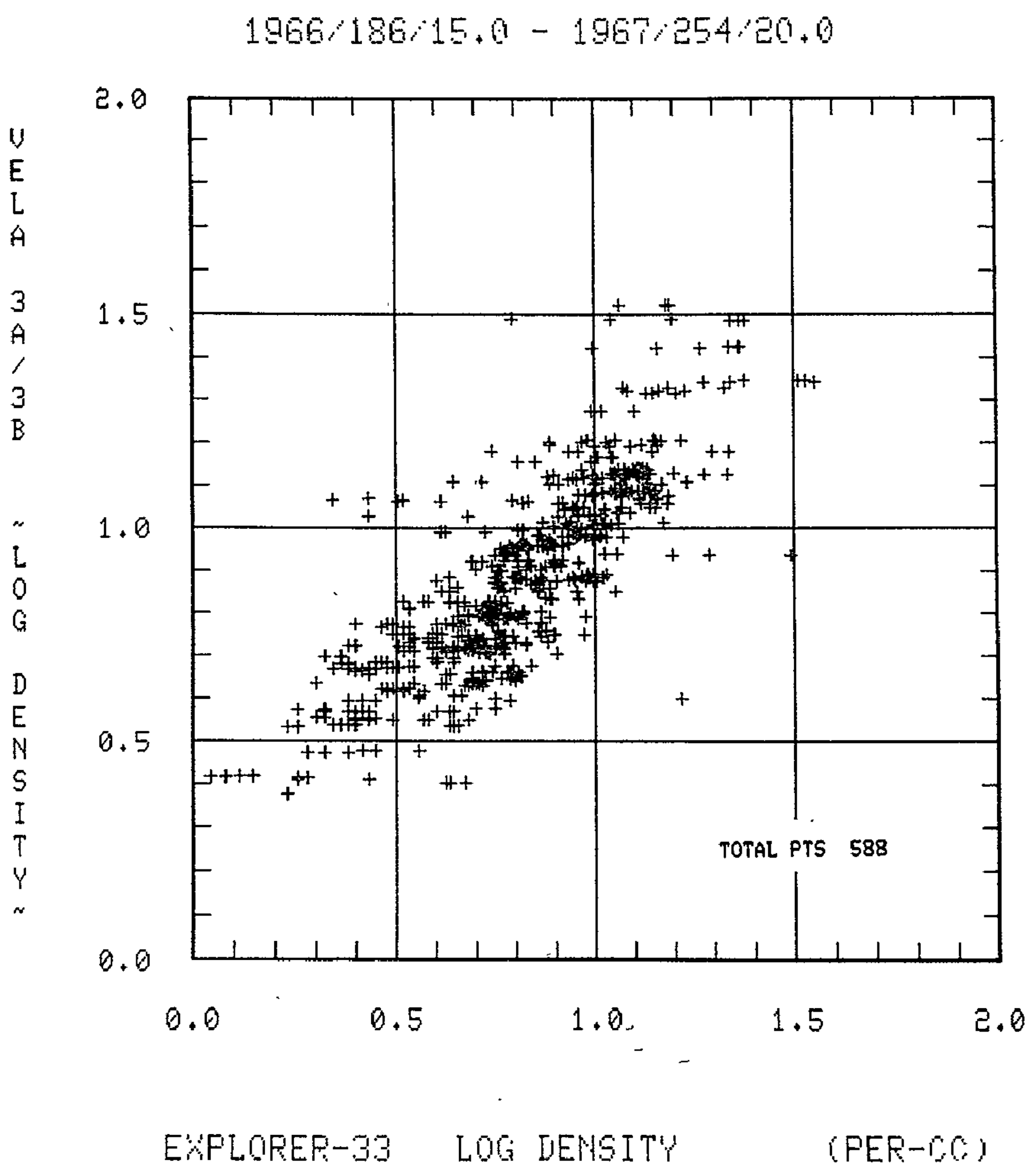

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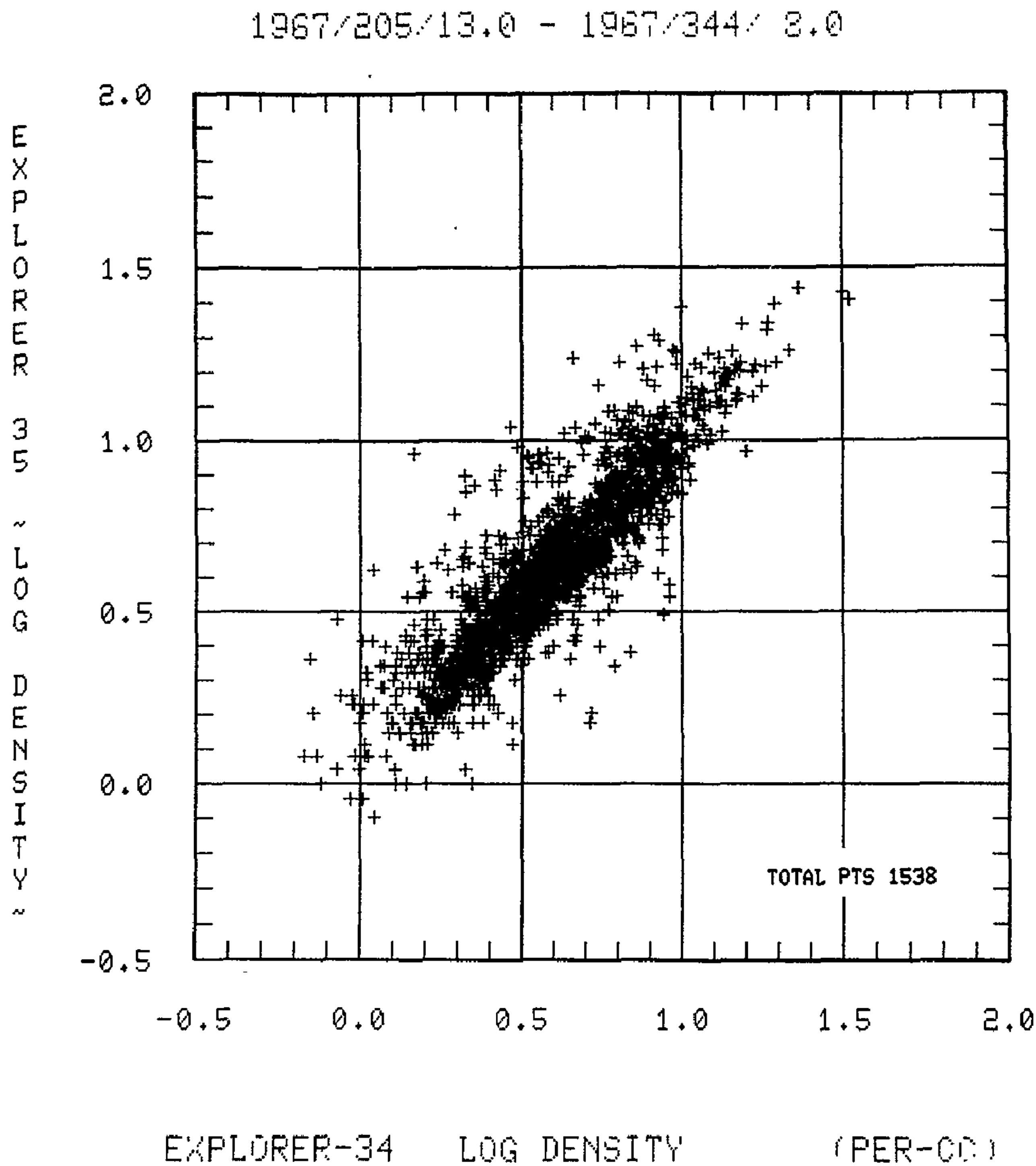

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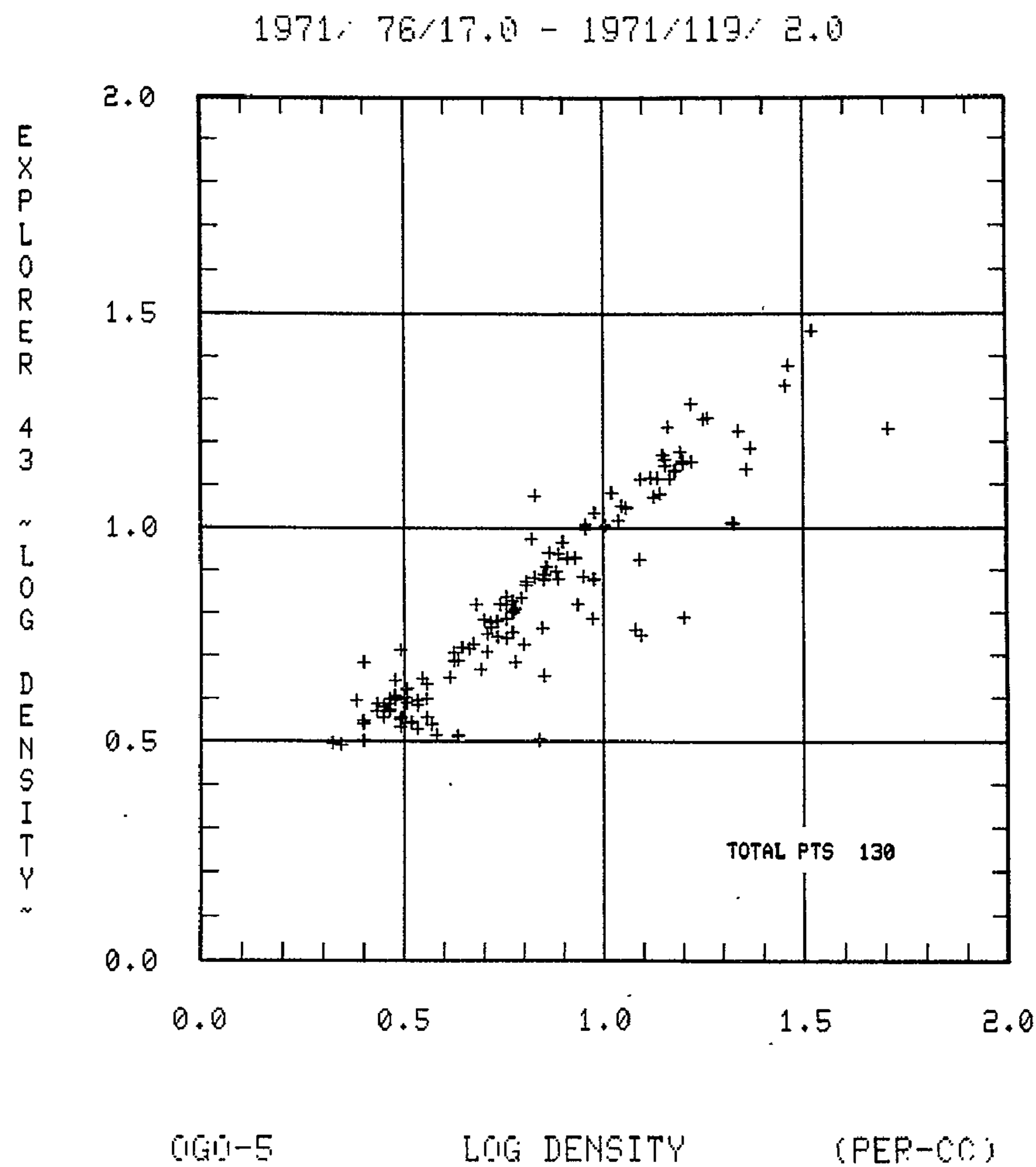

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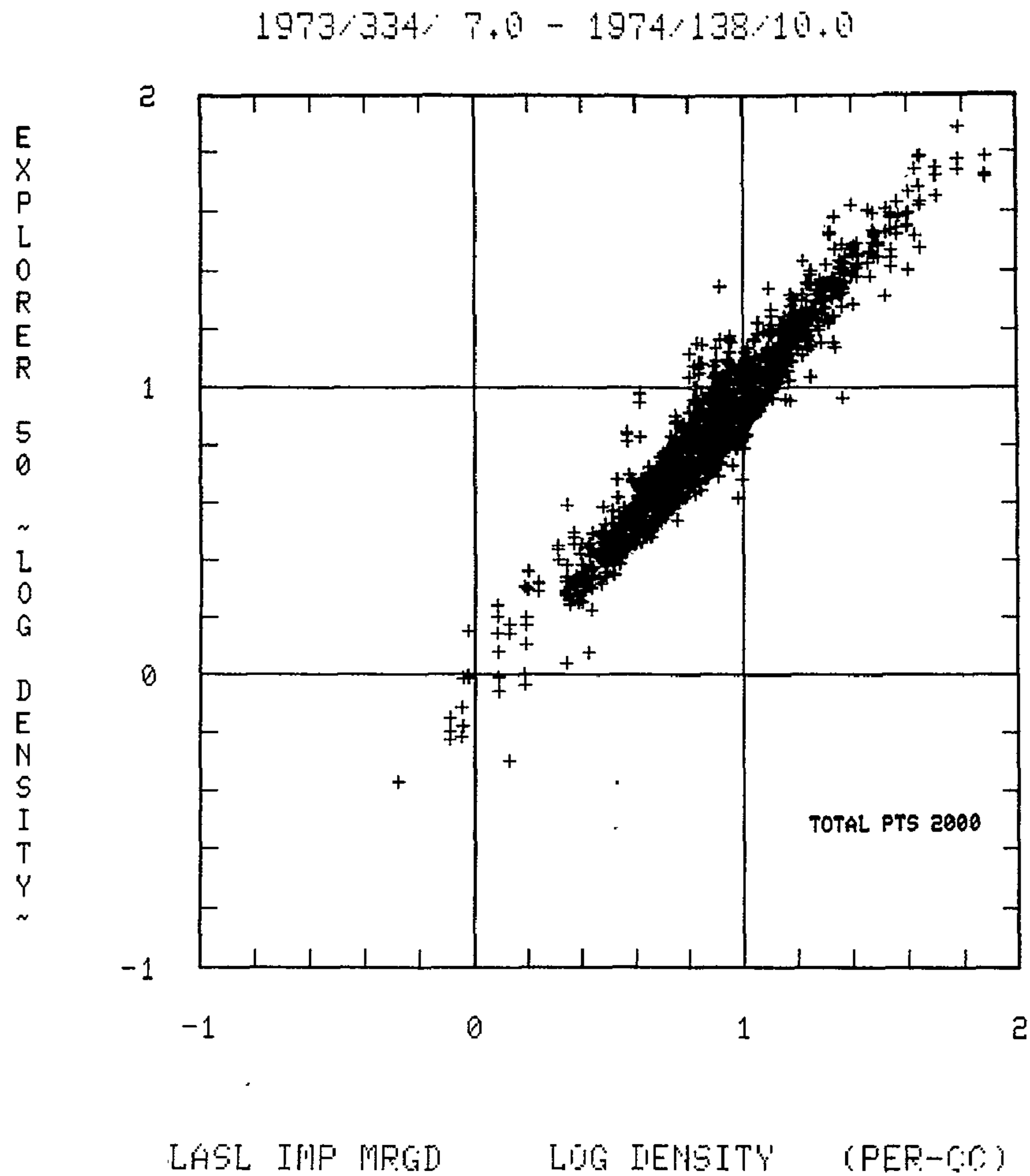

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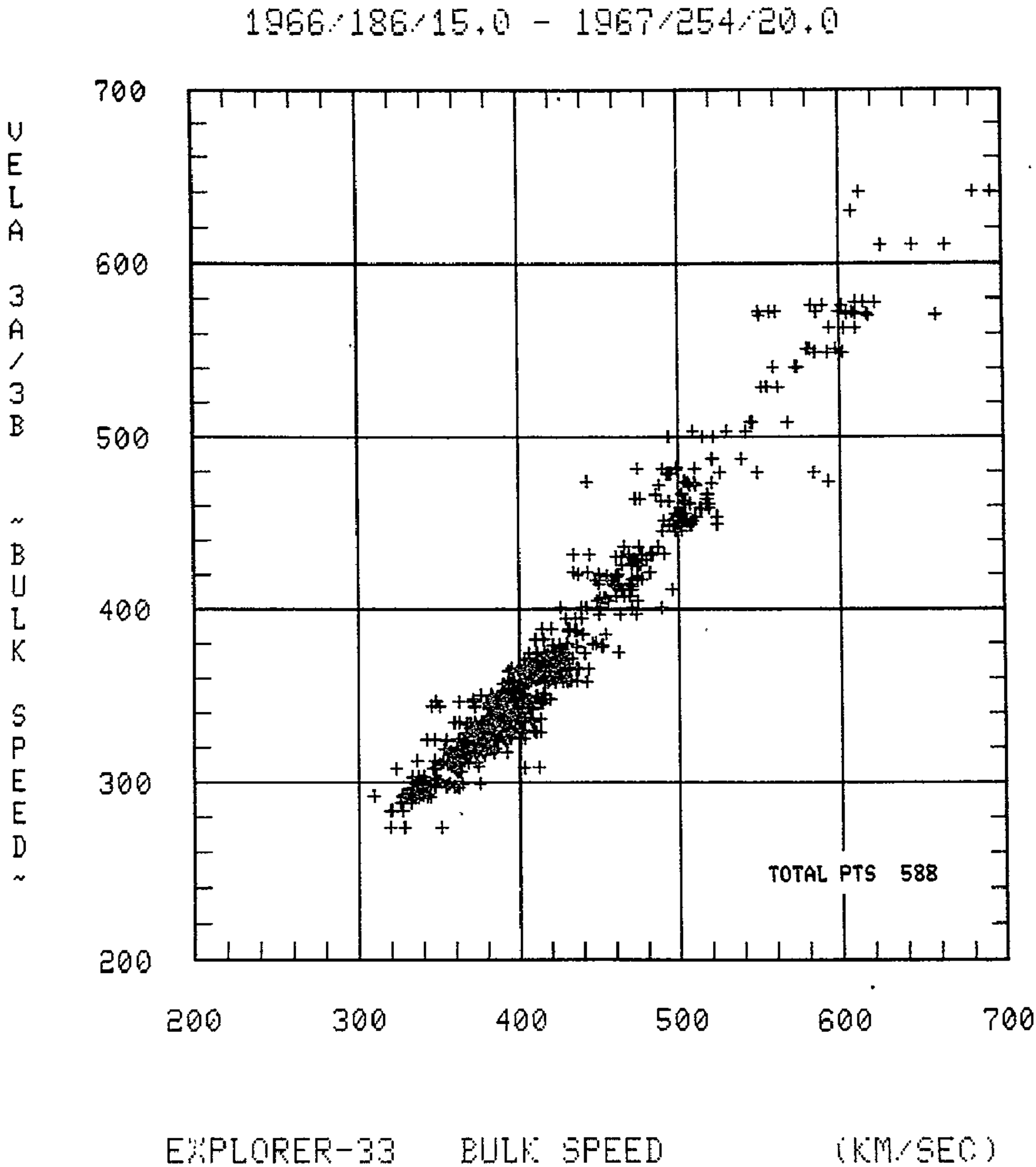

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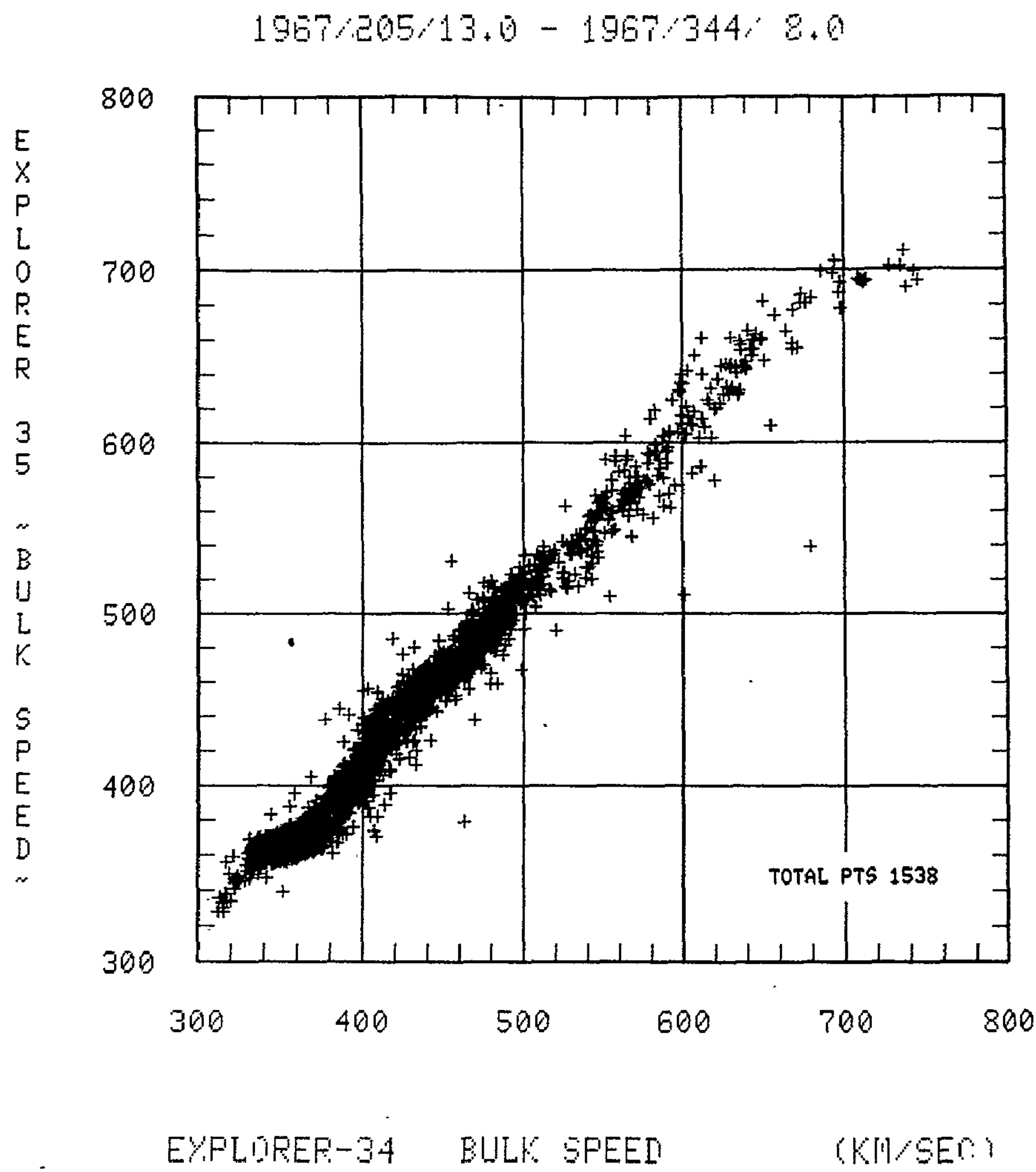

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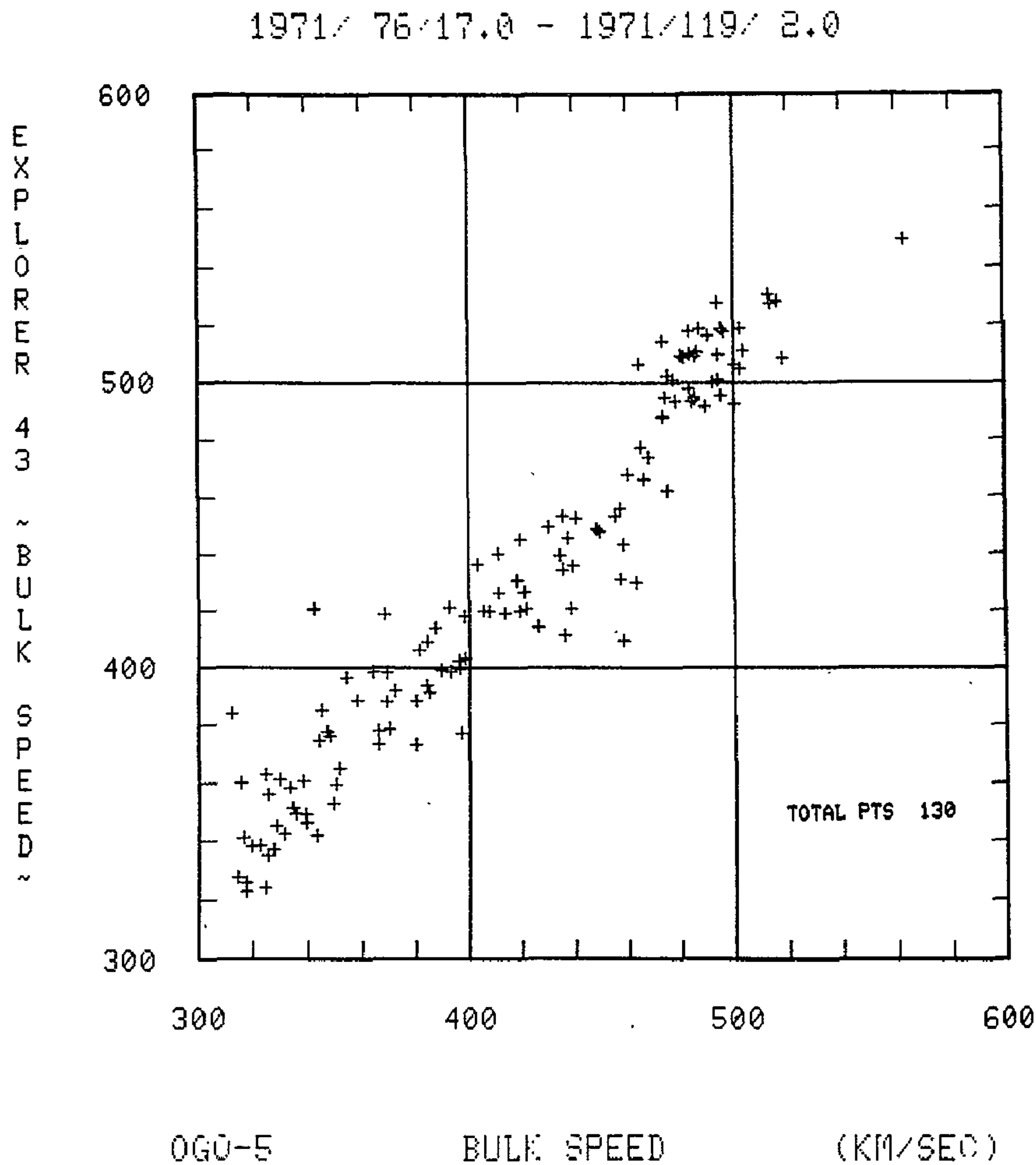

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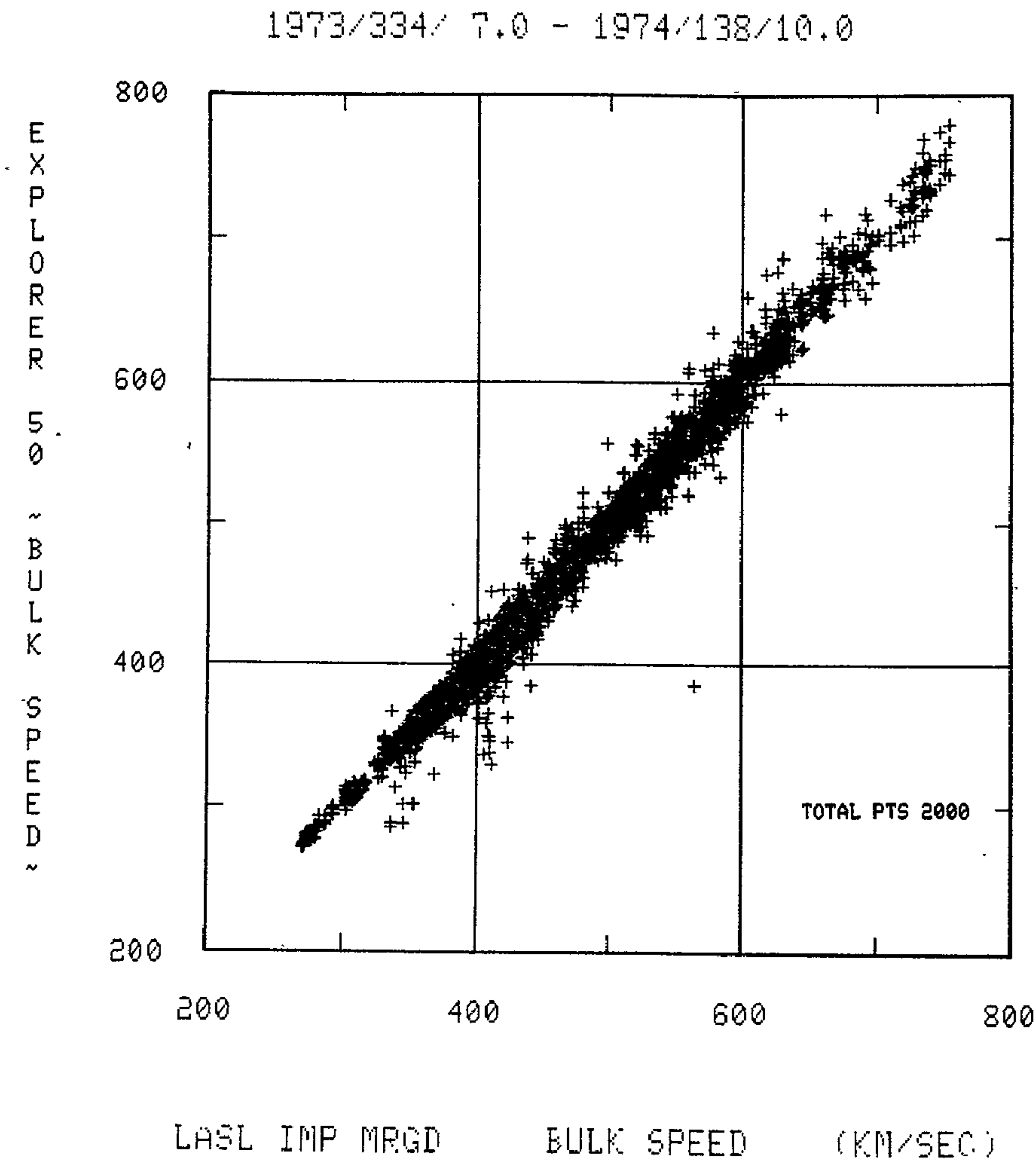

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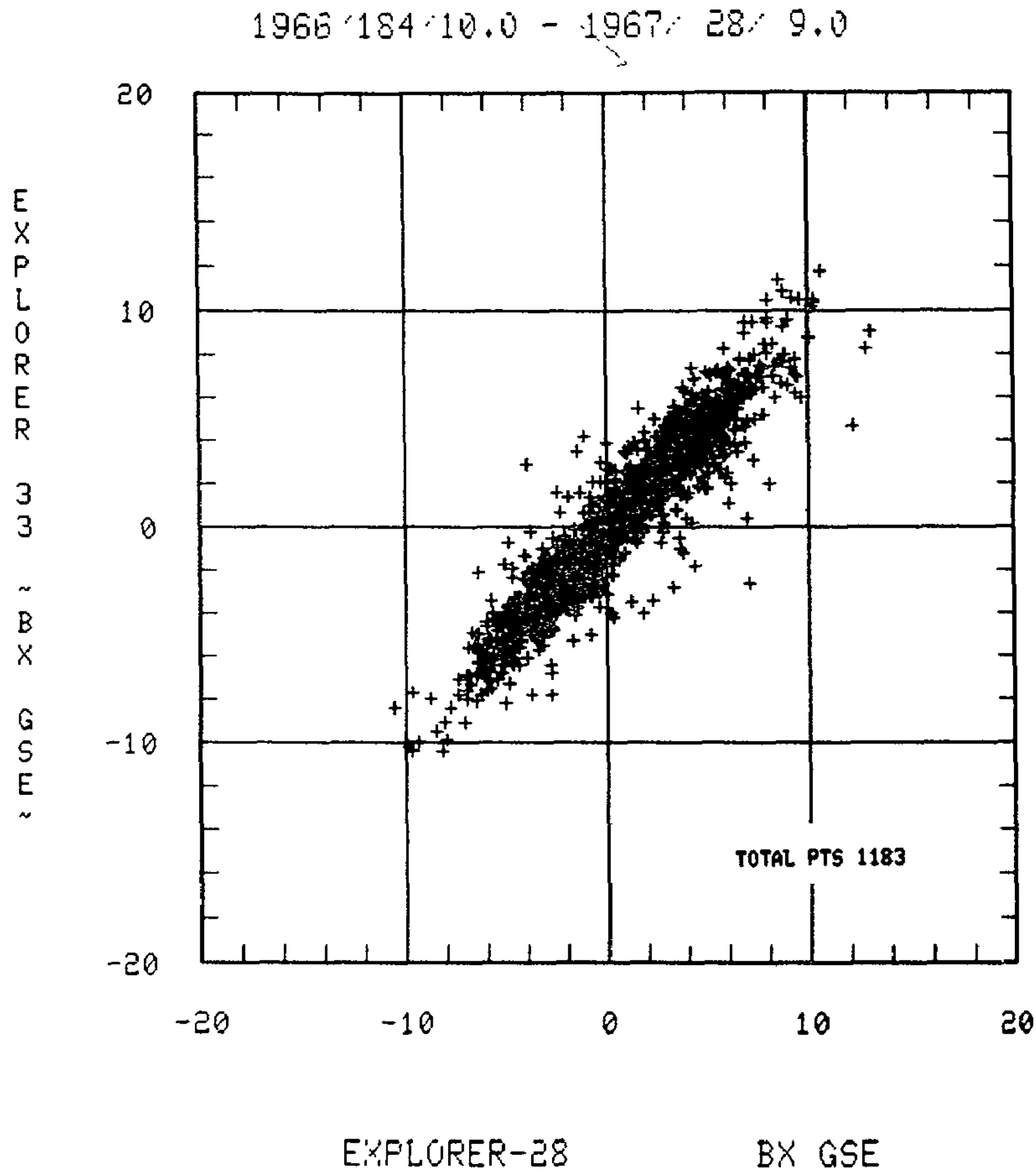

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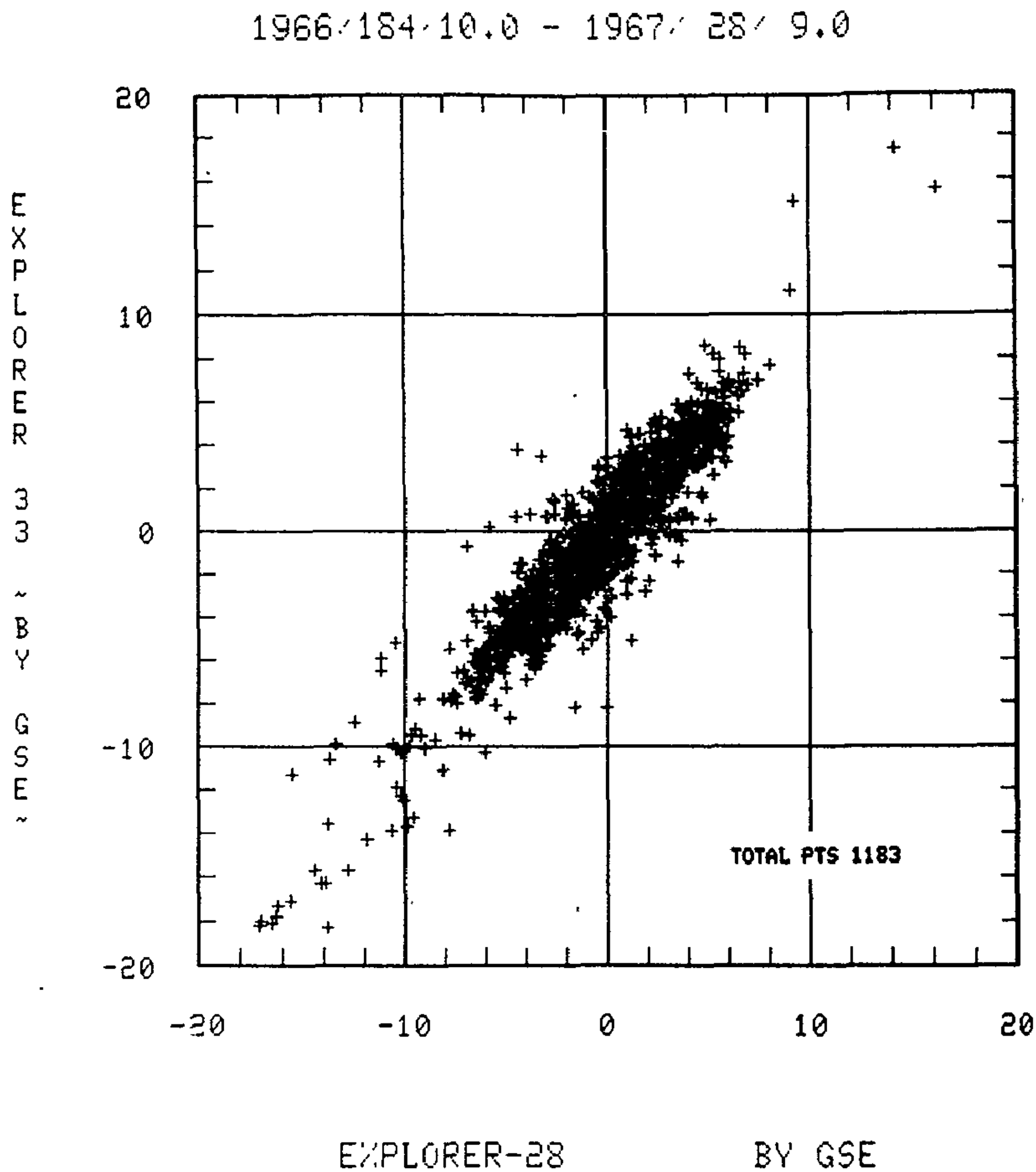

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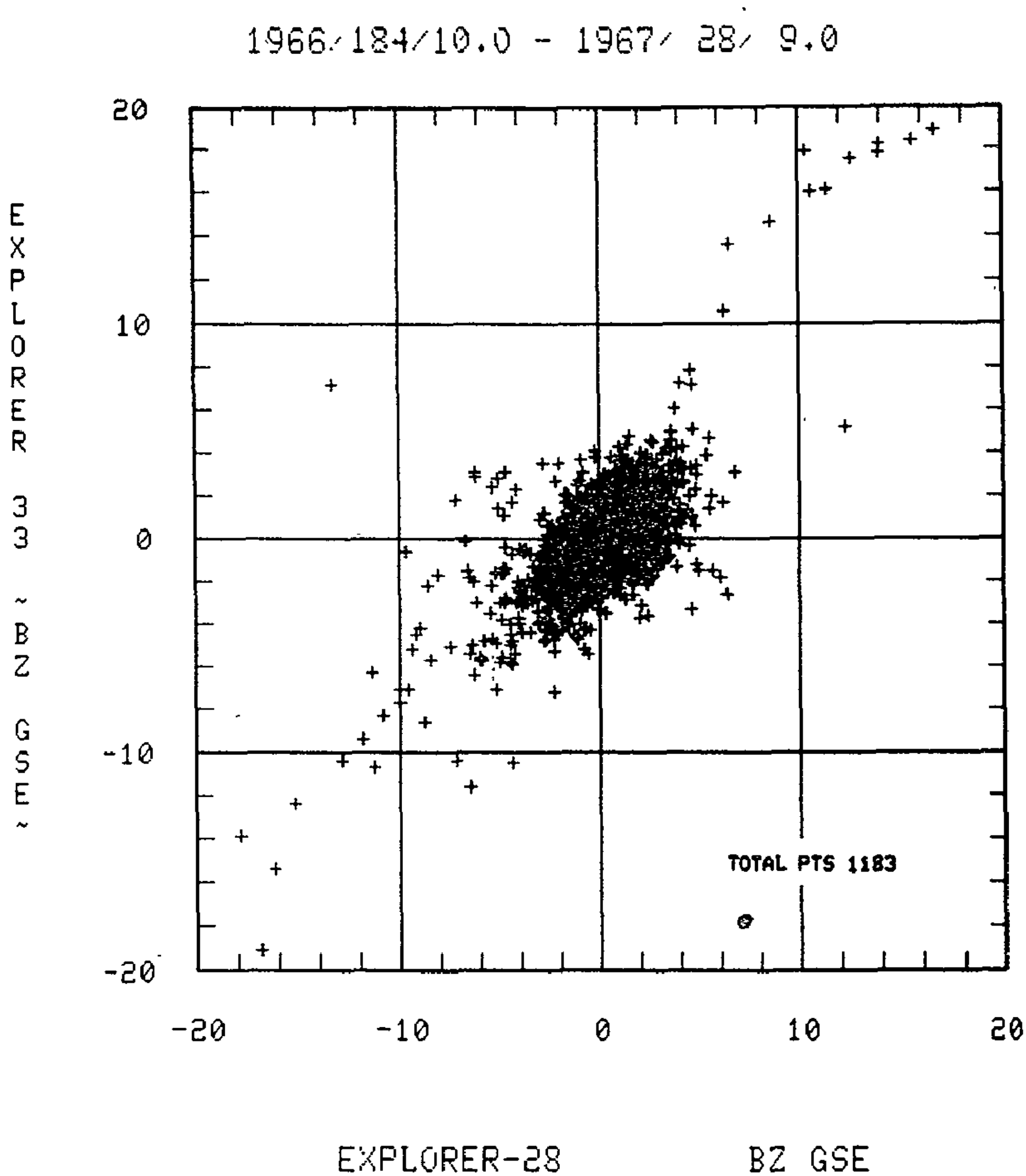

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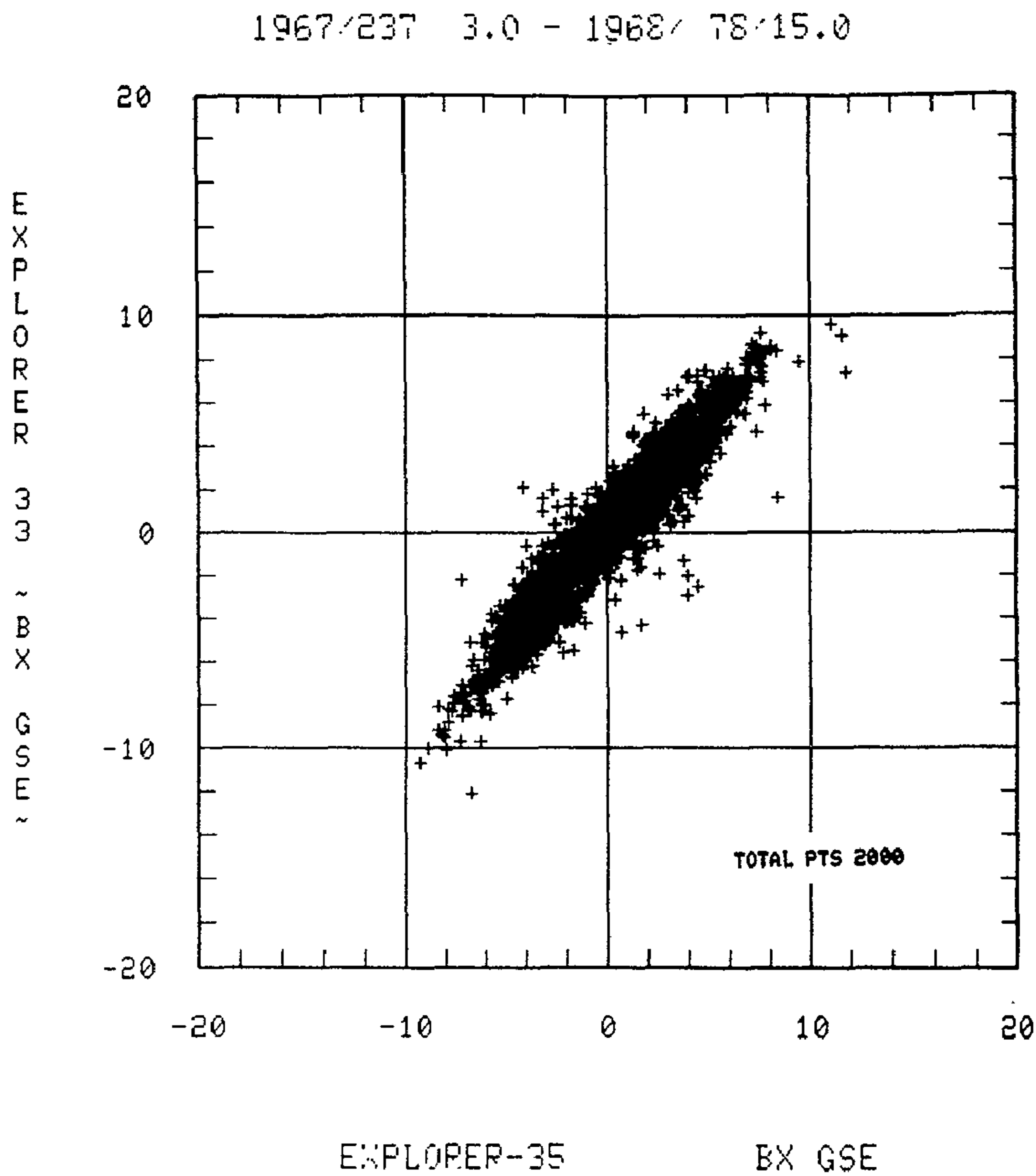

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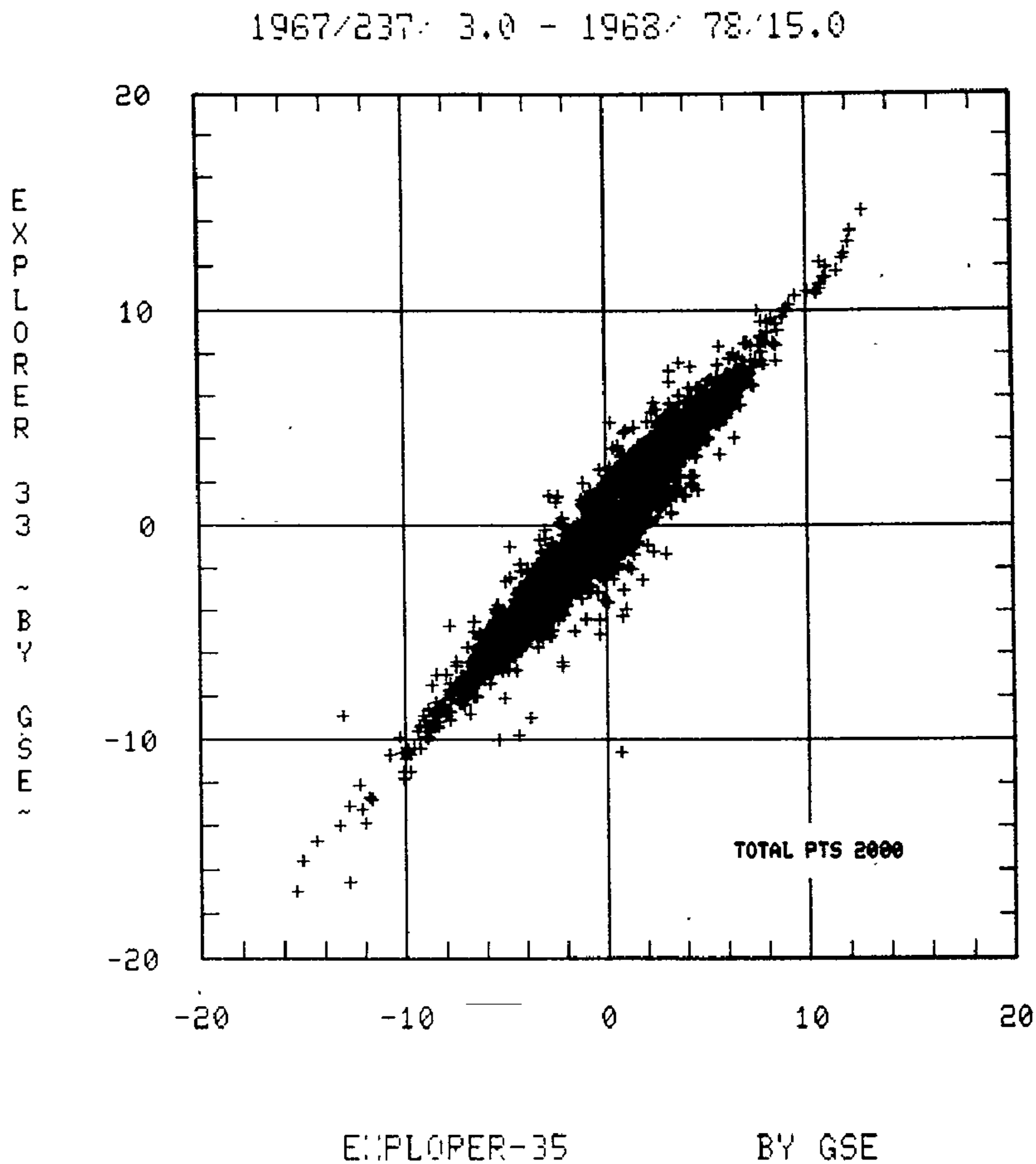

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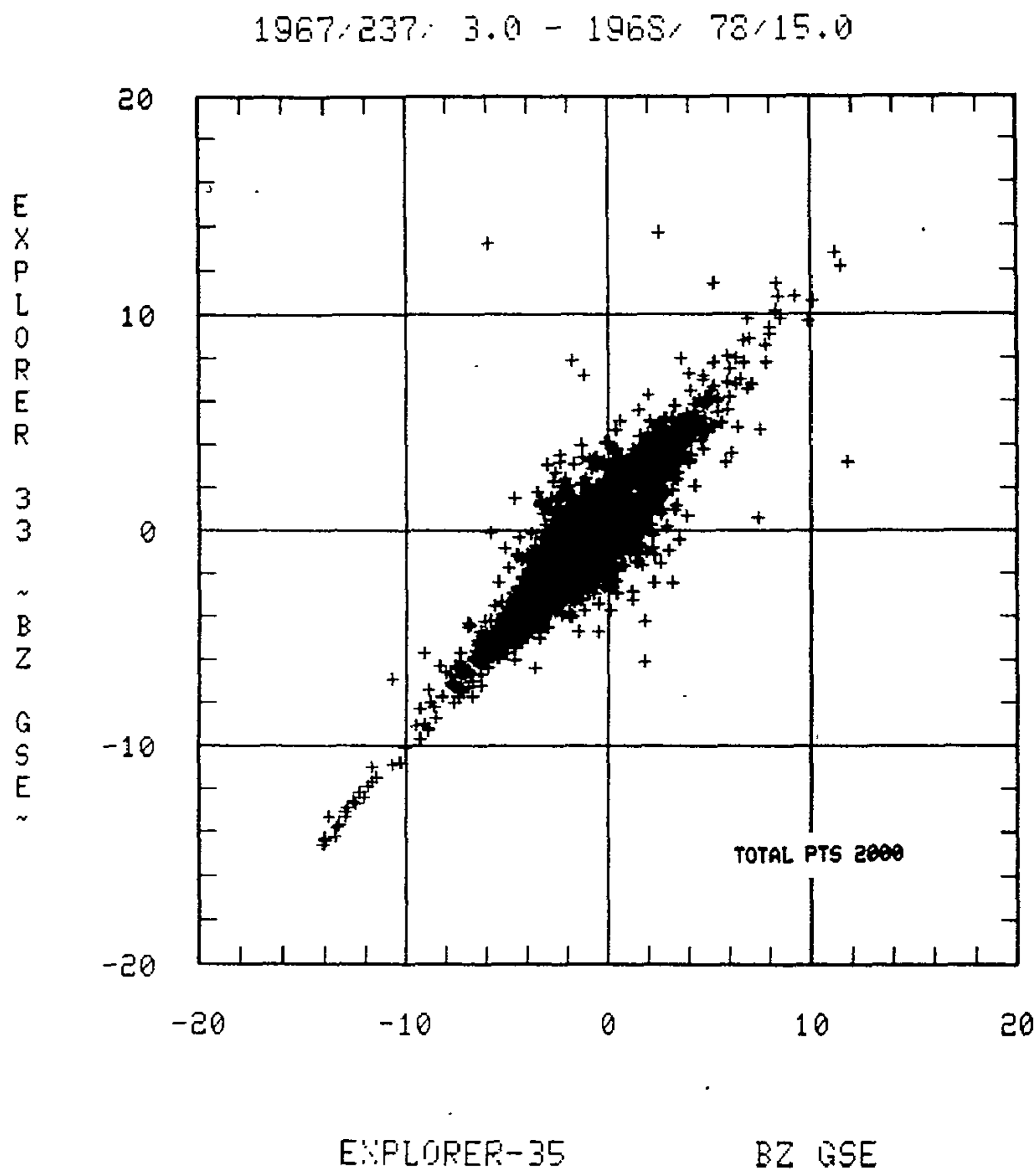

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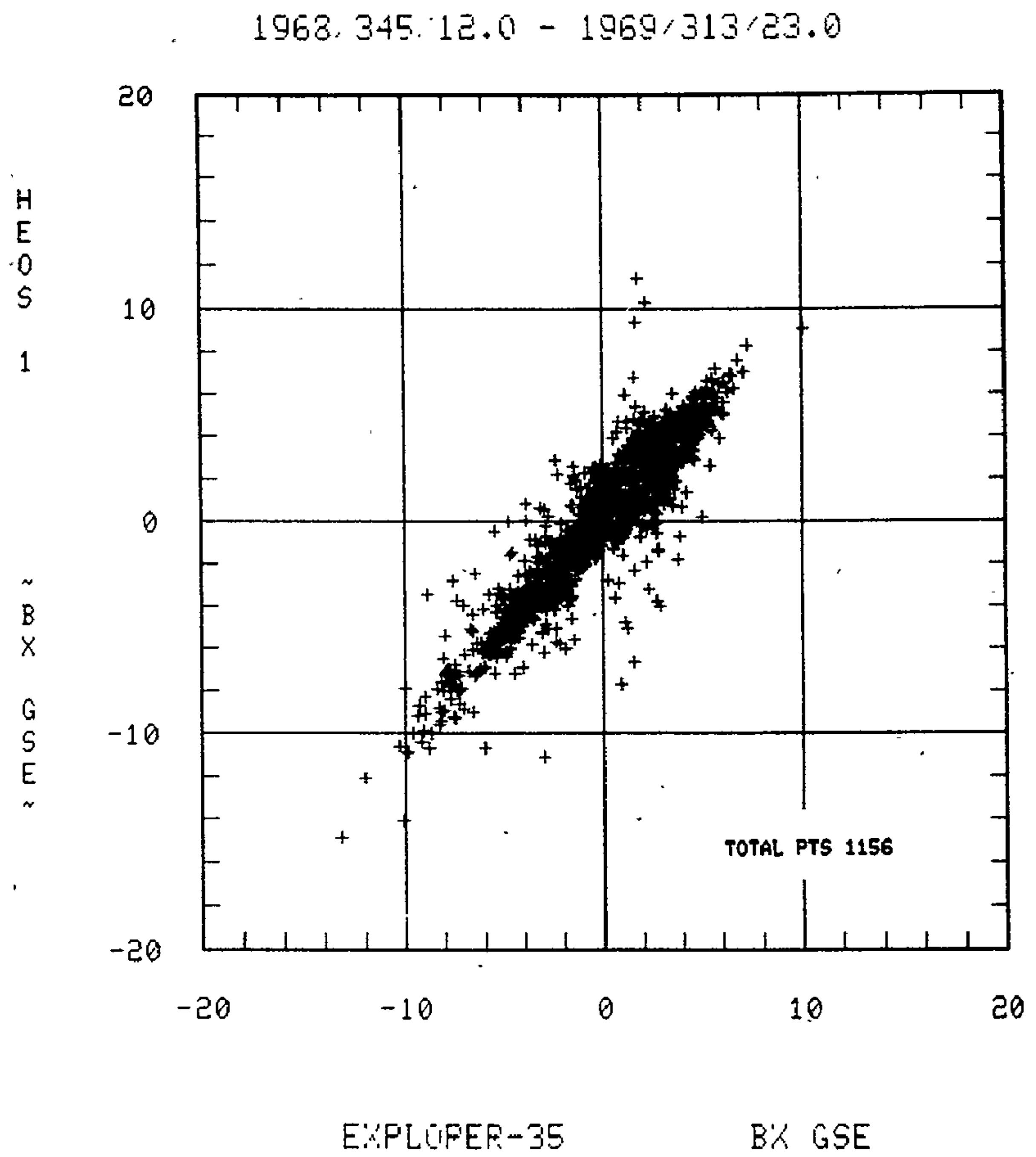

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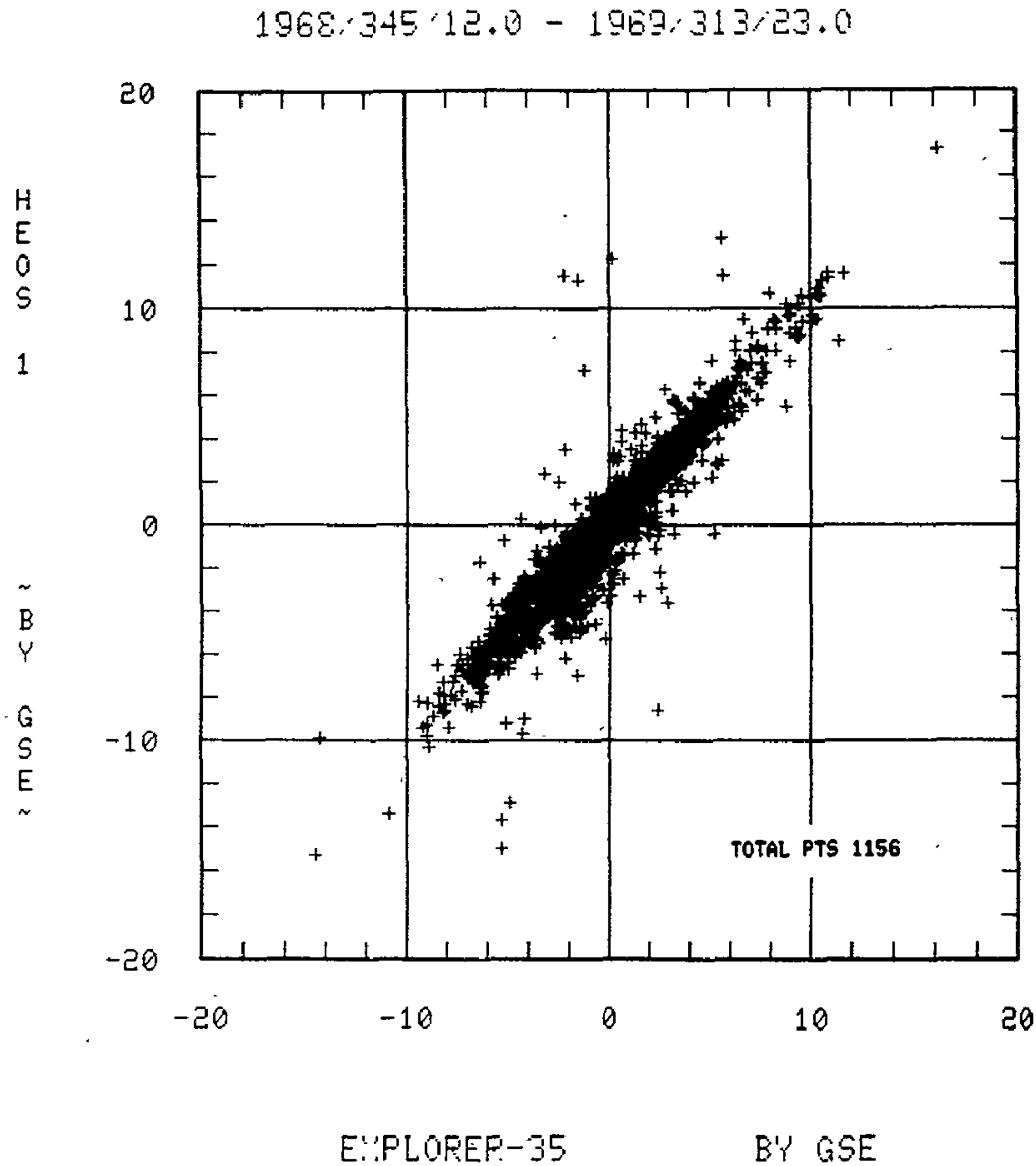

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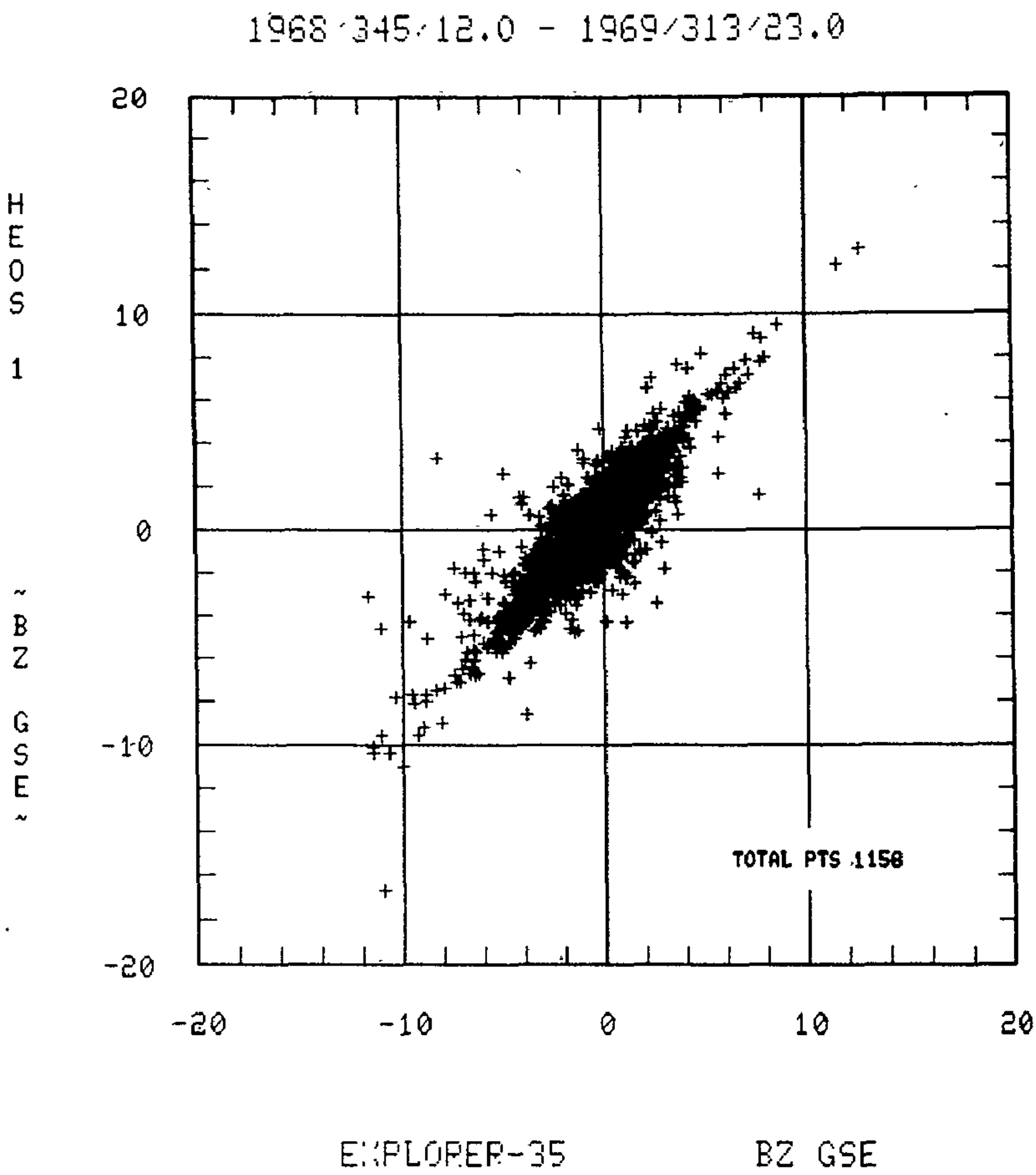

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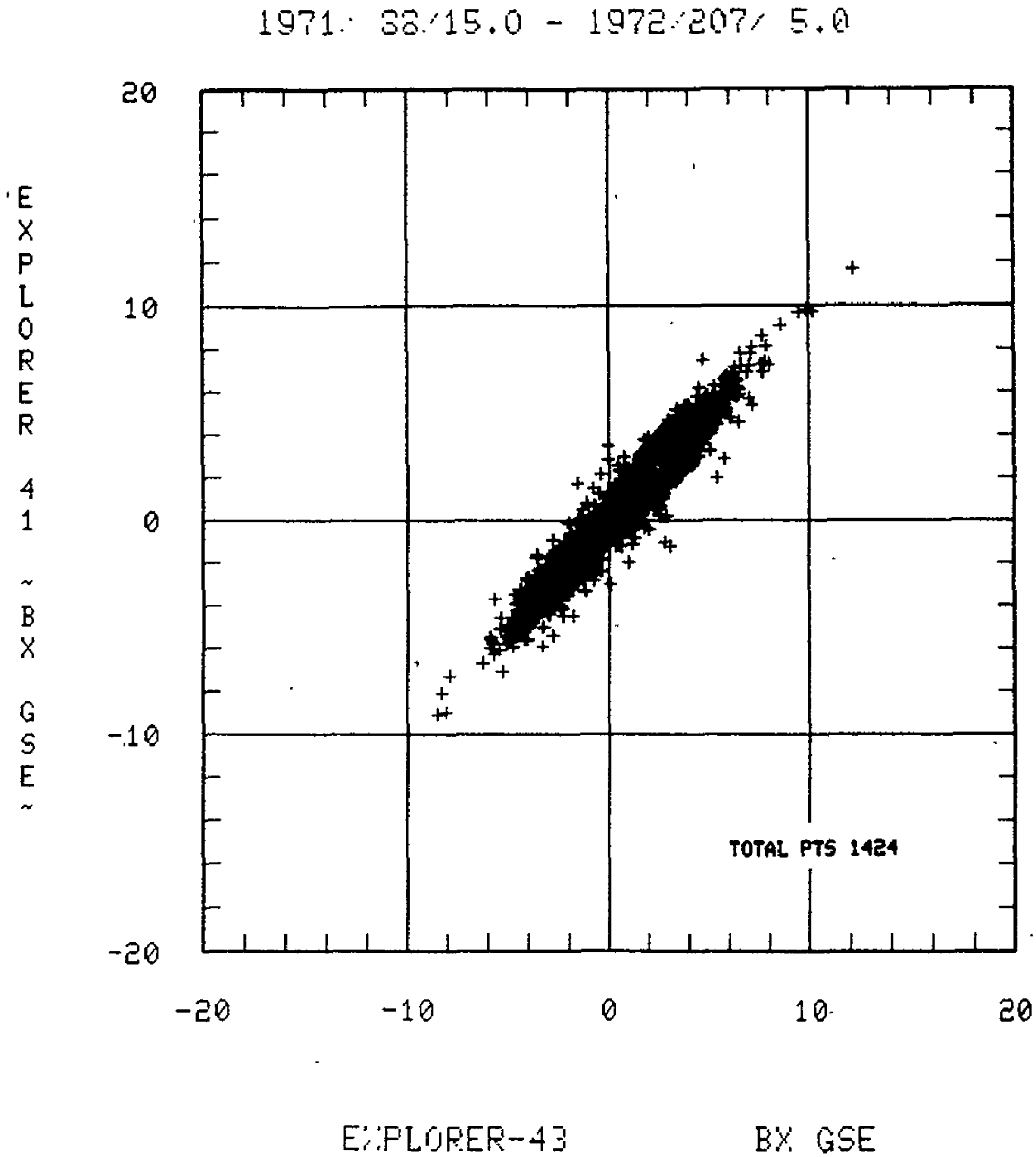

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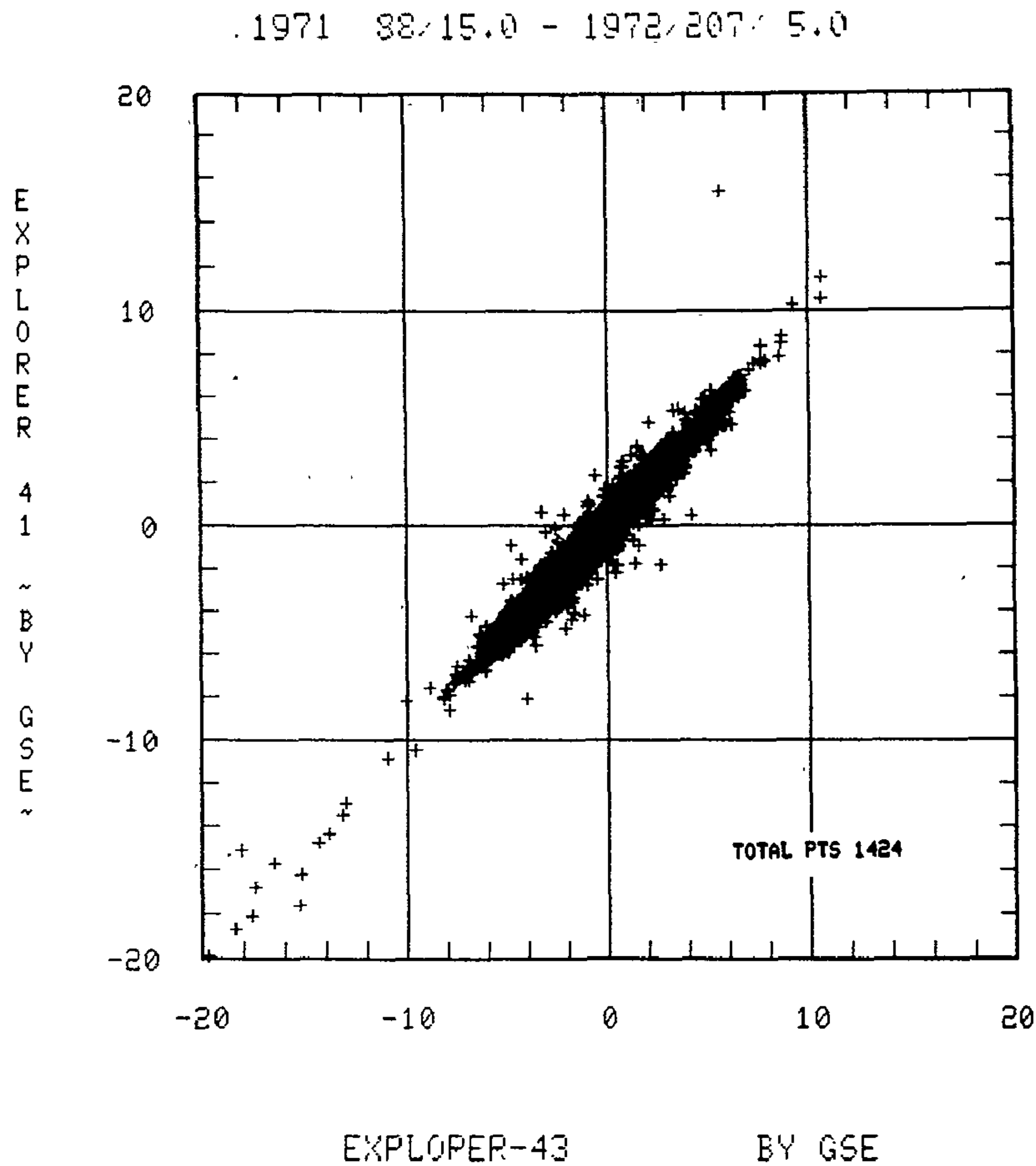

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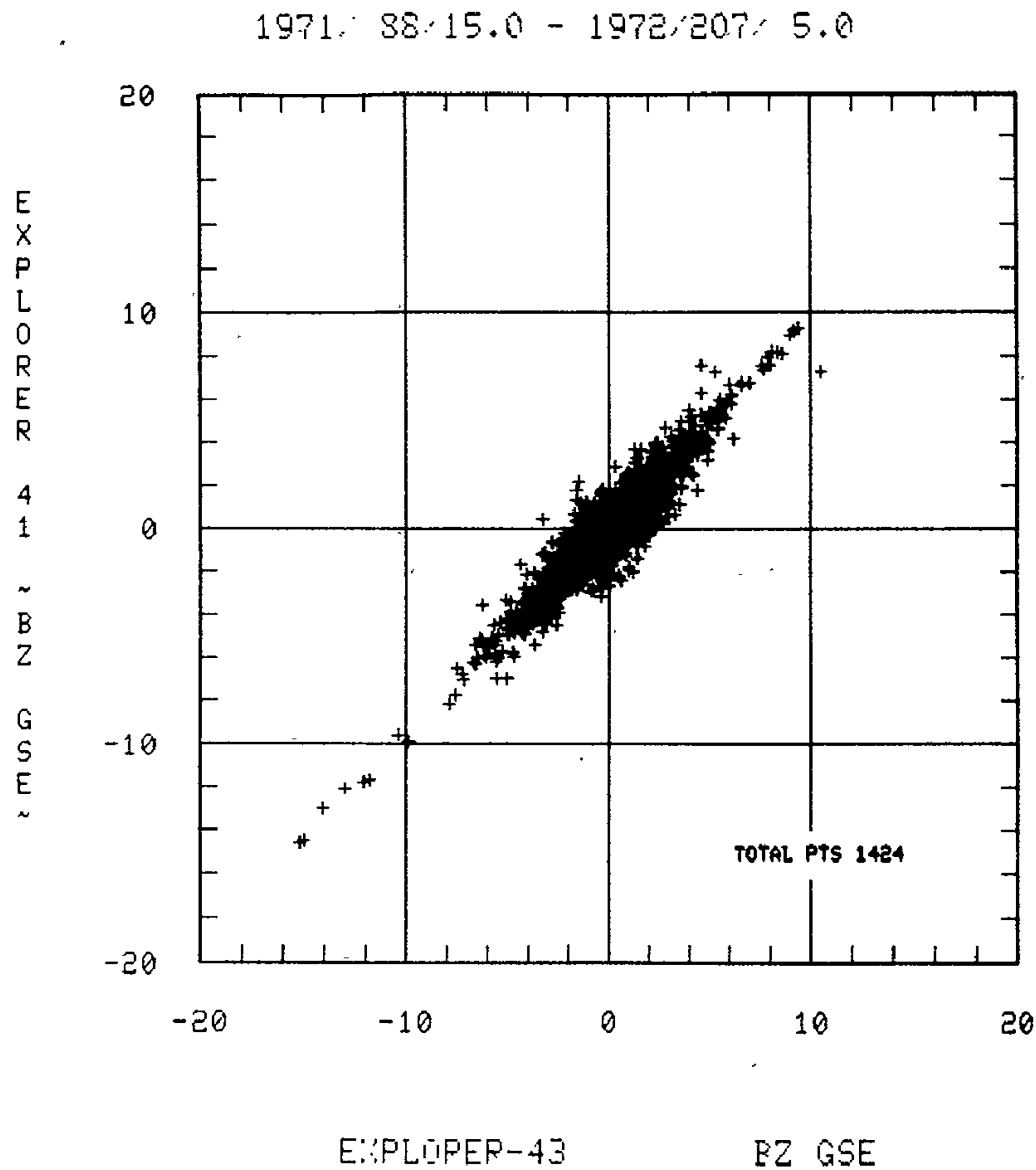

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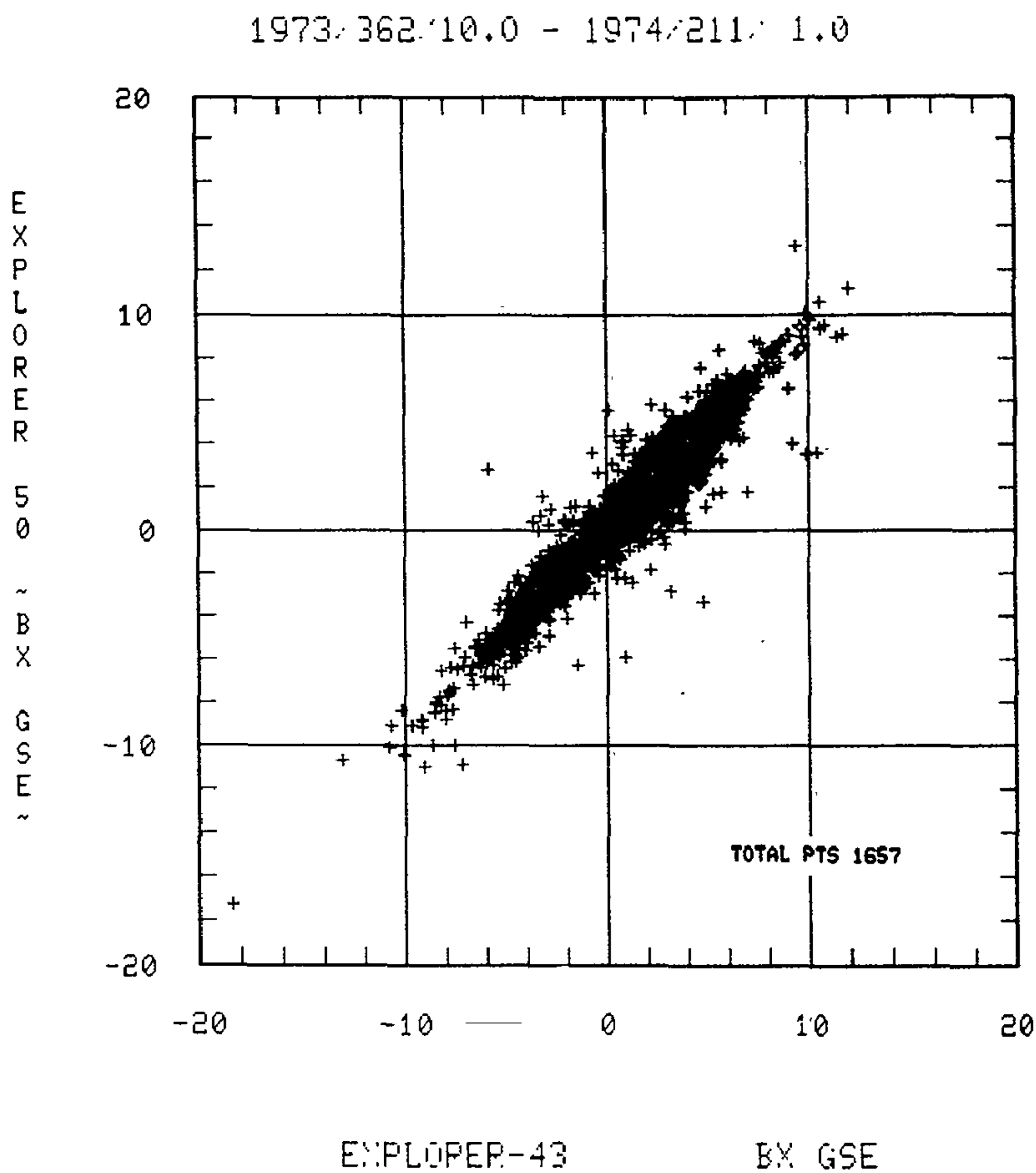

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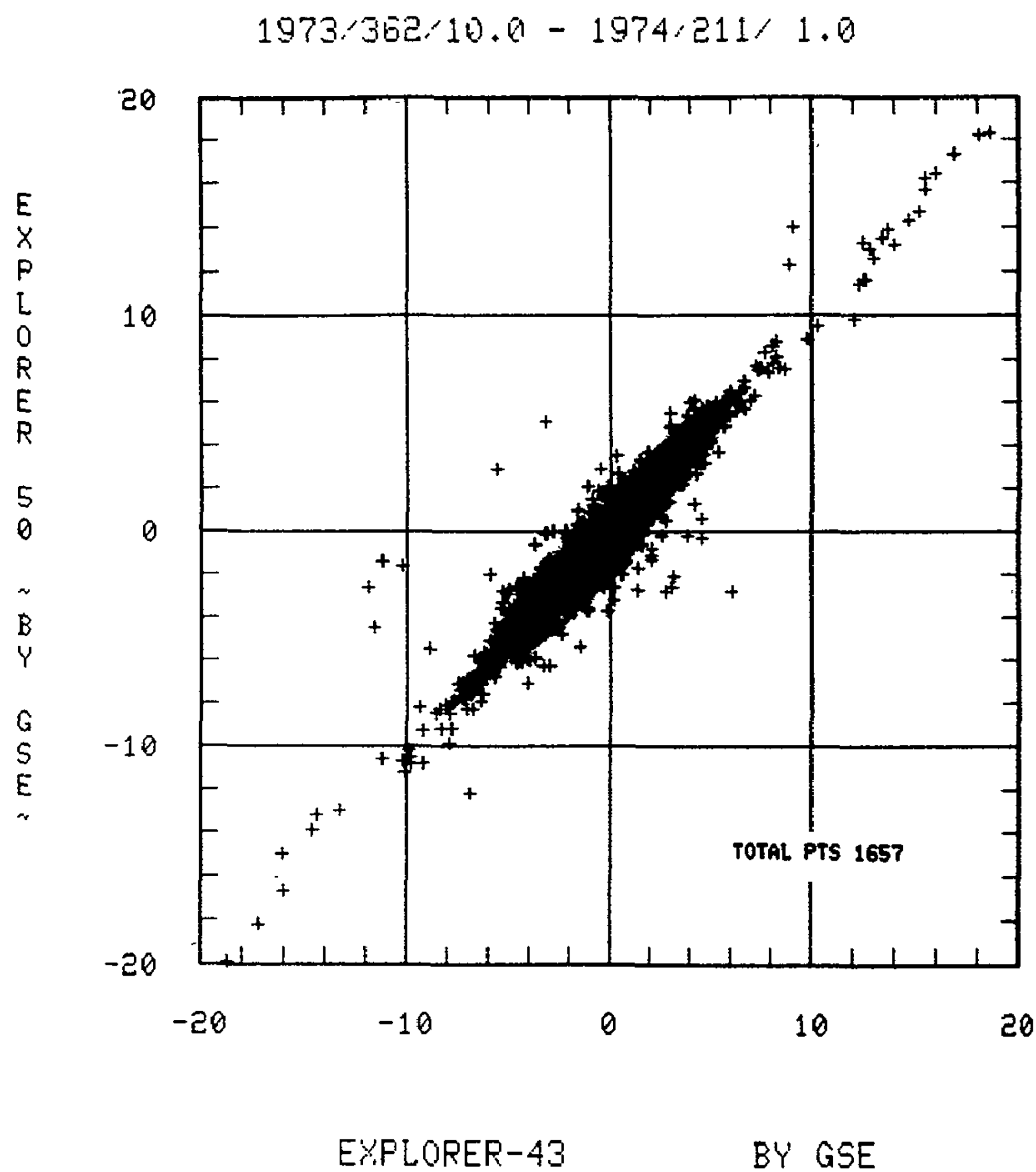

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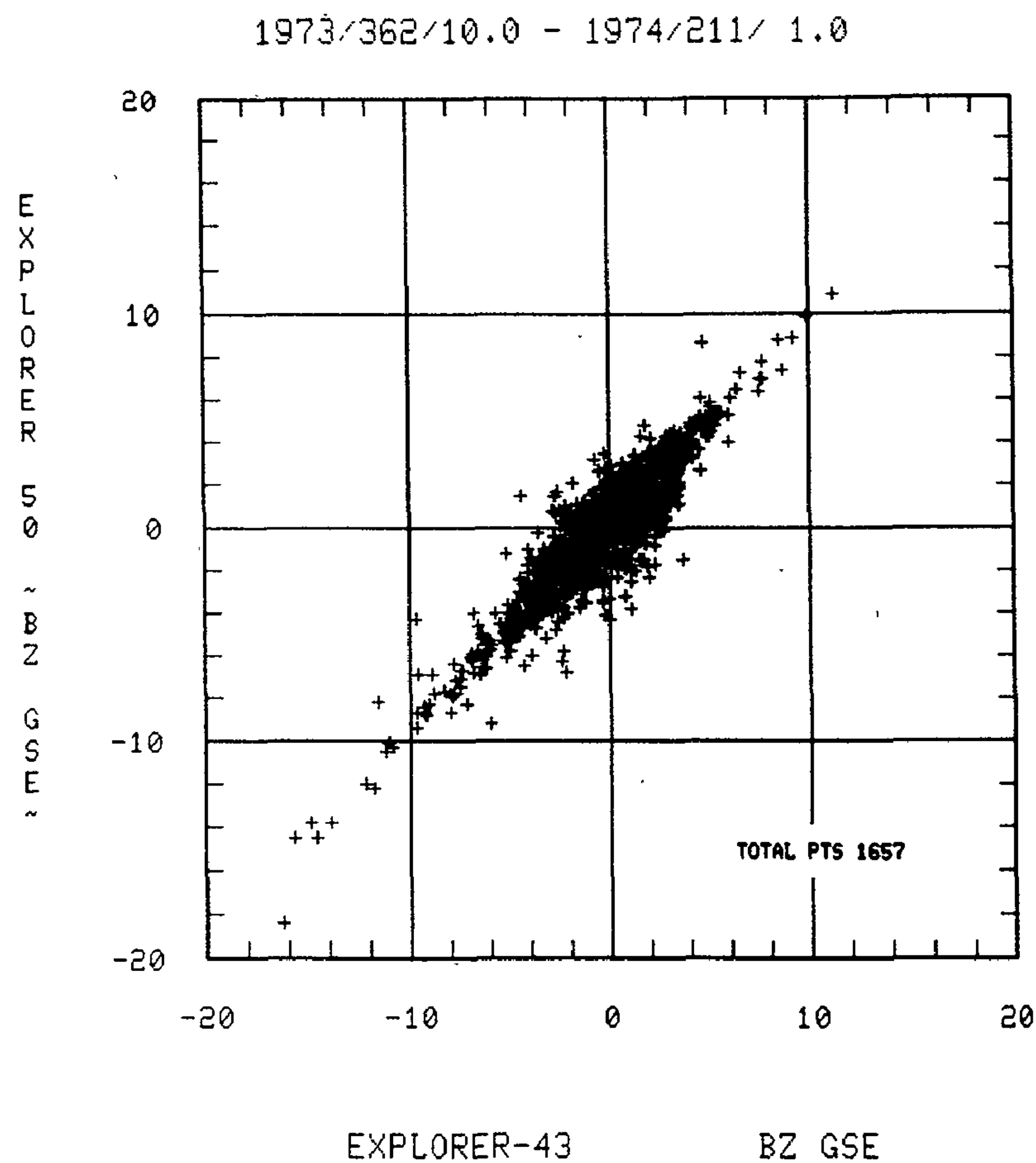
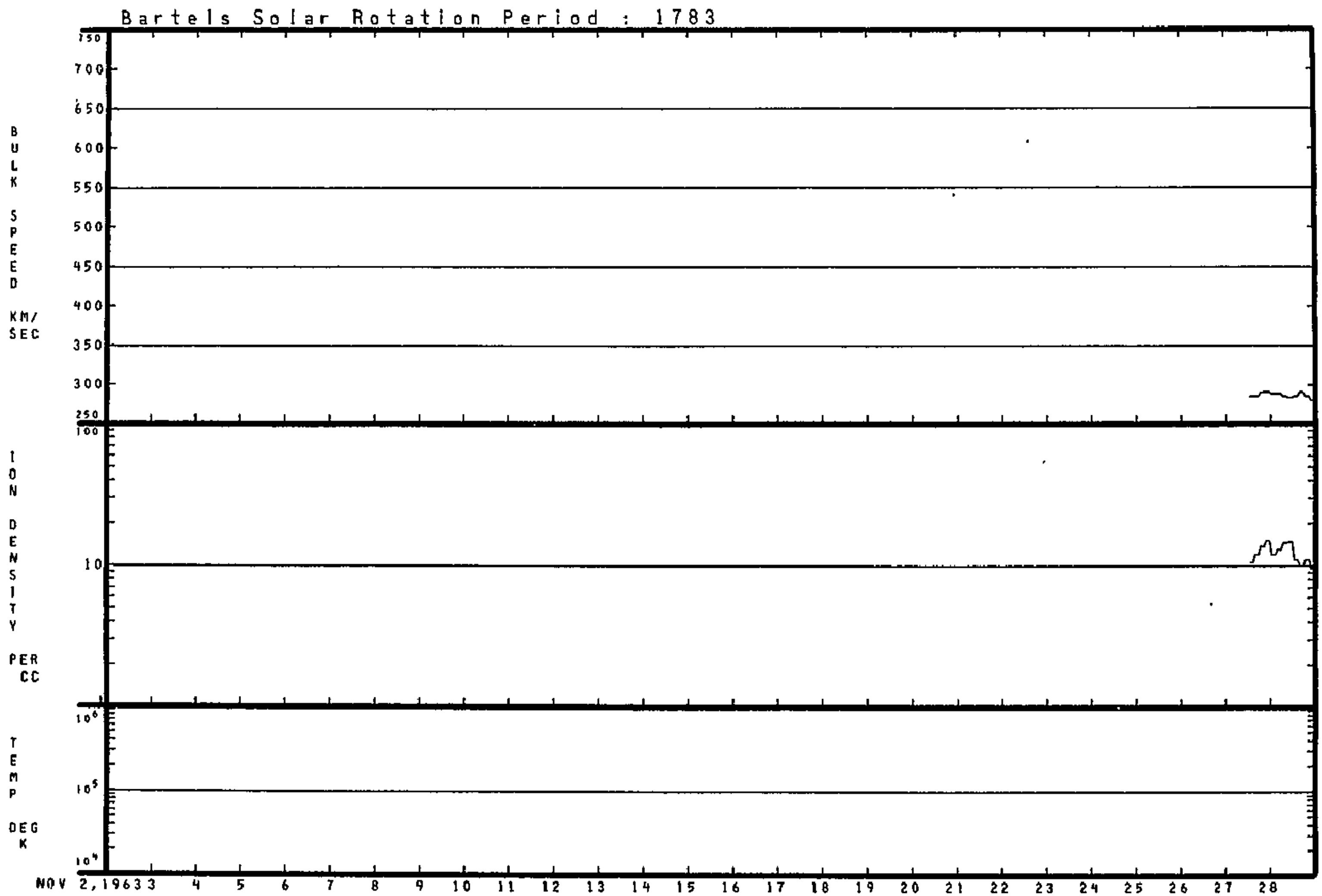


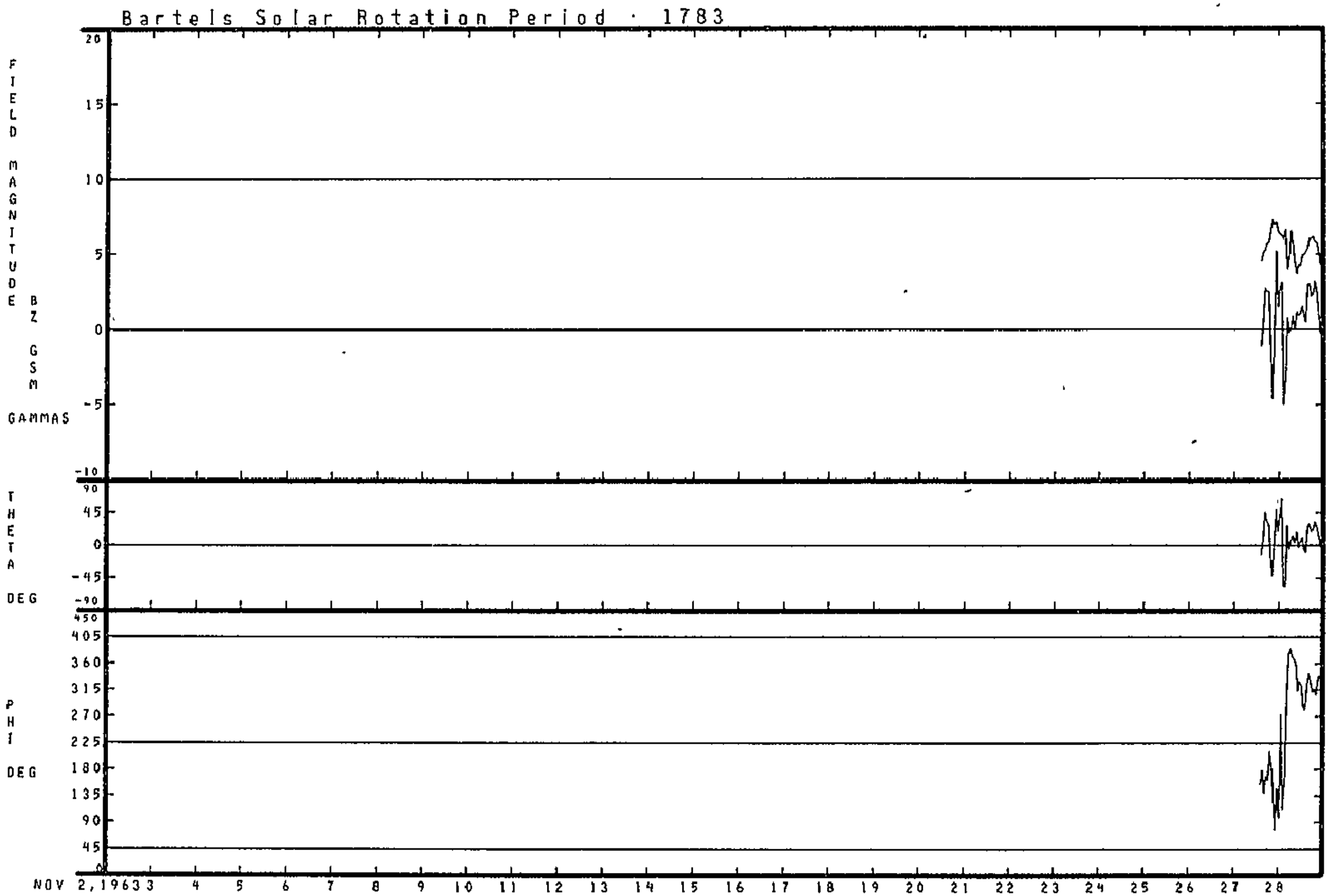
Scatter Plot 27

INTENSITY VERSUS TIME PROFILES

The following pages contain profiles of interplanetary plasma and magnetic field data covering the time period November 27, 1963, to December 30, 1975.

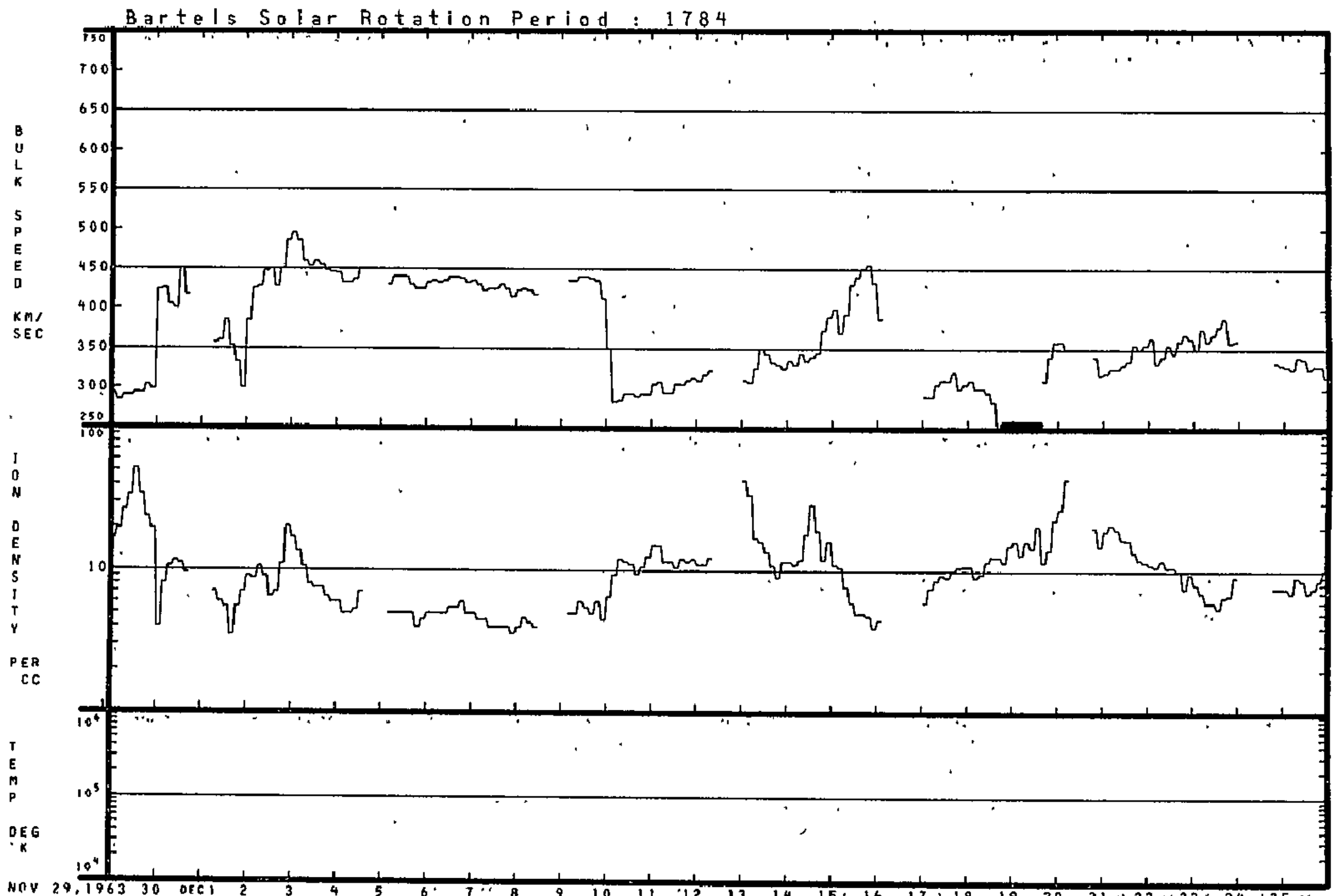
11/02/63 - 11/28/63



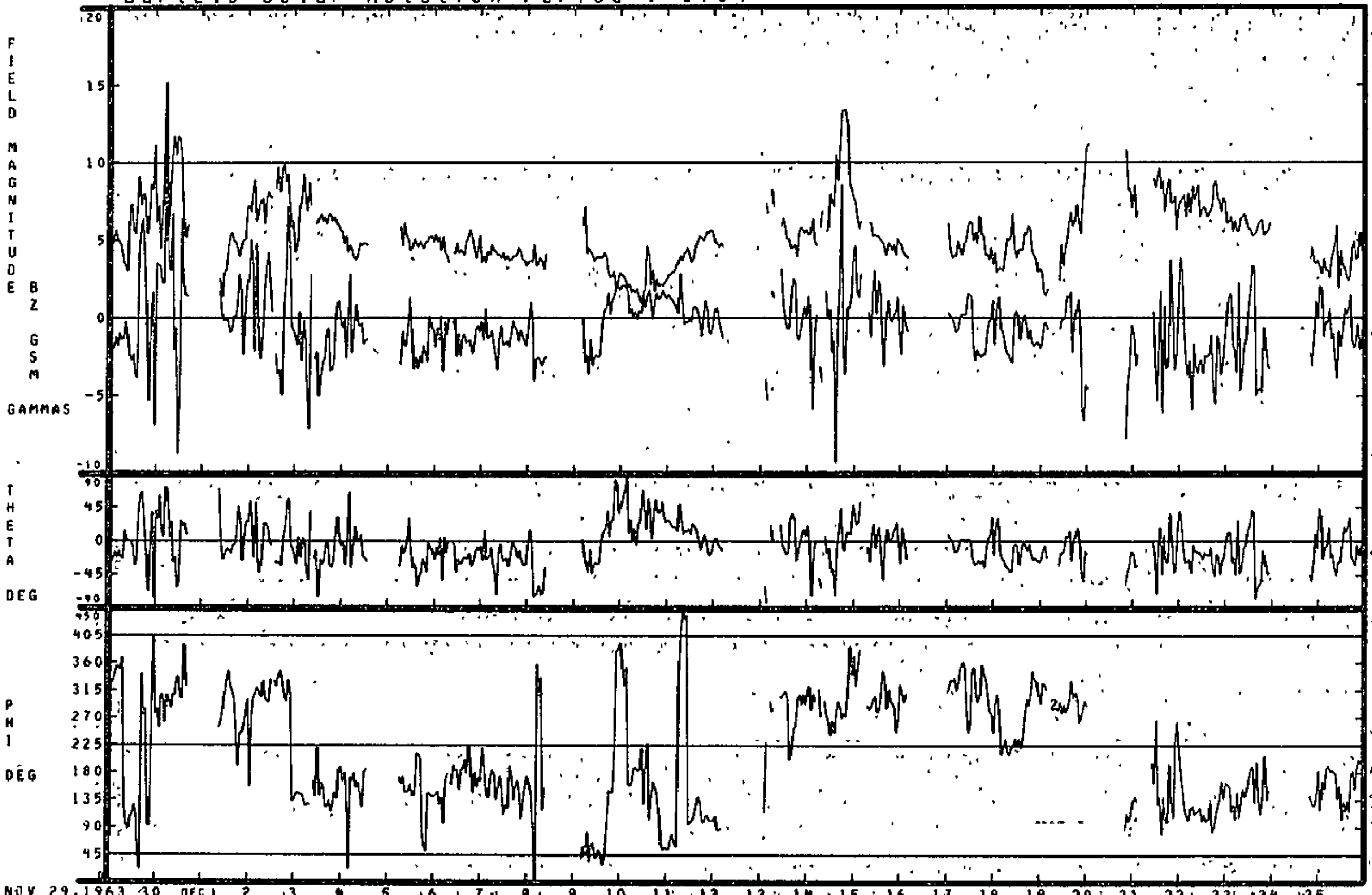


11/02/63 - 11/28/63

11/29/63 - 12/25/63

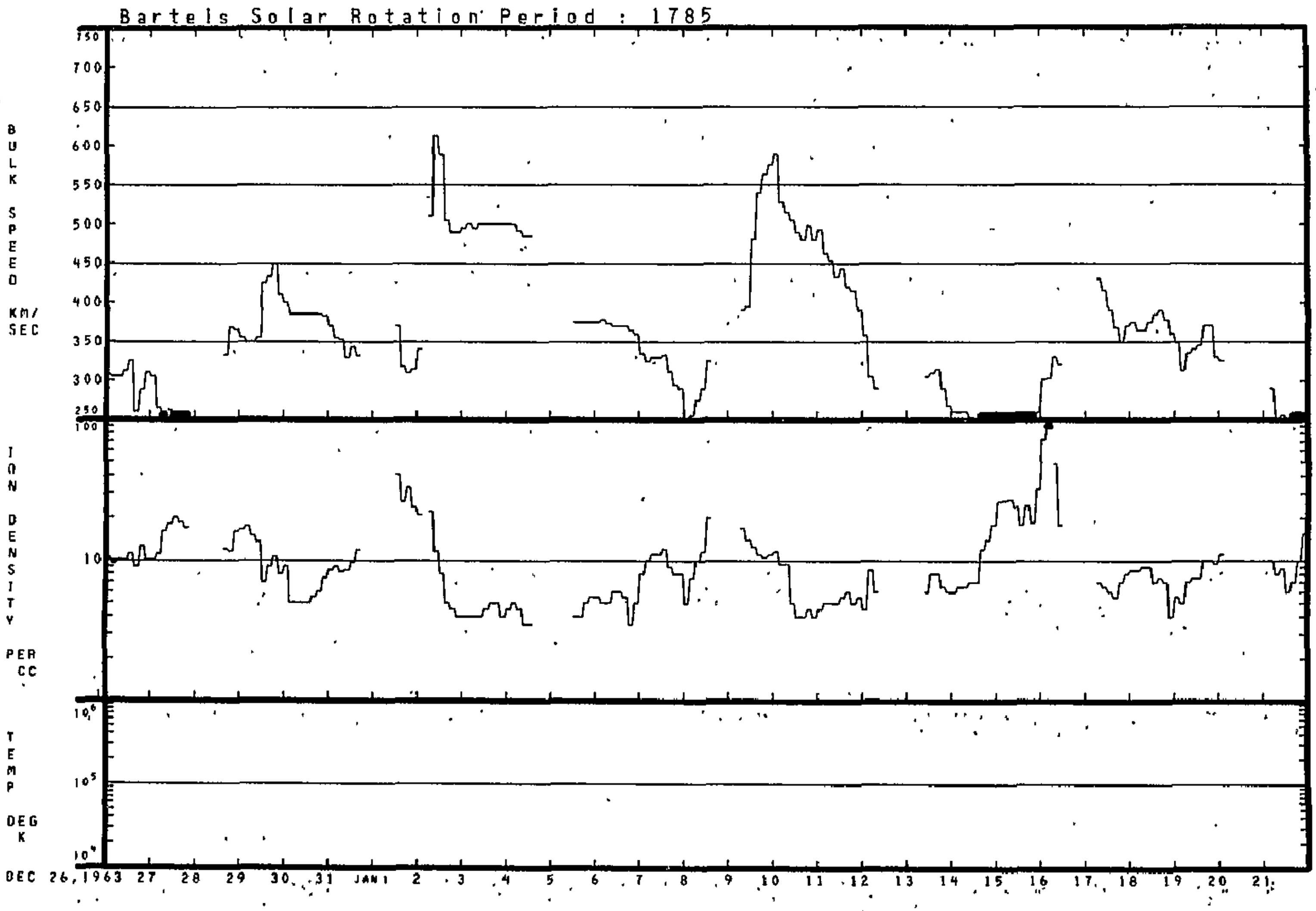


Bartels Solar Rotation Period : 1784



11/29/63 - 12/25/63

12/26/63 - 01/21/64



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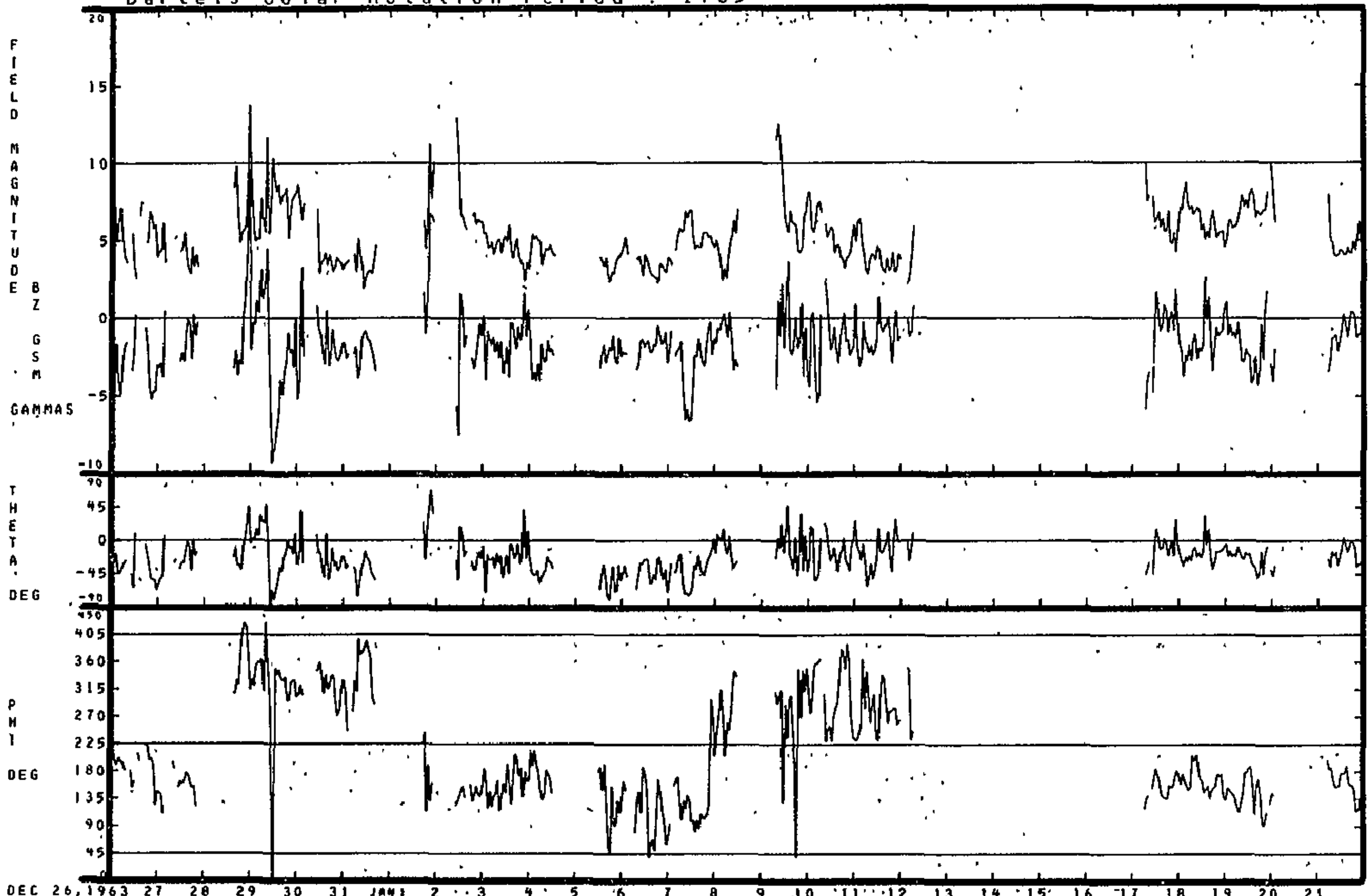
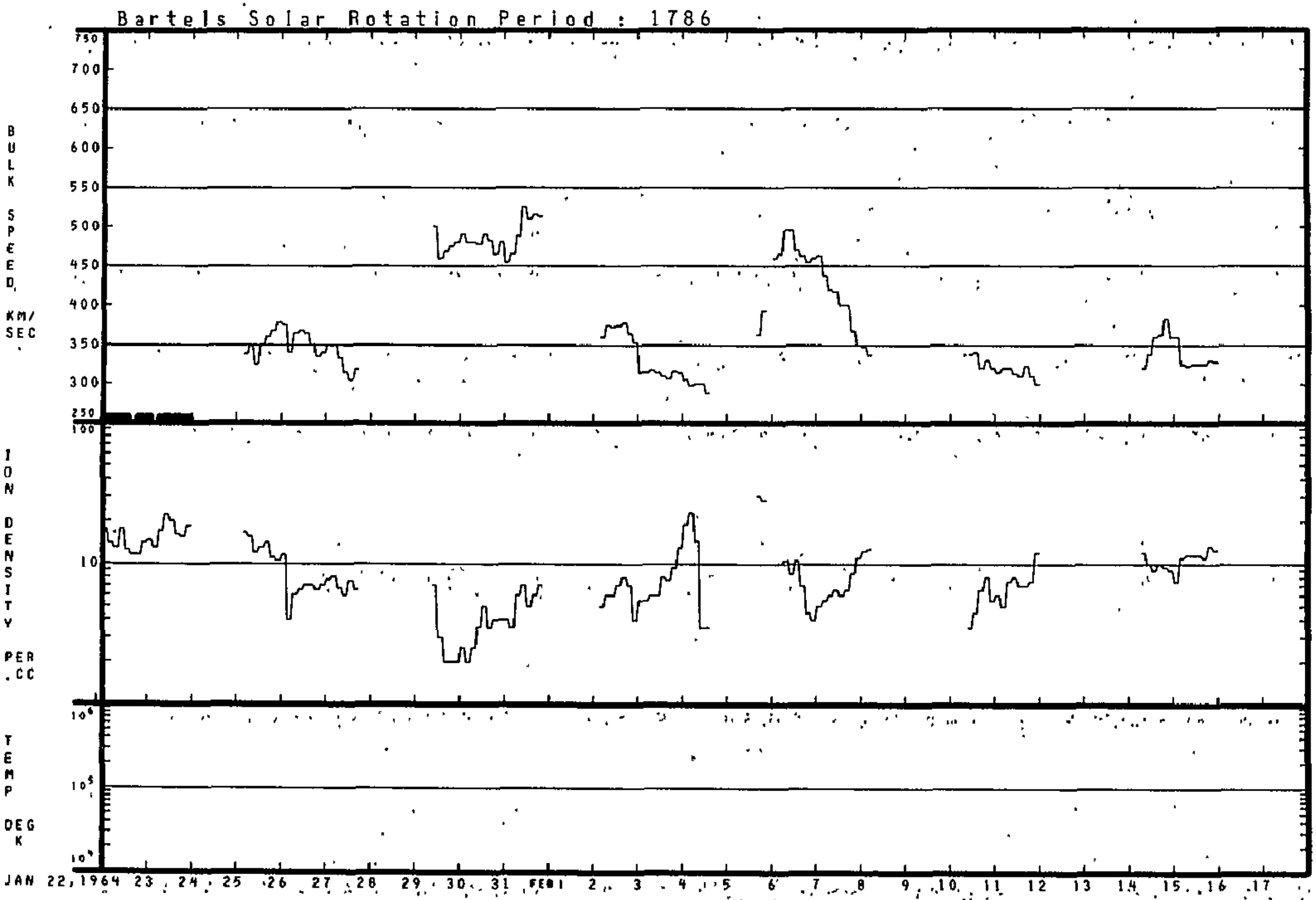
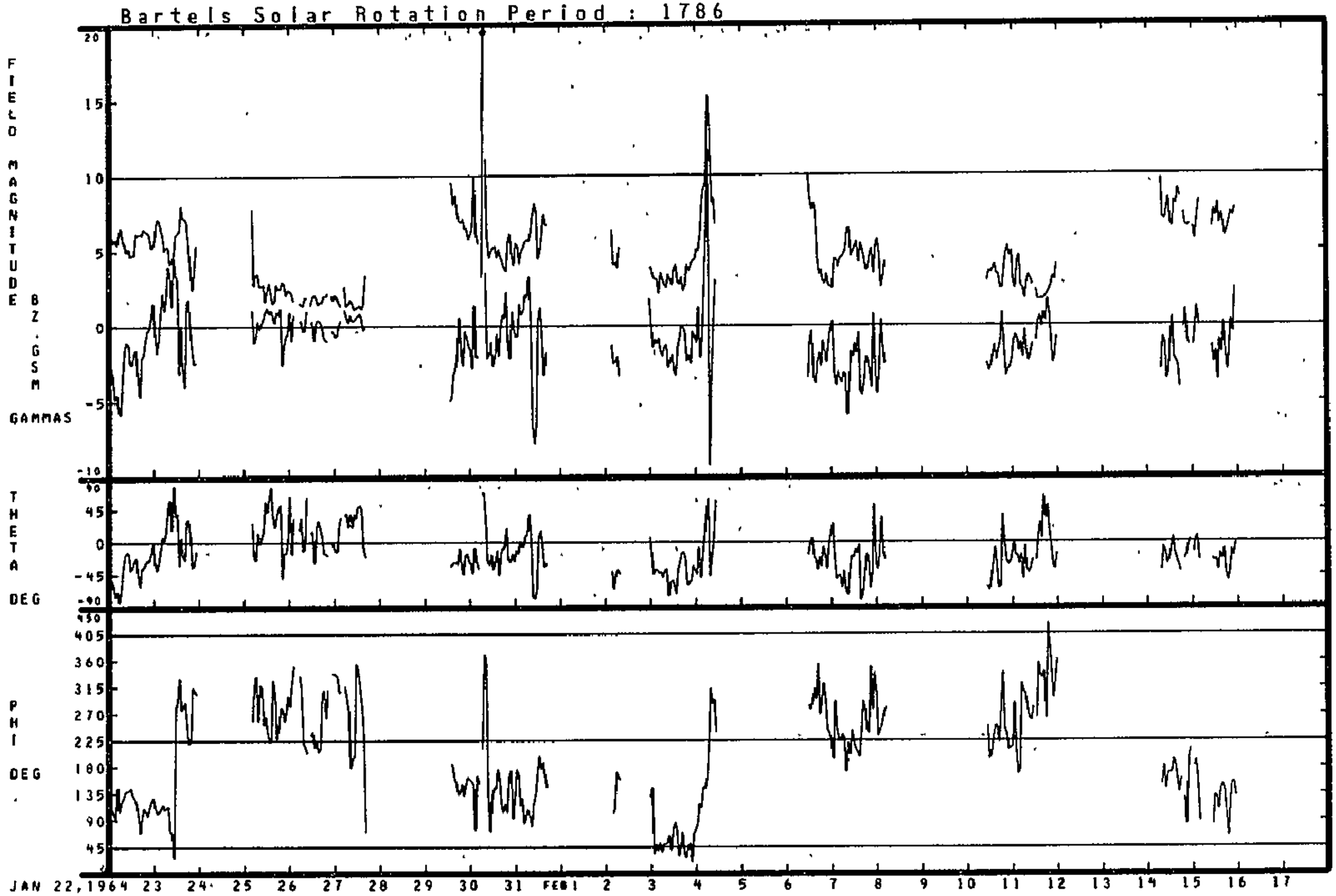


12/26/63 - 01/21/64

01/22/64 - 02/17/64

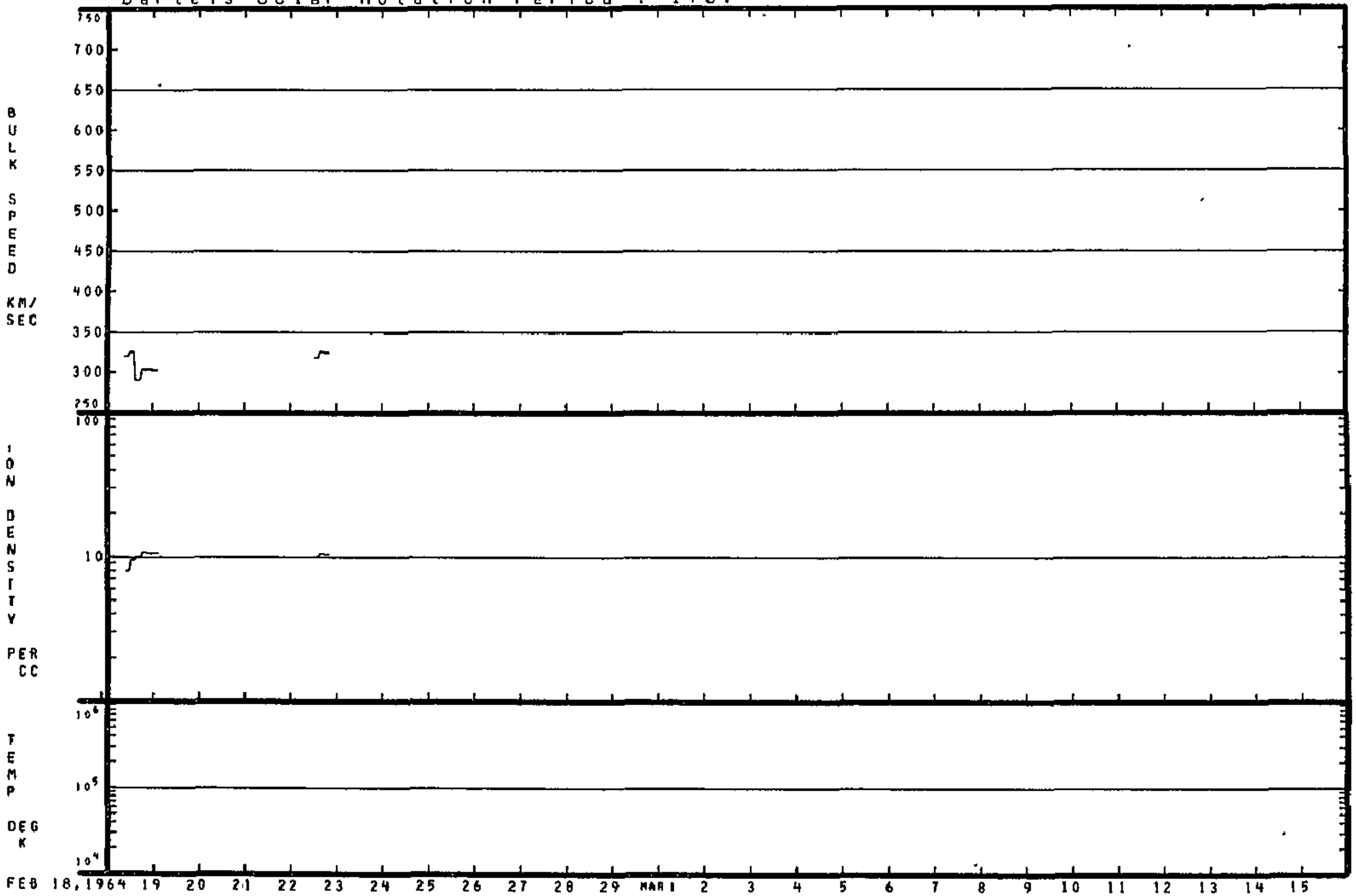


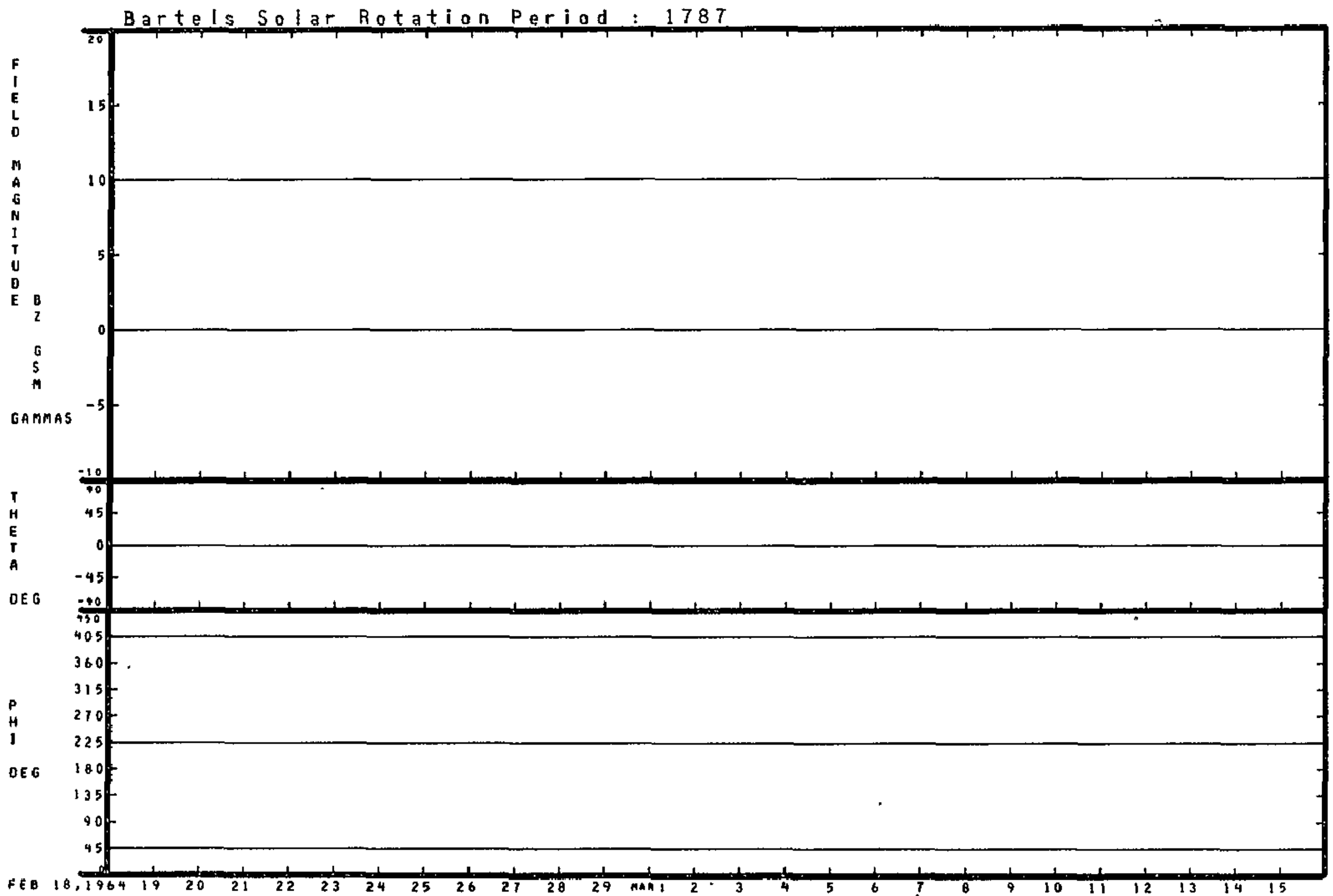


01/22/64 - 02/17/64

02/18/64 - 03/15/64

Bartels Solar Rotation Period : 1787

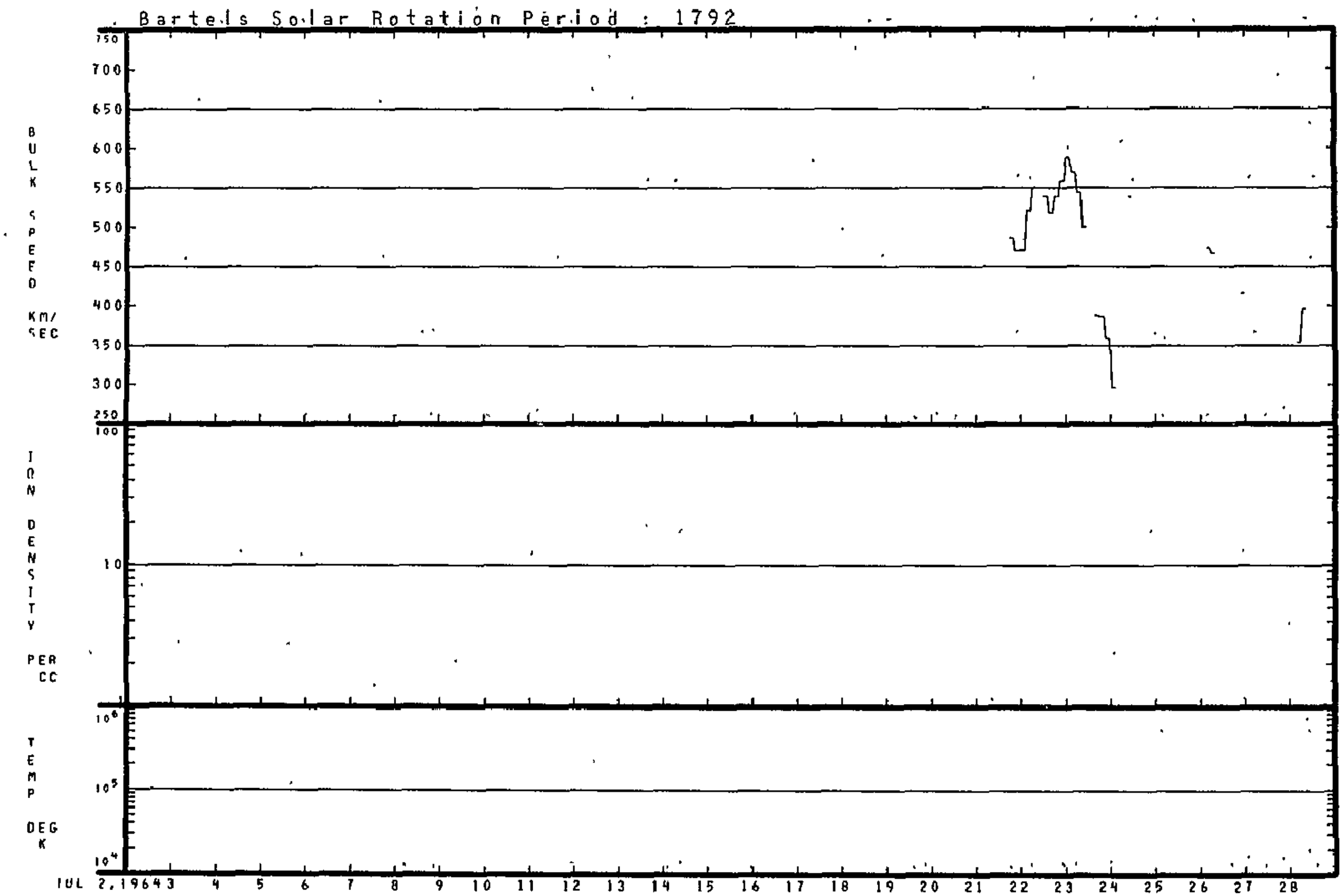




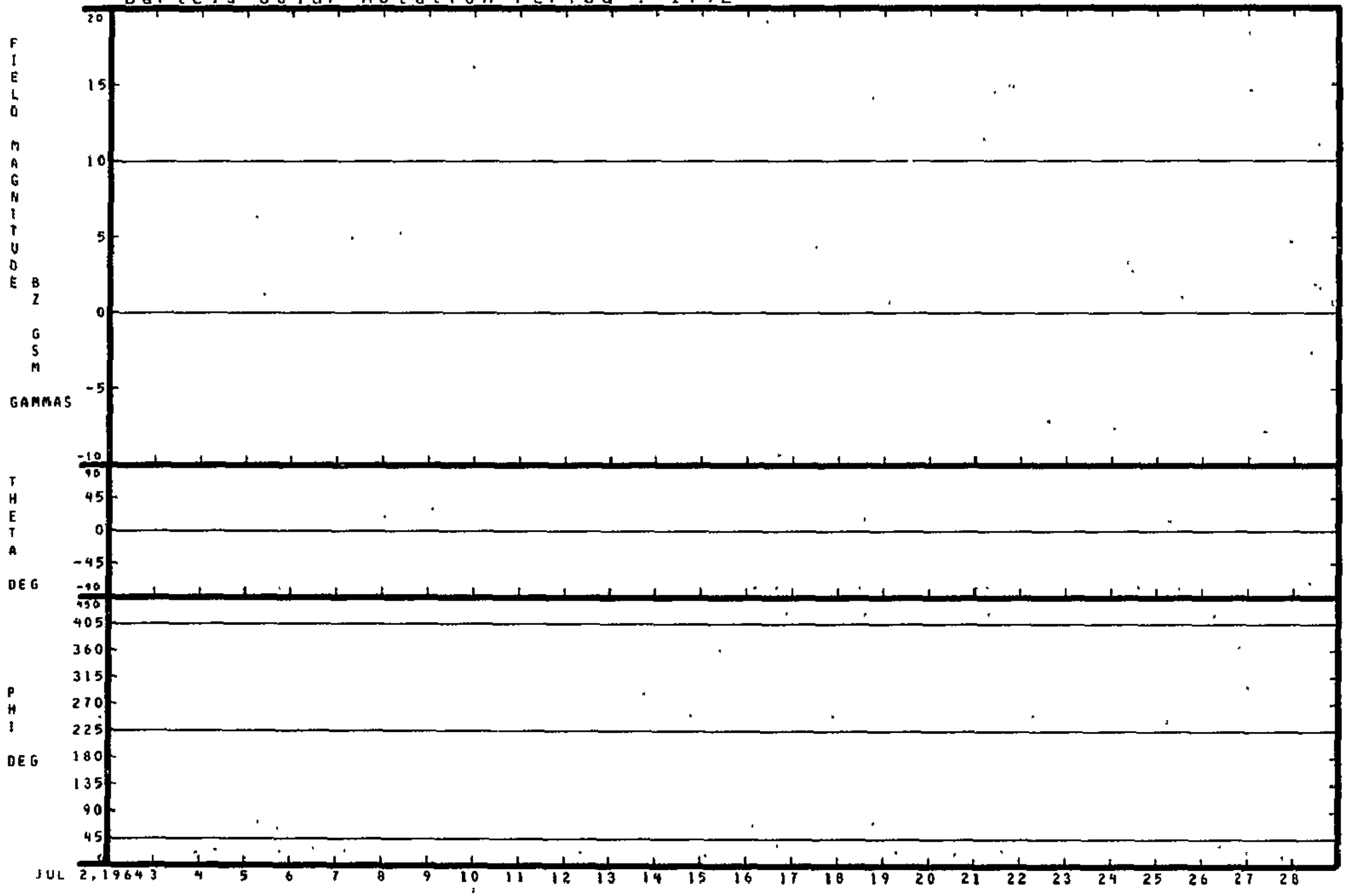
02/18/64 - 03/15/64

No interplanetary plasma or magnetic field data were available for solar rotations 1788 through 1791.

07/02/64 - 07/28/64



Bartels Solar Rotation Period : 1792



07/02/64 - 07/28/64

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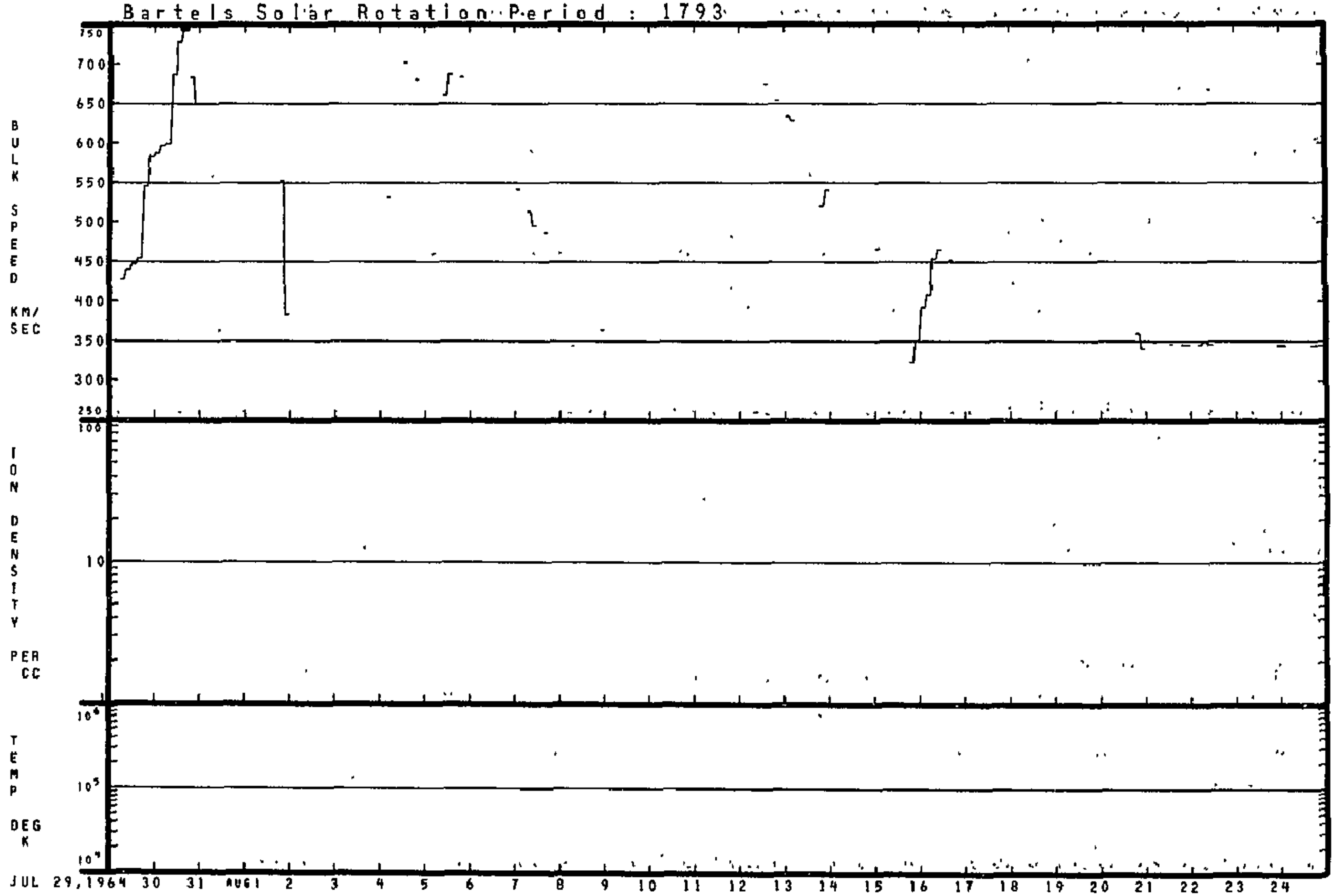
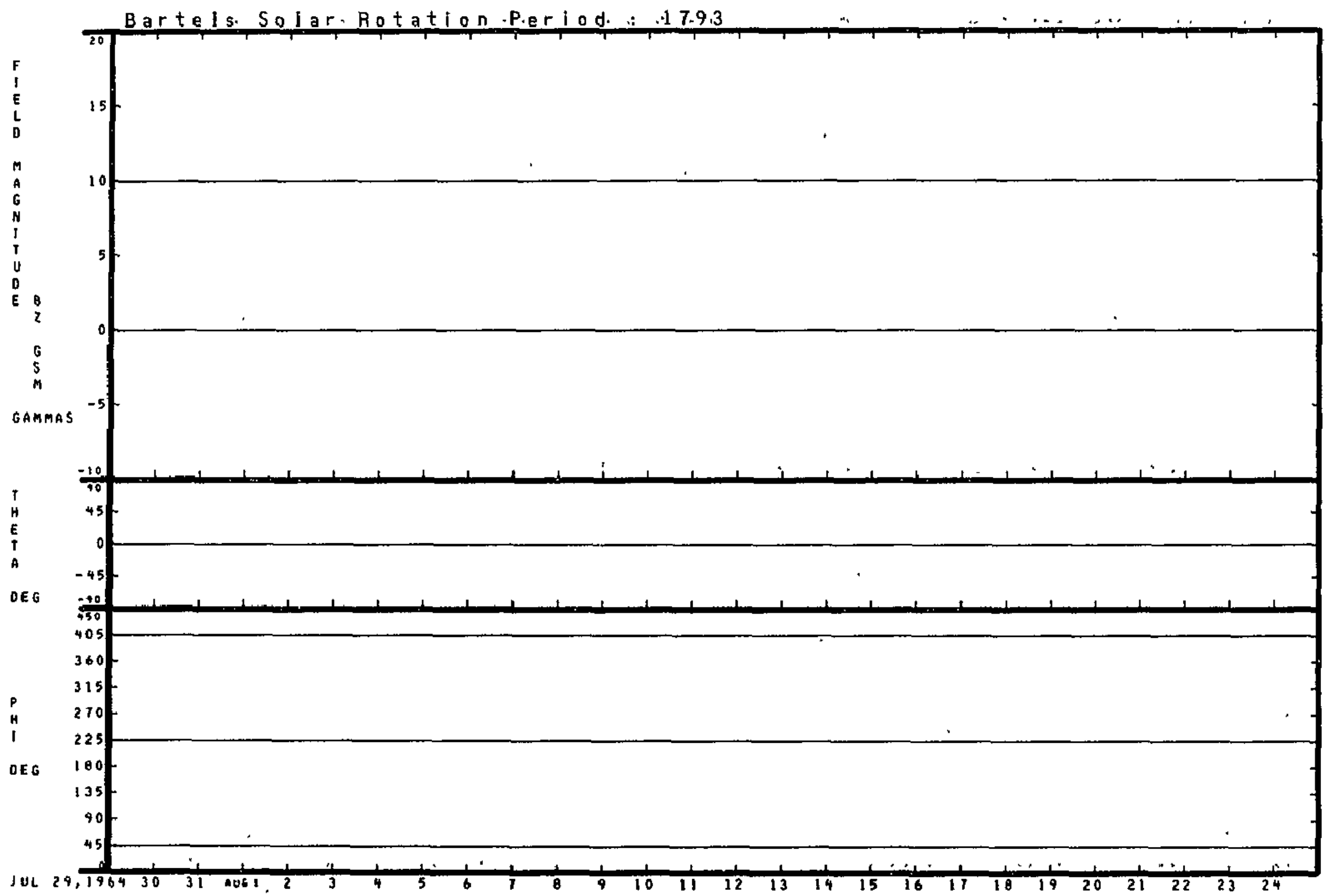


07/29/64 - 08/24/64



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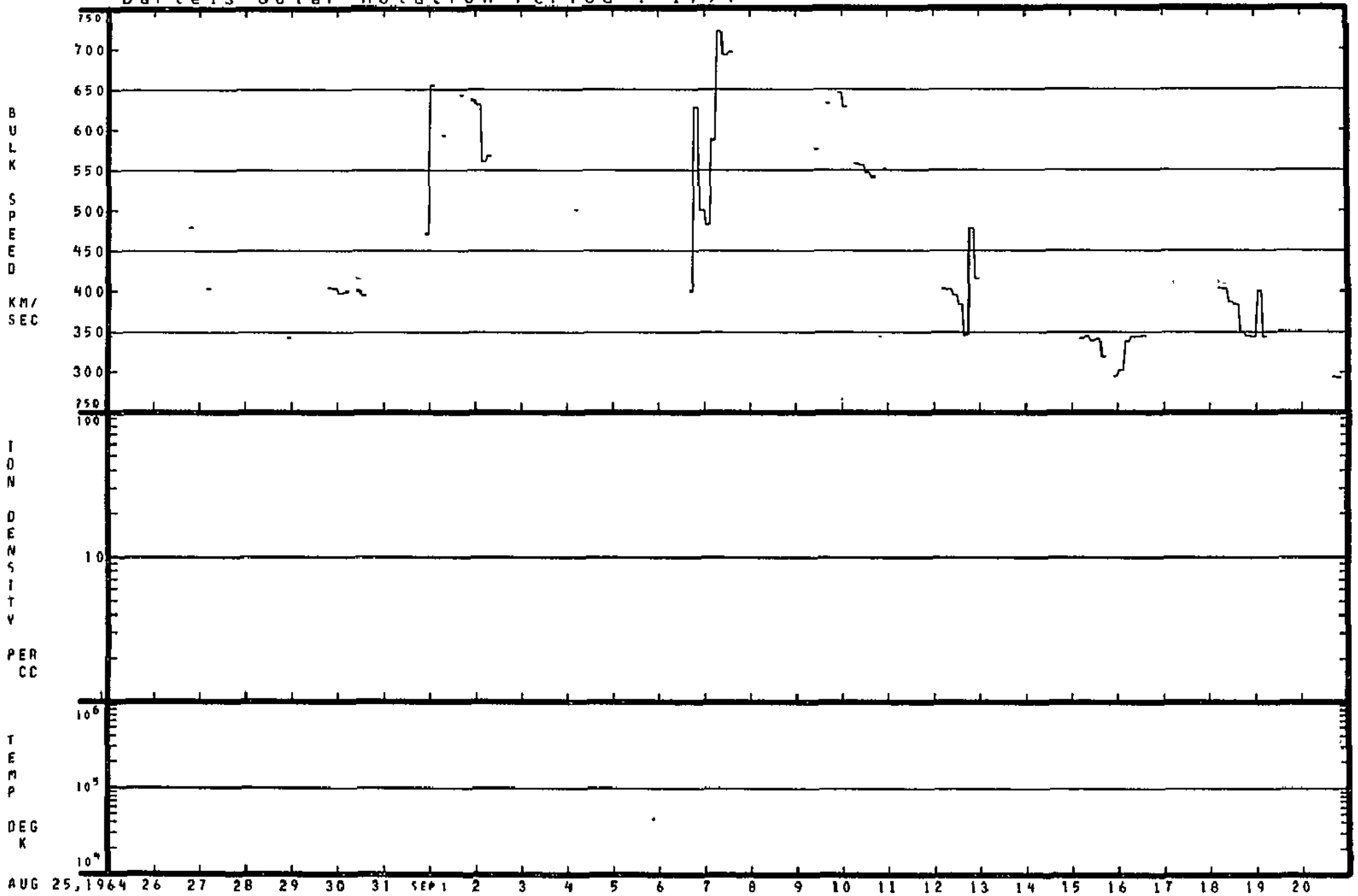
08/25/64 - 09/20/64

Bartels Solar Rotation Period : 1794



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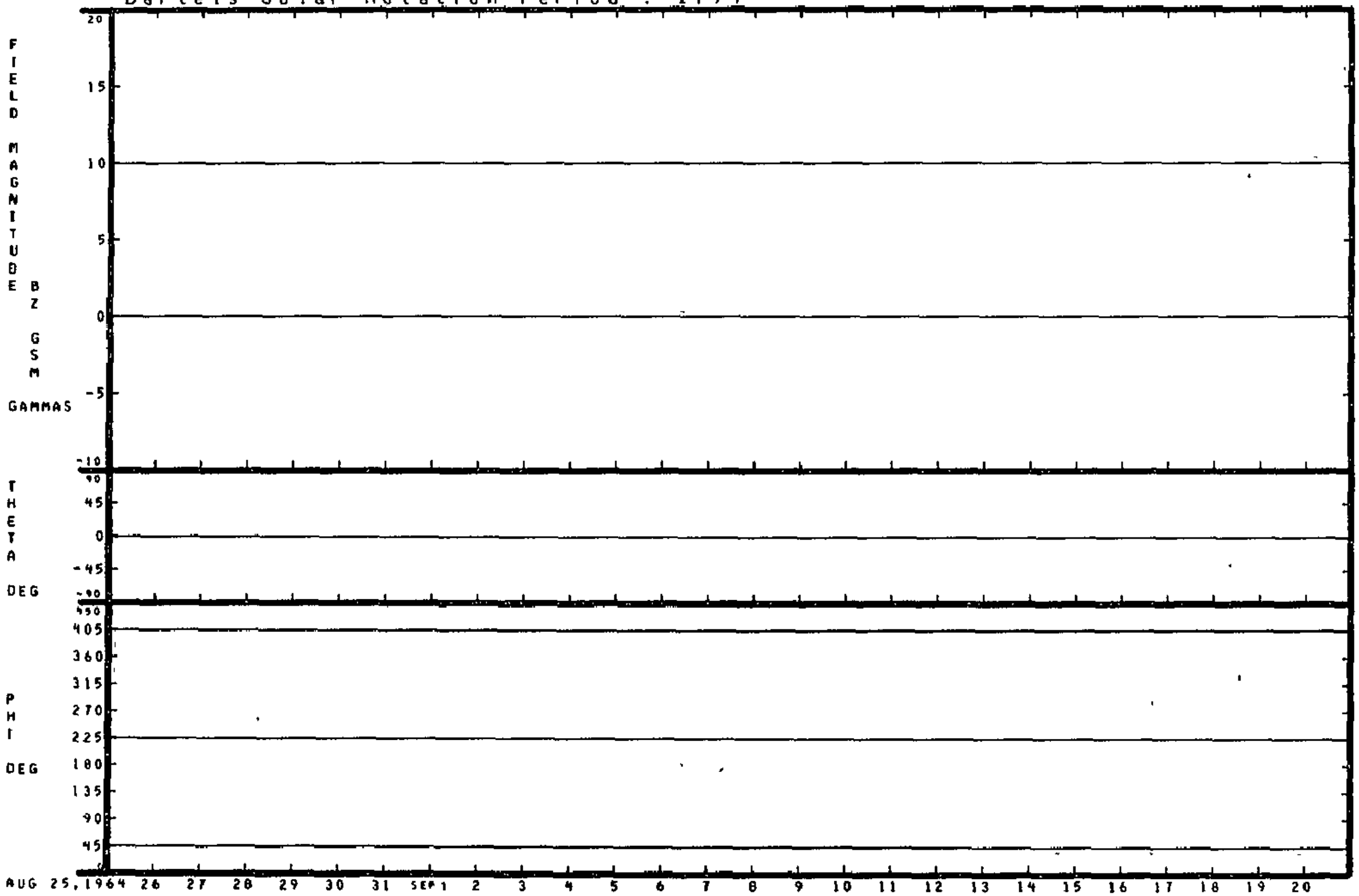

08/25/64 - 09/20/64

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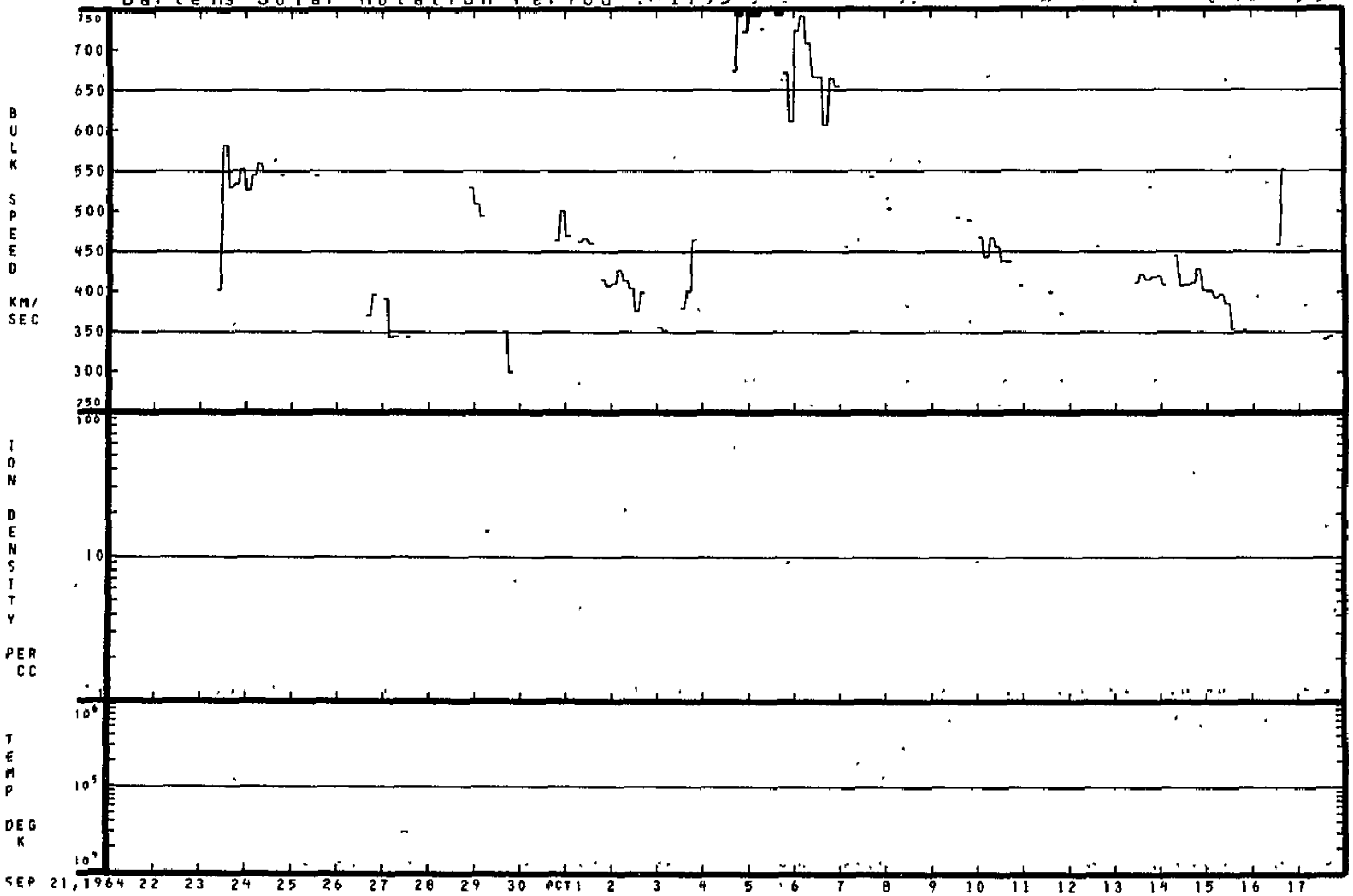
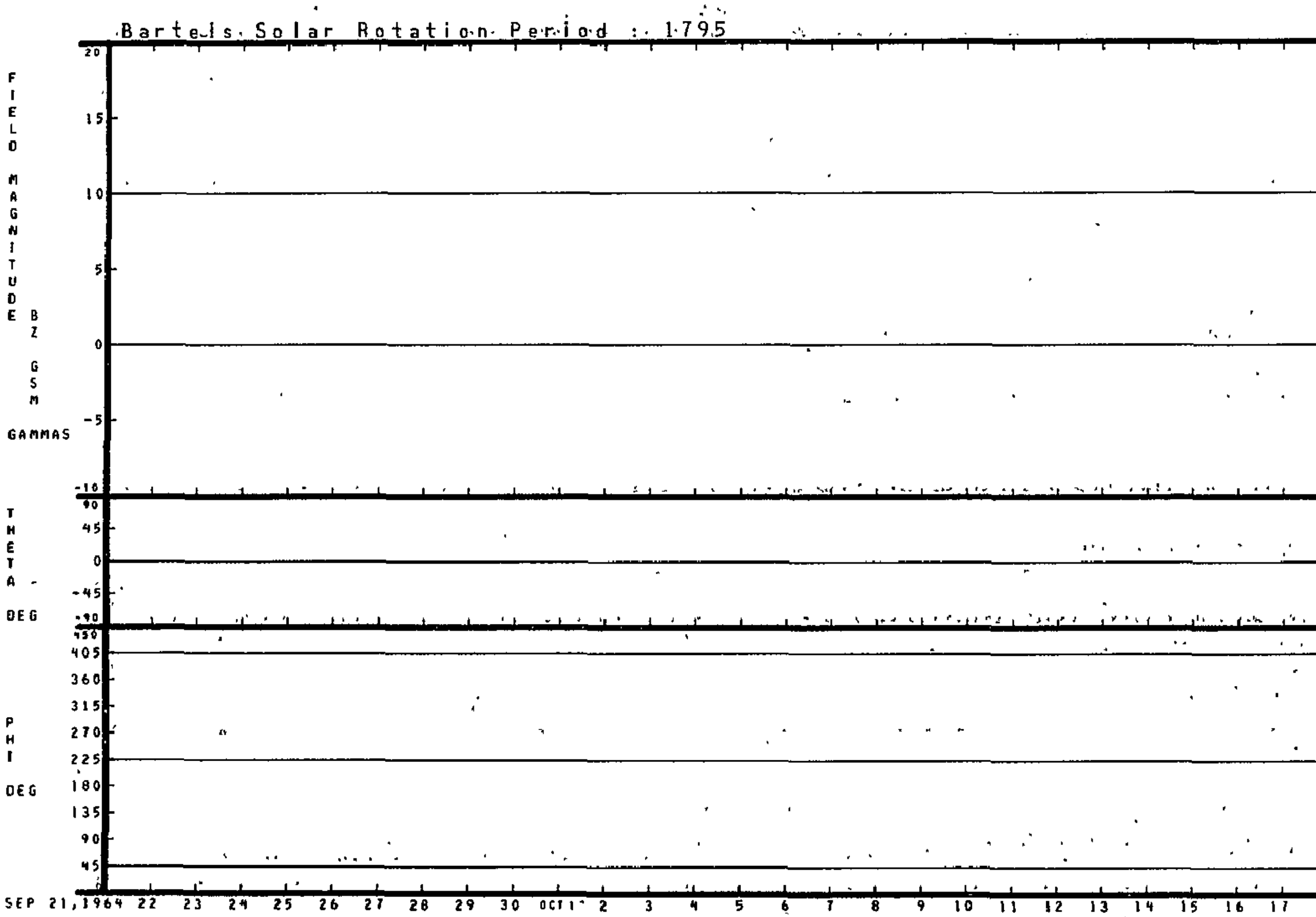
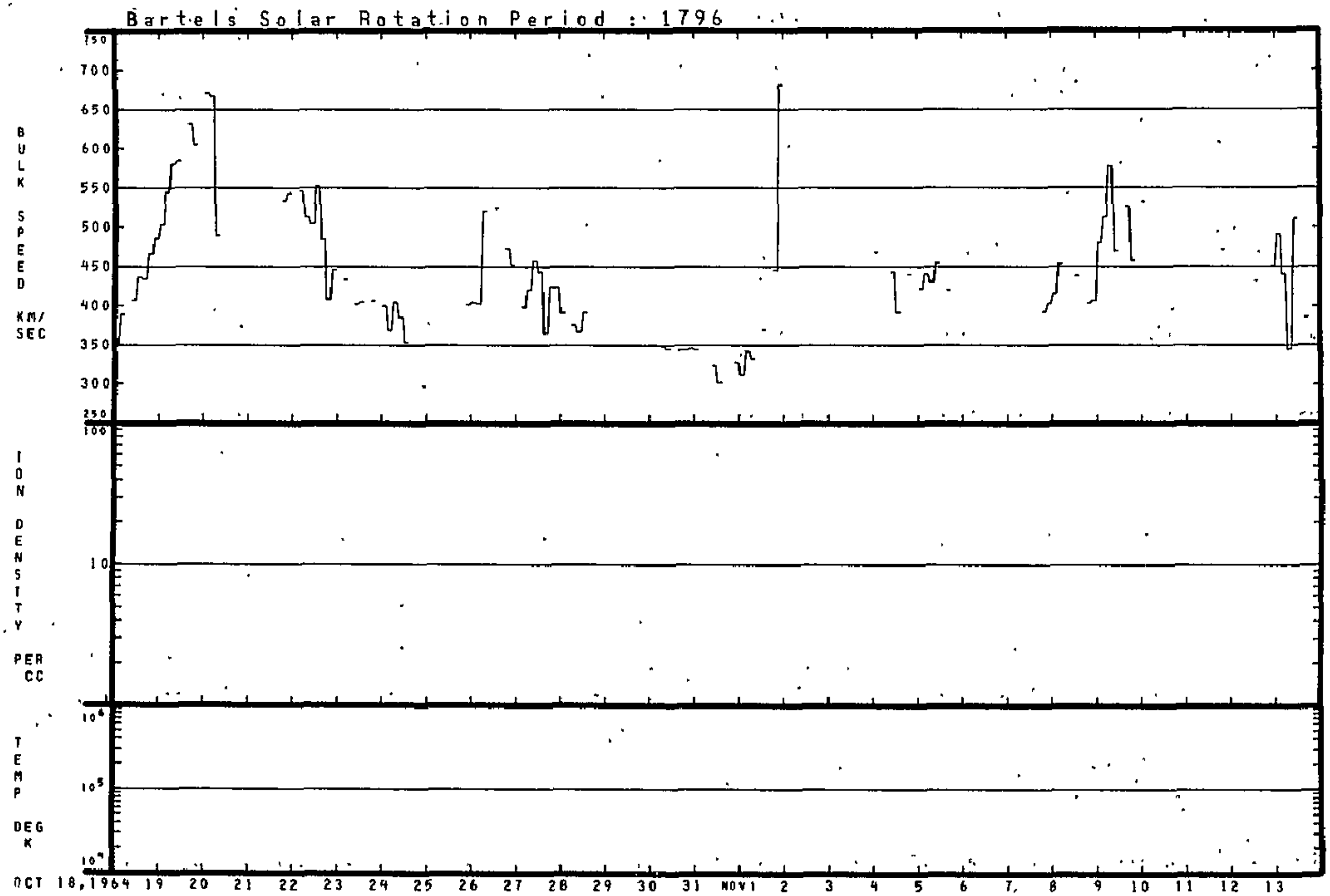


09/21/64 - 10/17/64



09/21/64 - 10/17/64

10/18/64 - 11/13/64



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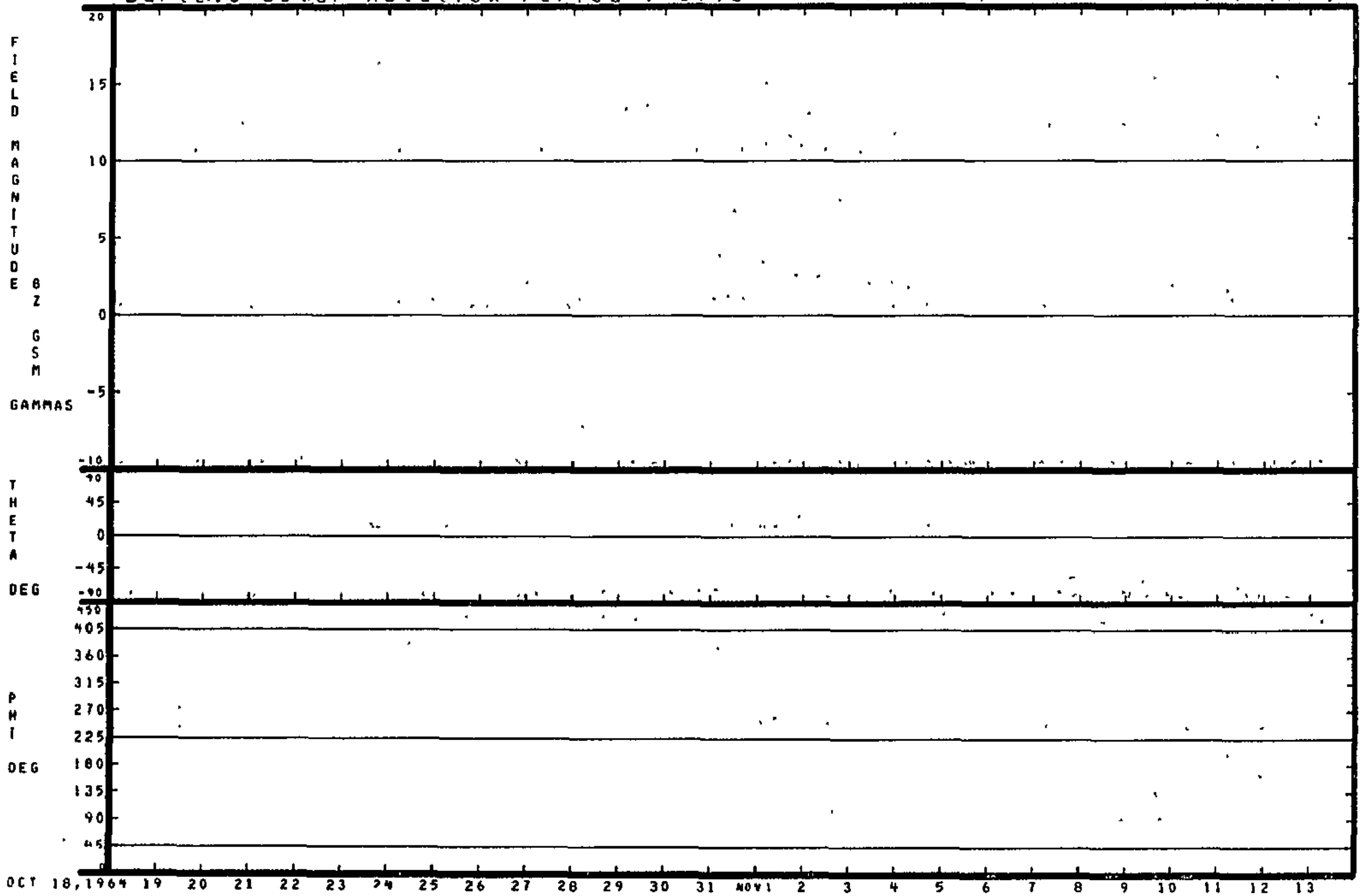

10/18/64 - 11/13/64

11/14/64 - 12/10/64

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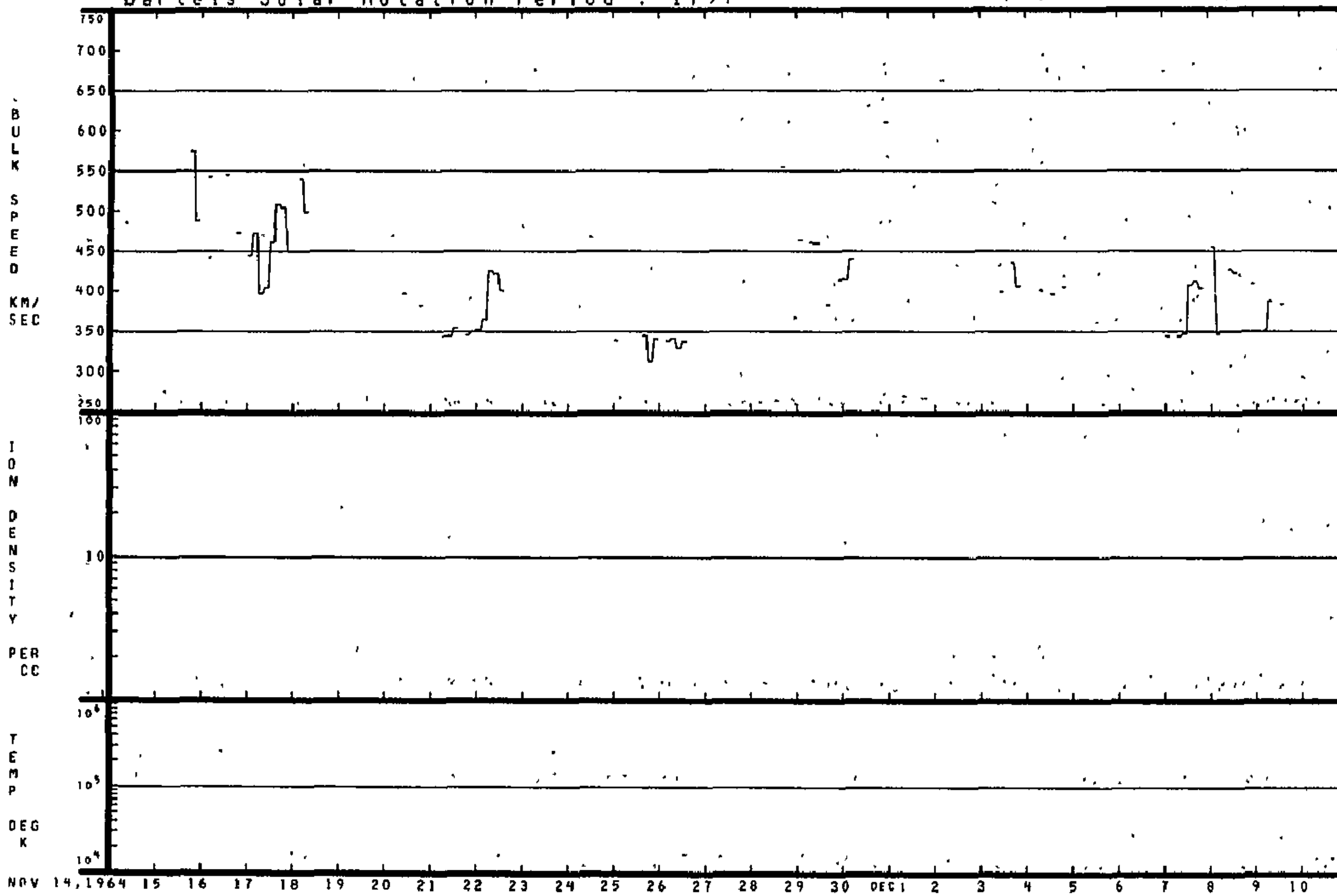

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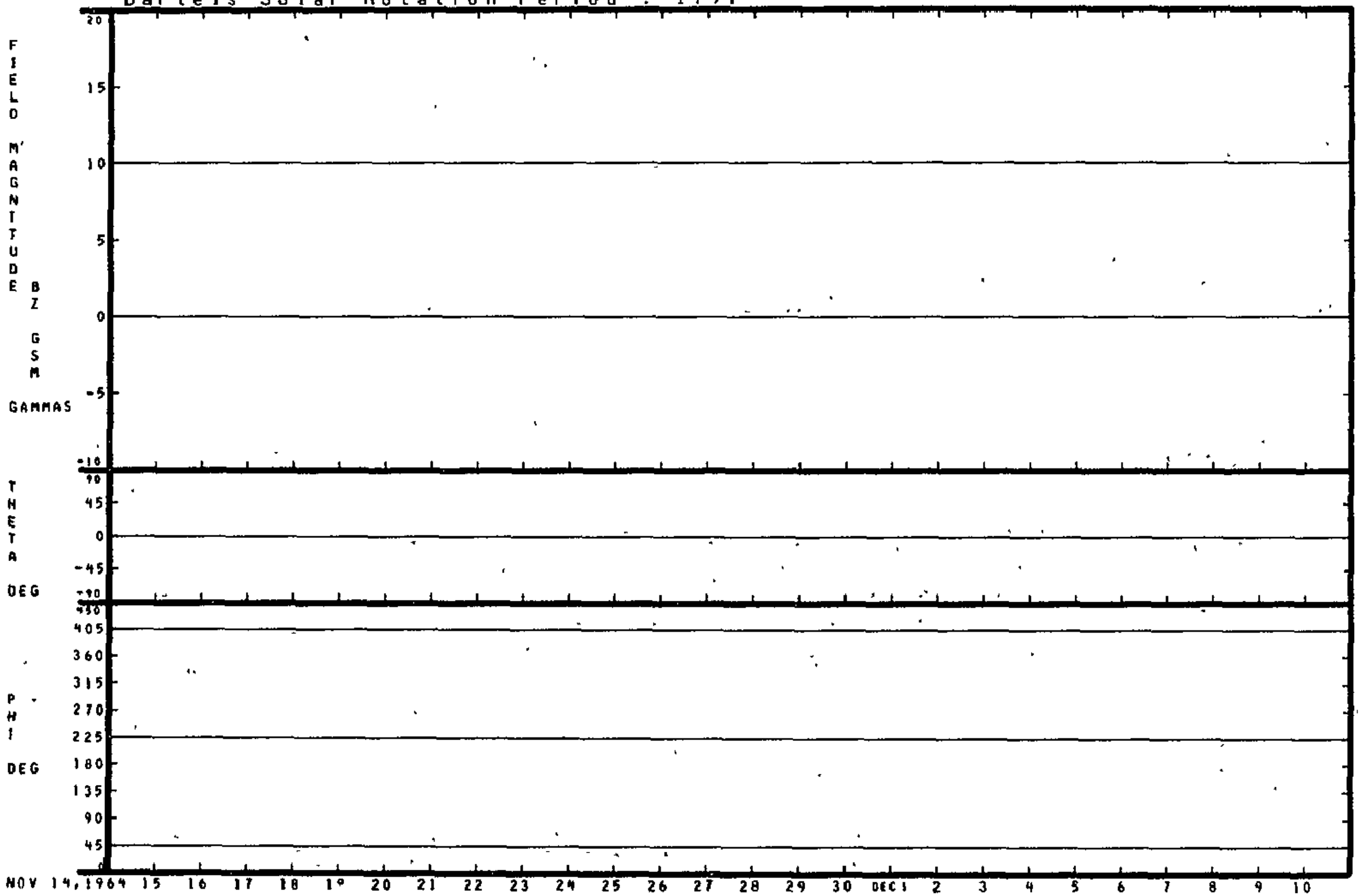

11/14/64 - 12/10/64

12/11/64 - 01/06/65

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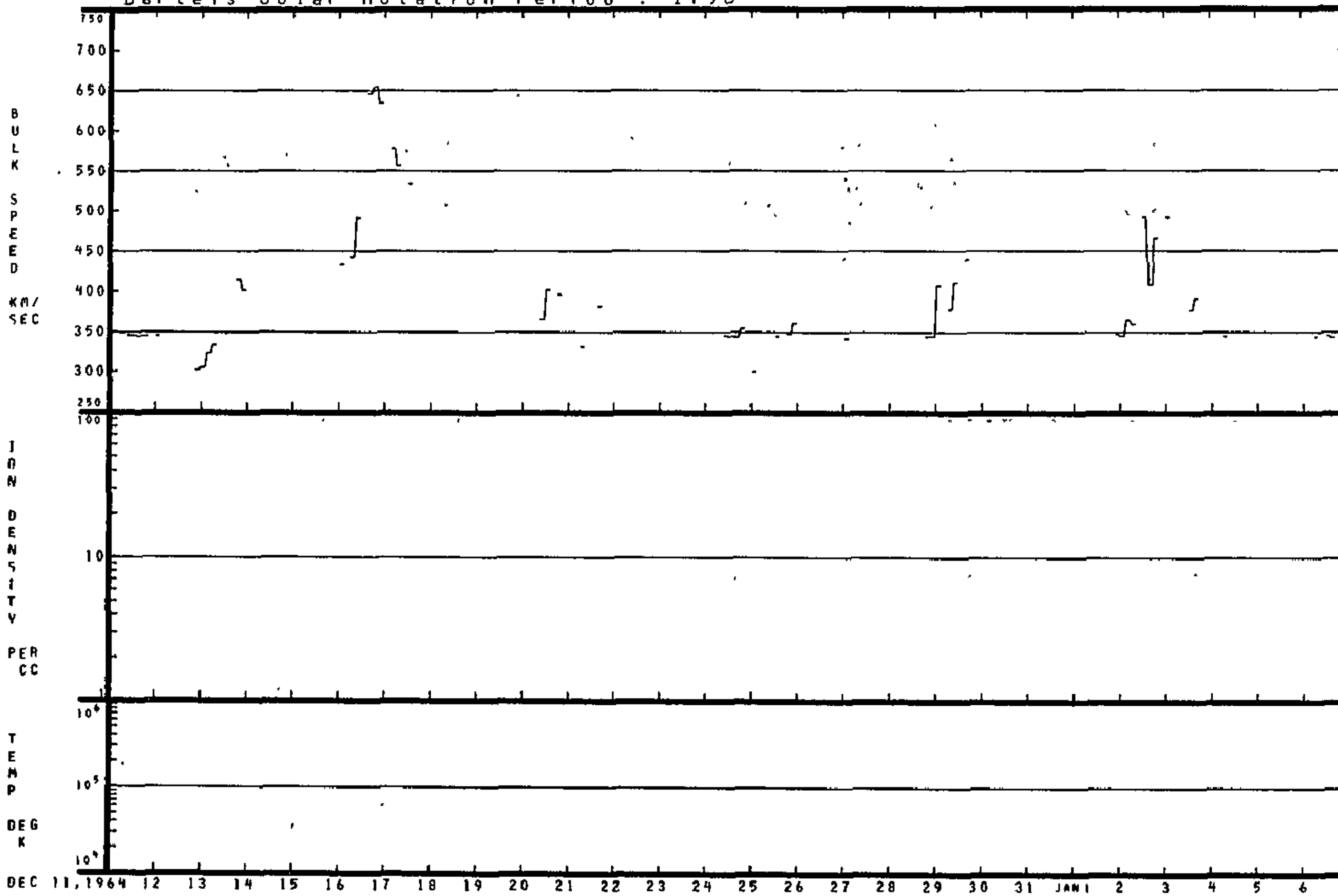

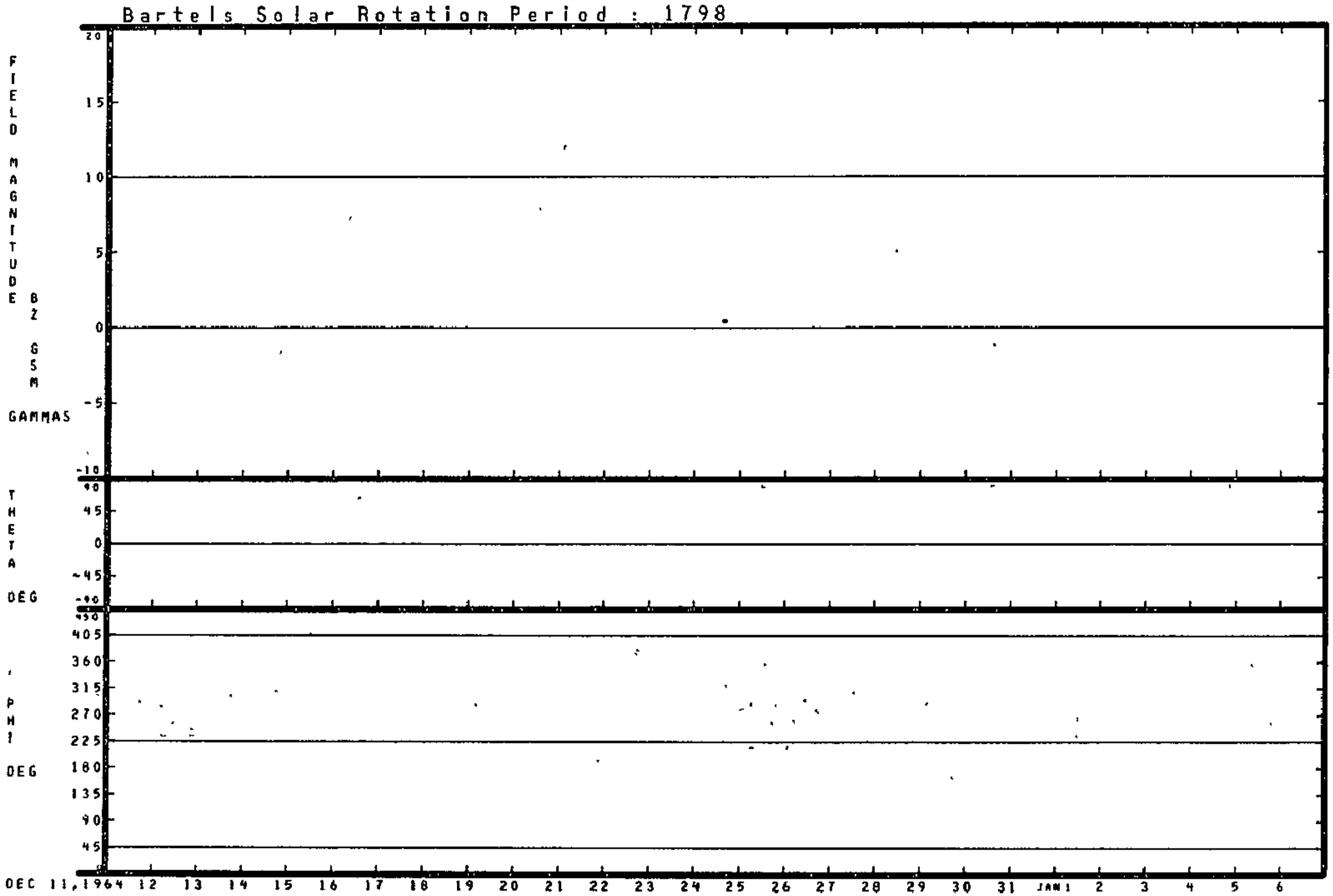

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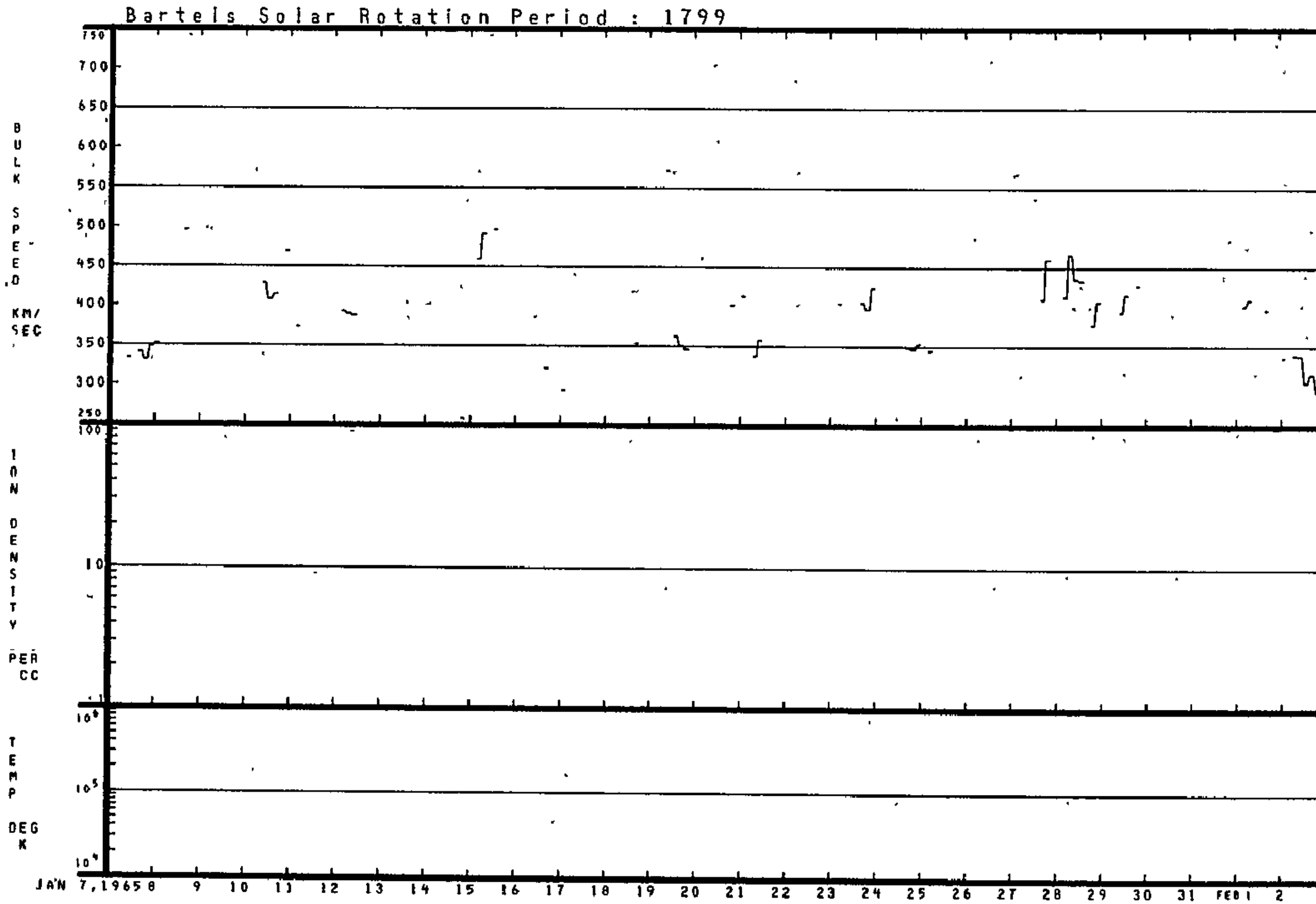

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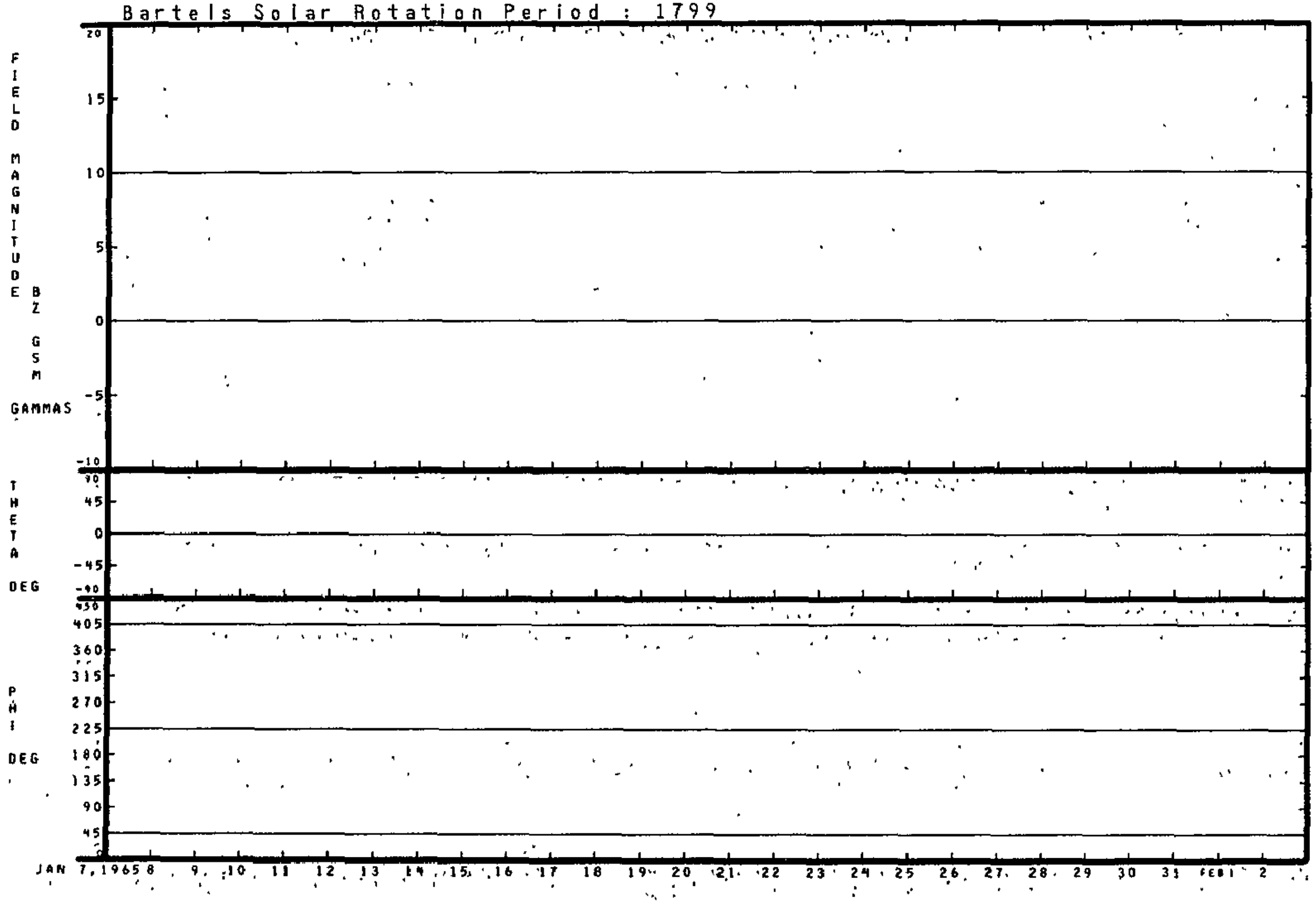

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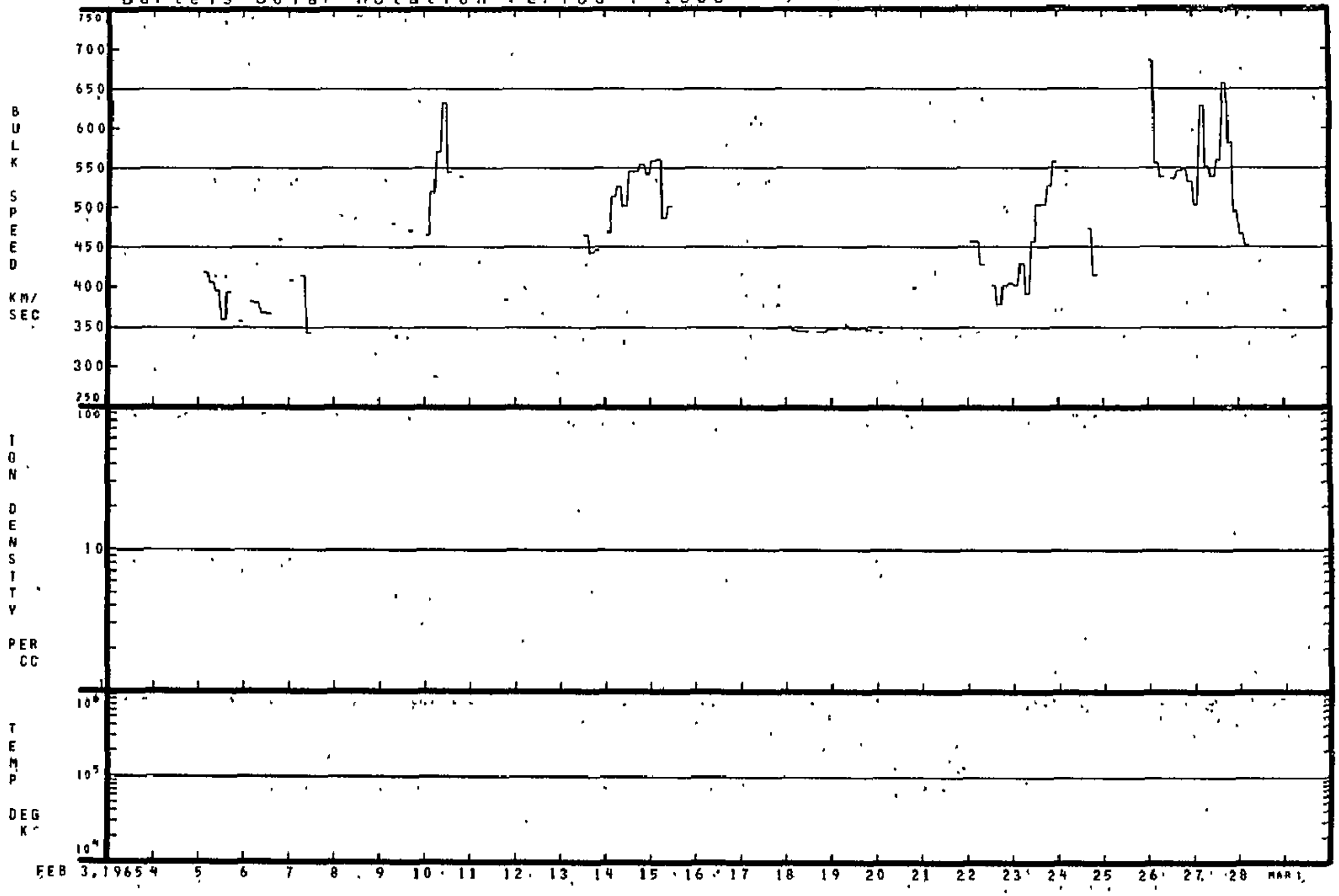

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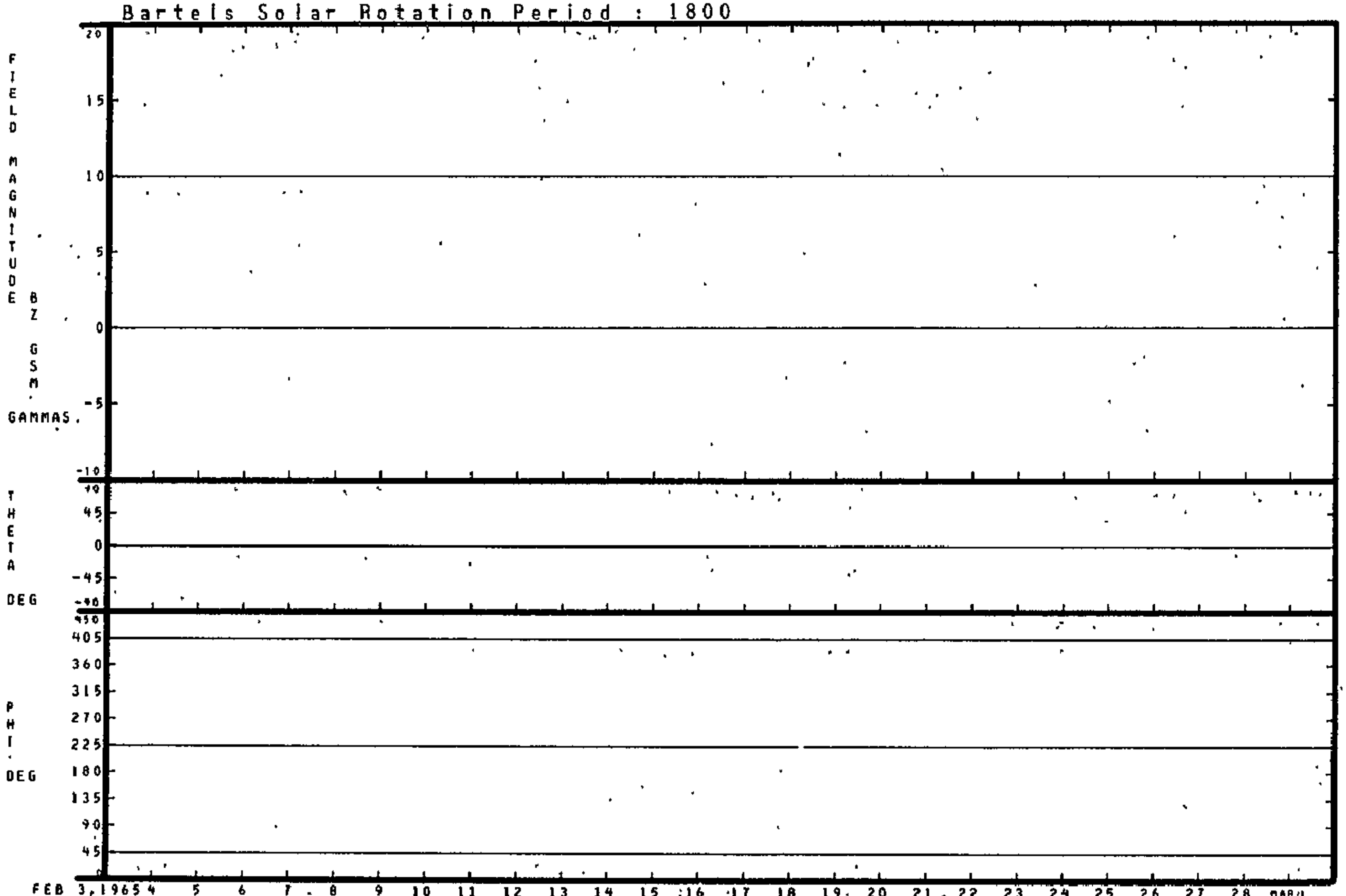

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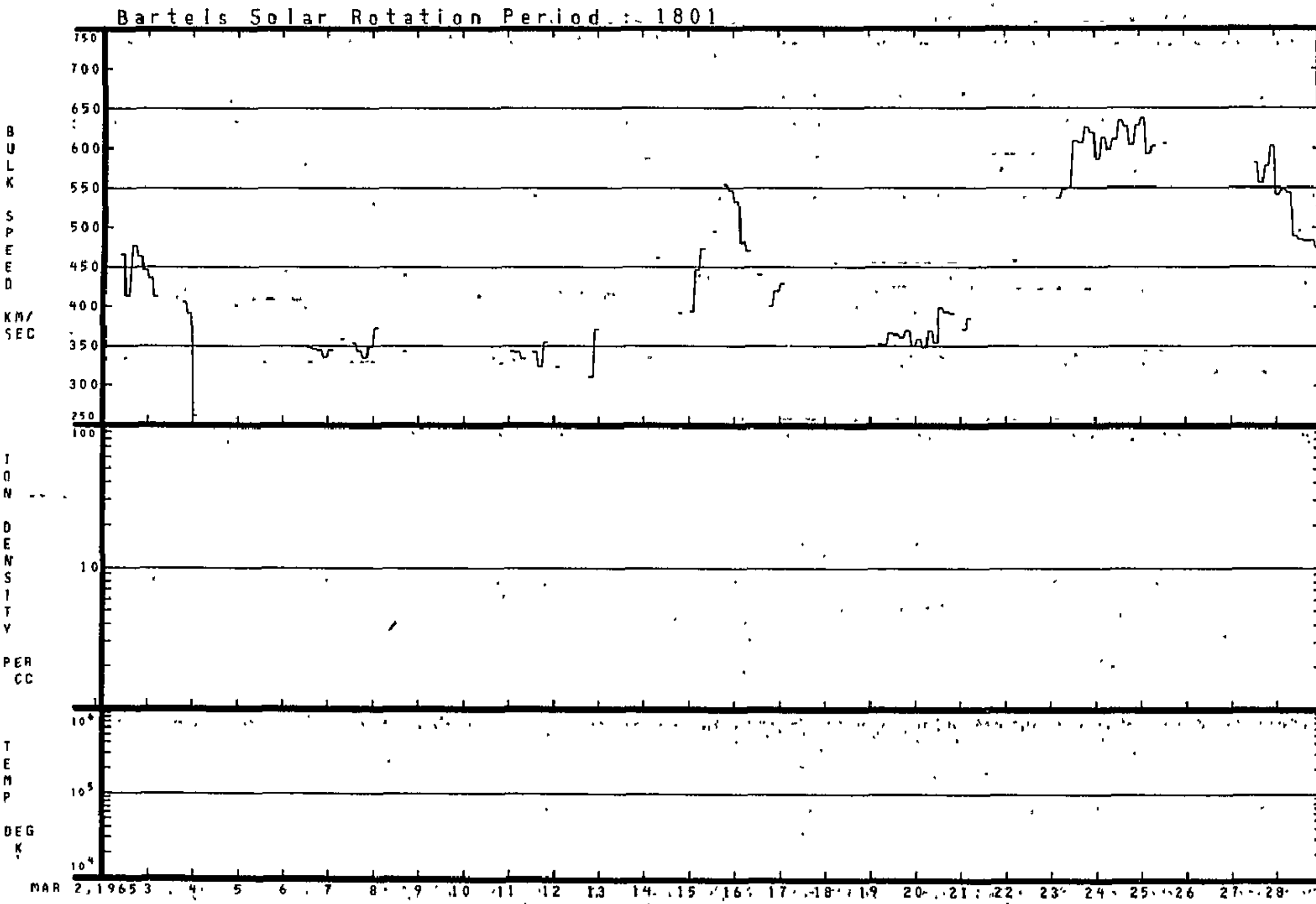

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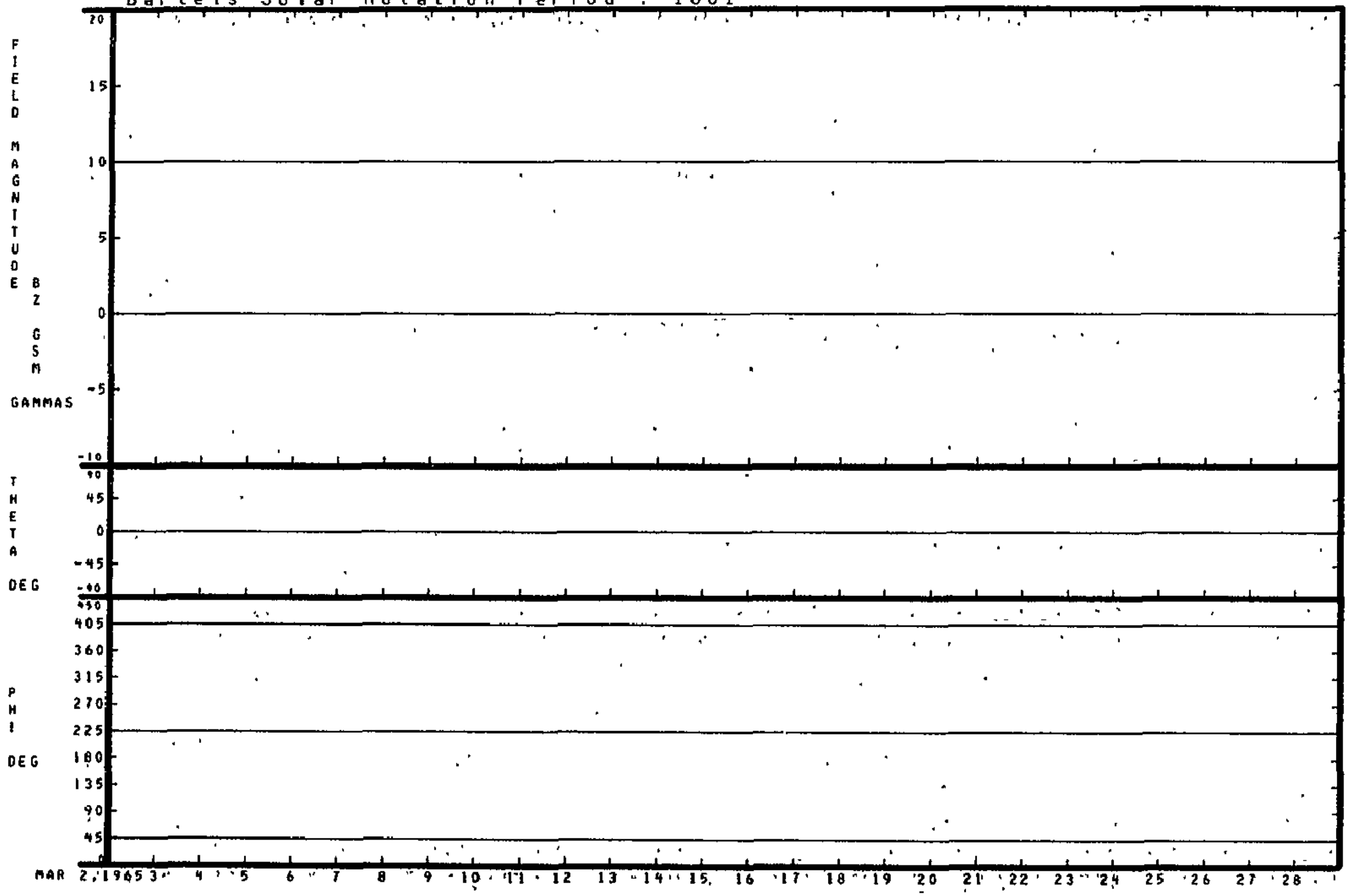

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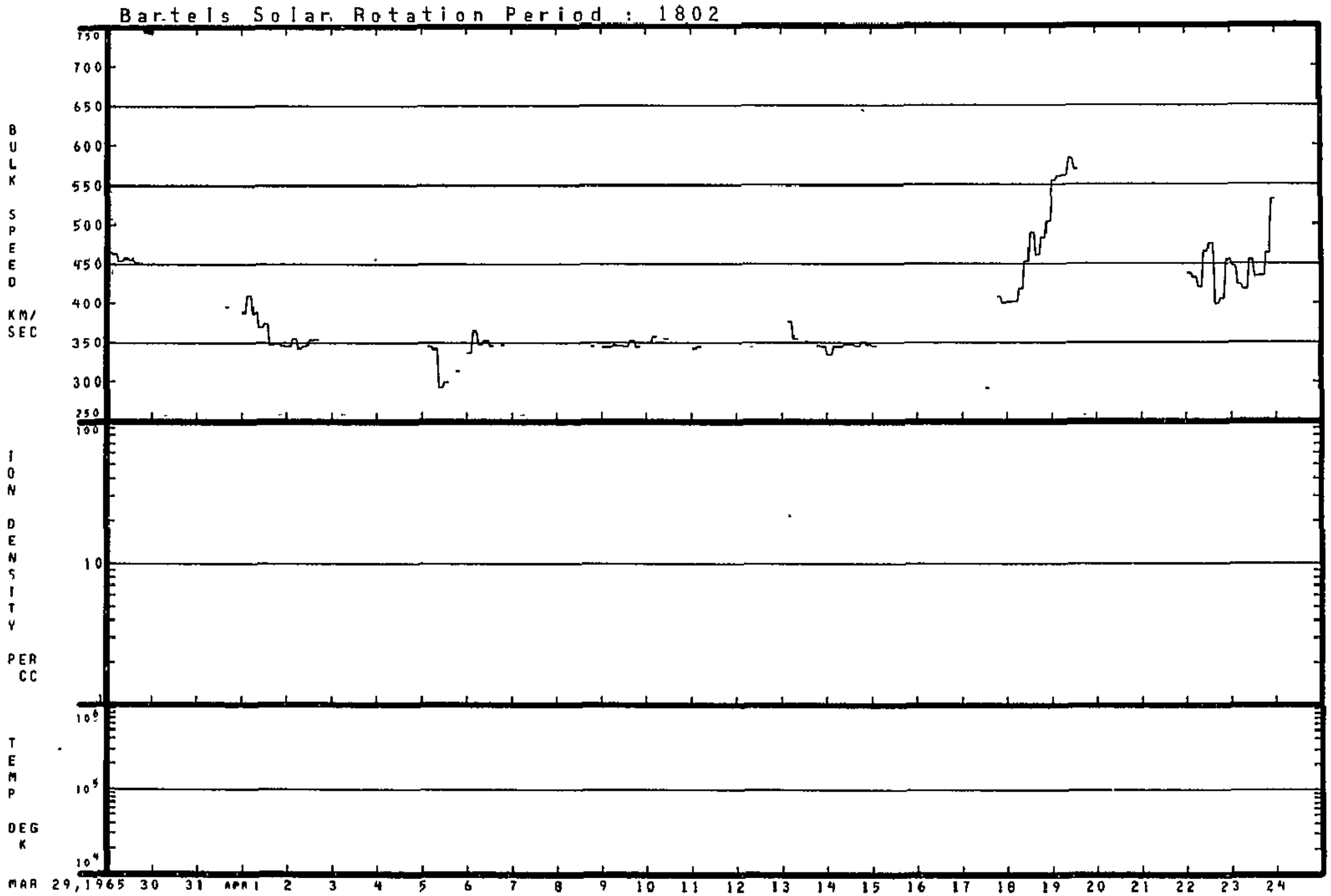

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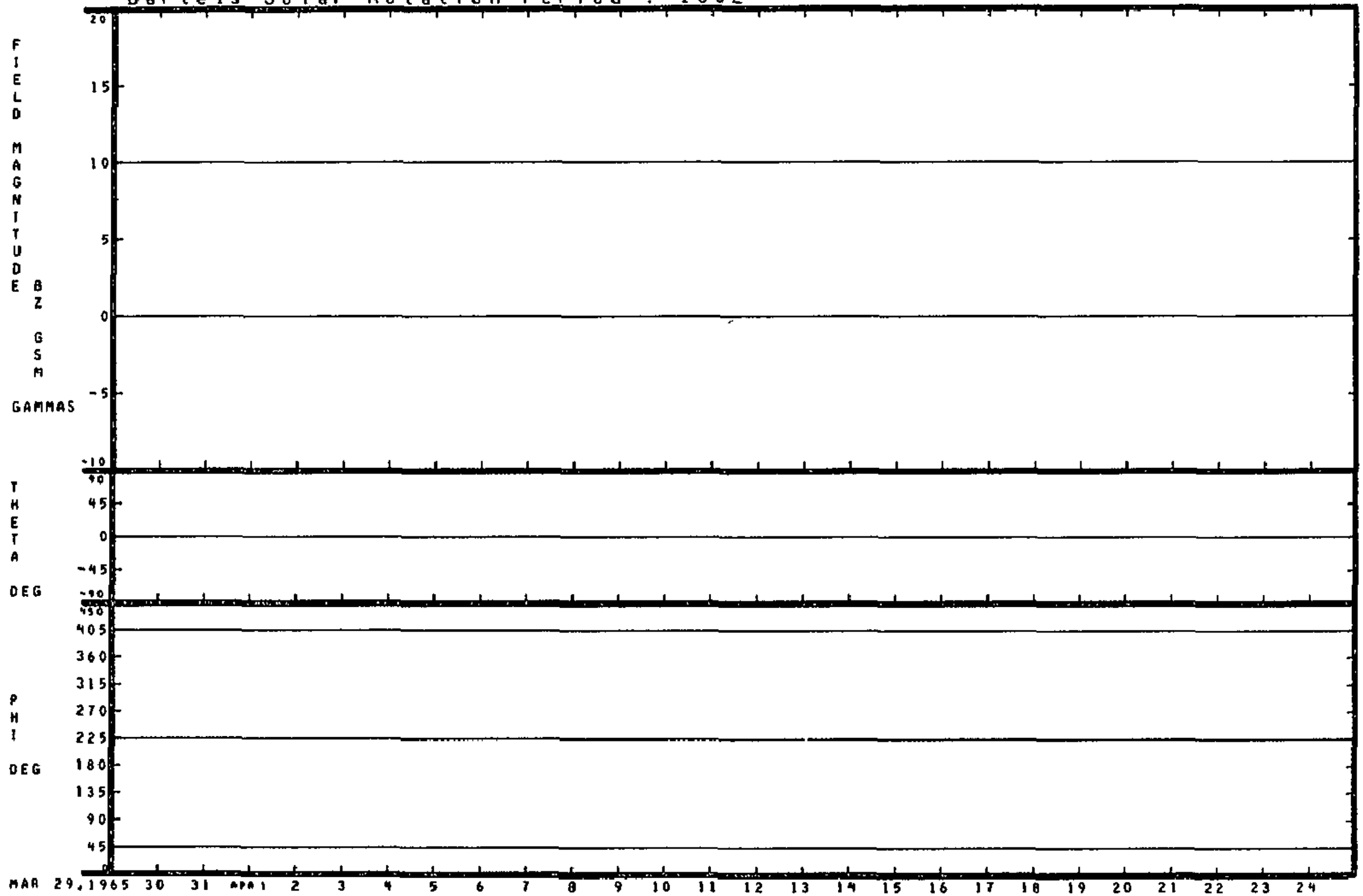
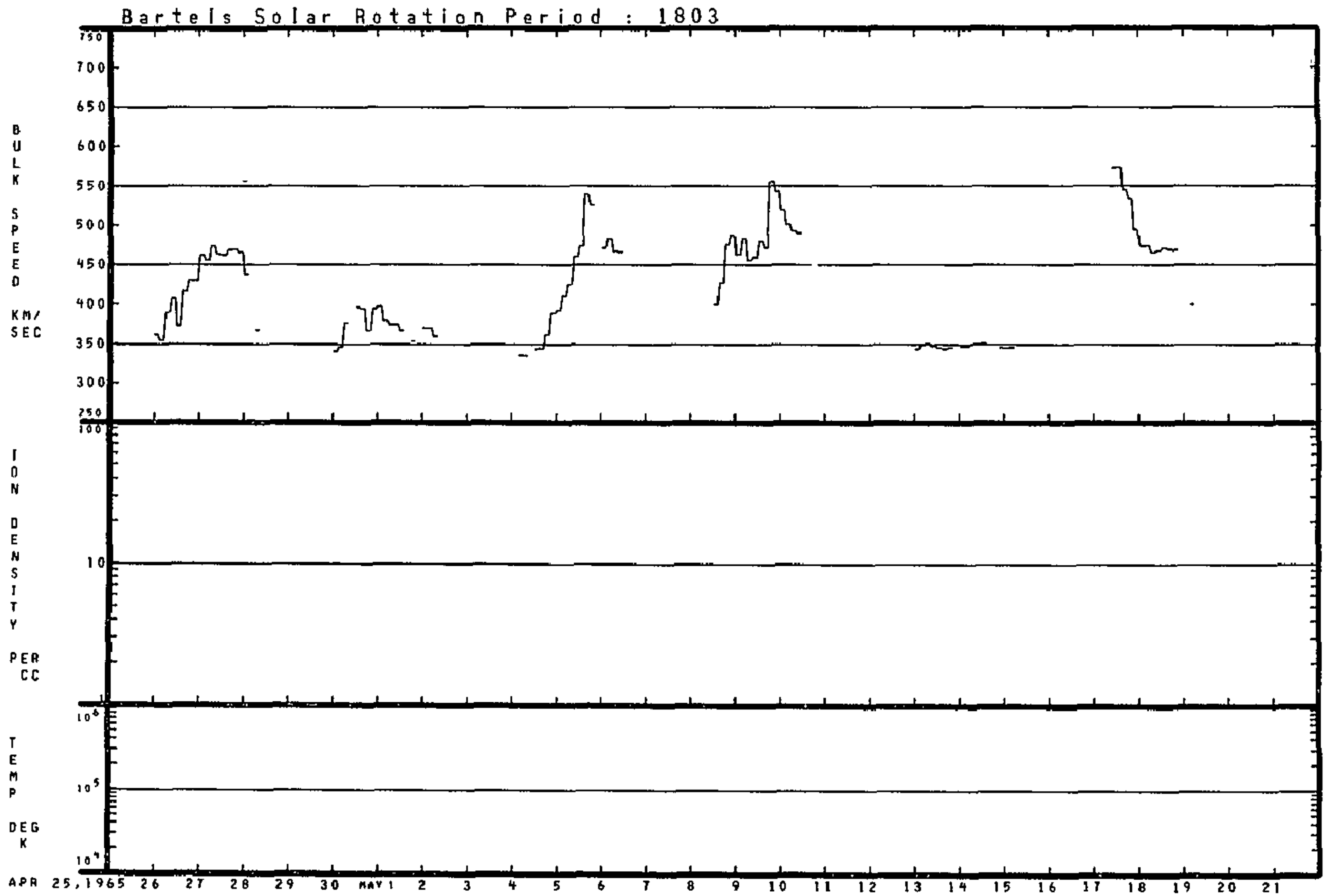
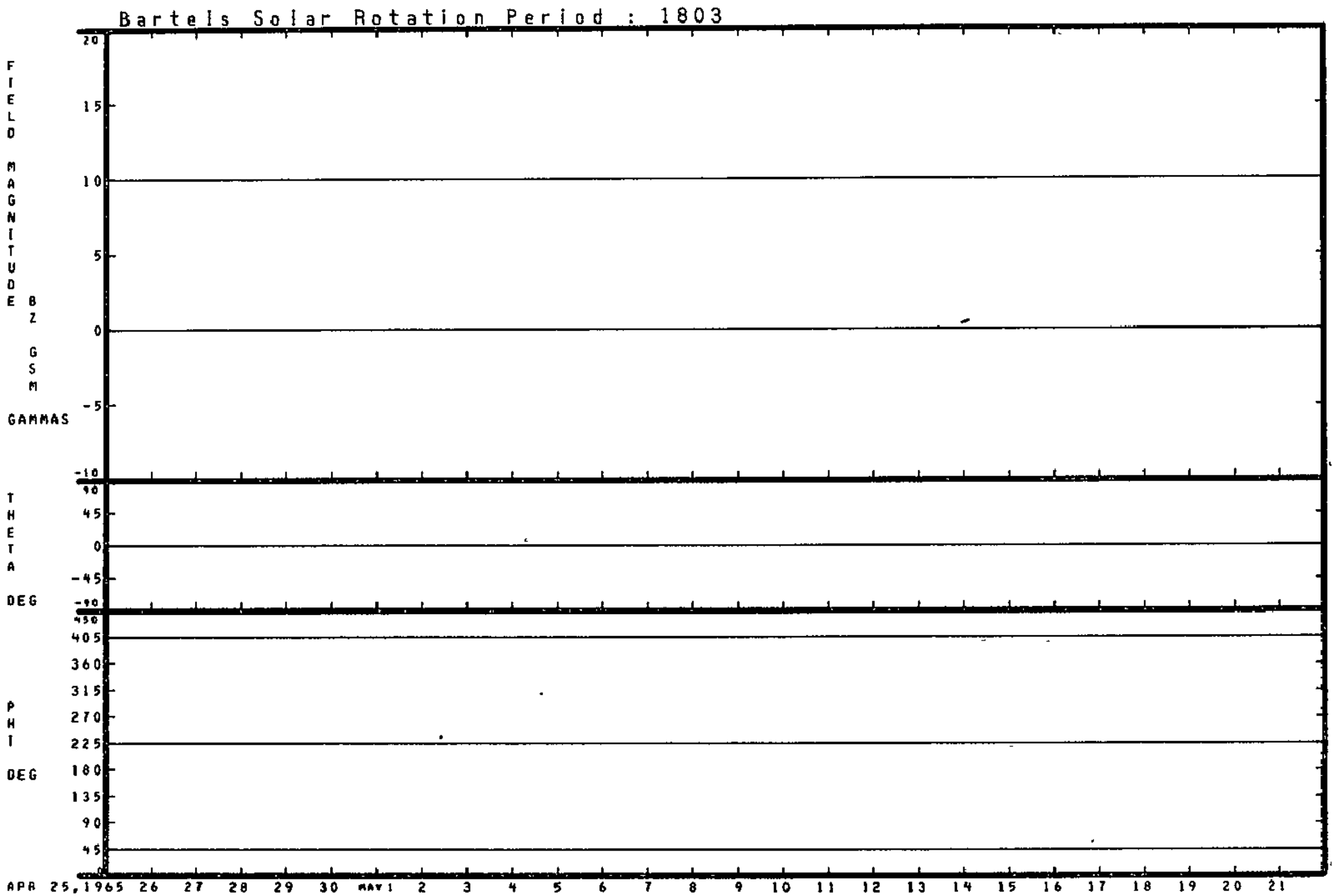


03/29/65 - 04/24/65

a-2

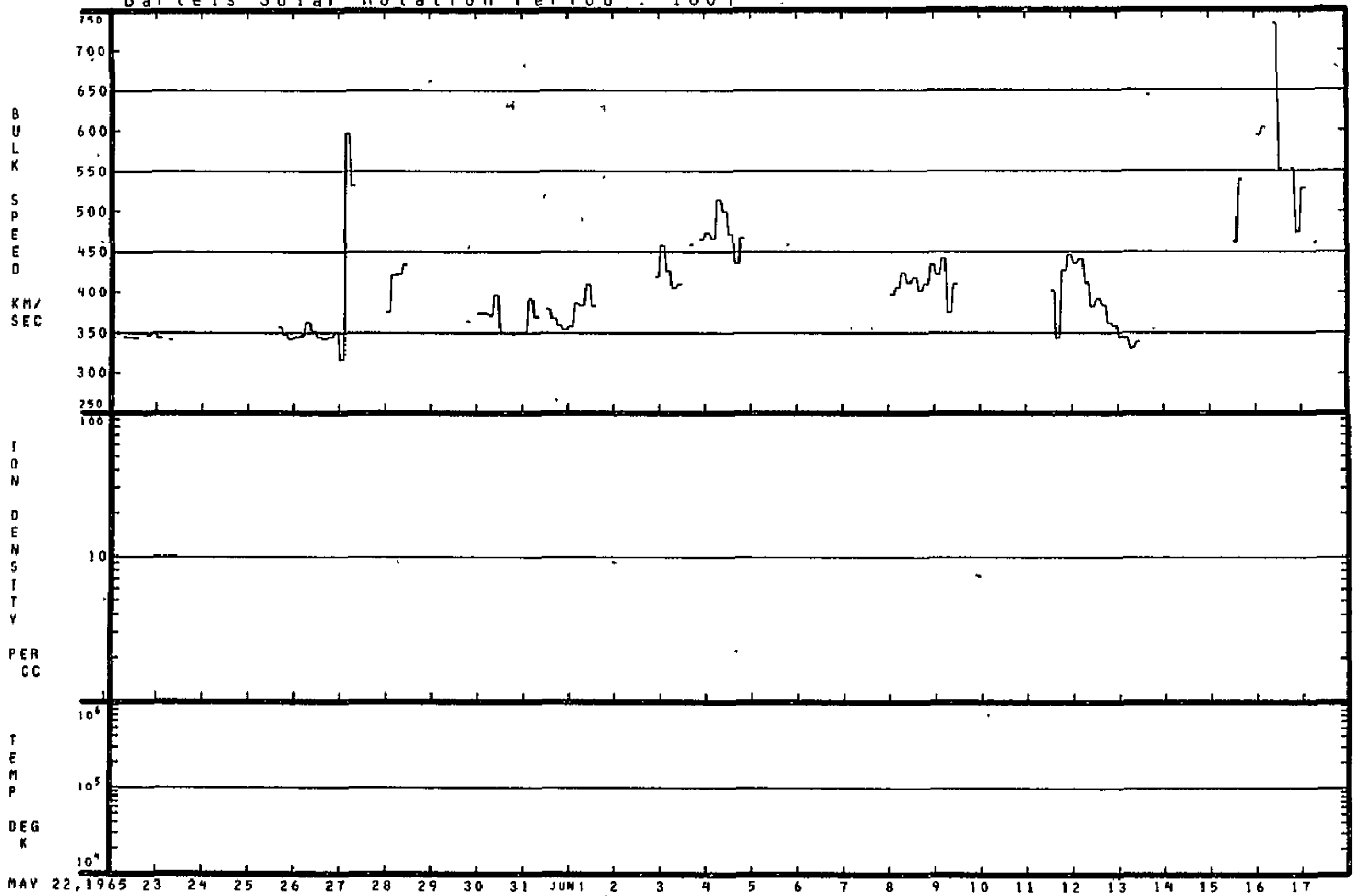
04/25/65 - 05/21/65





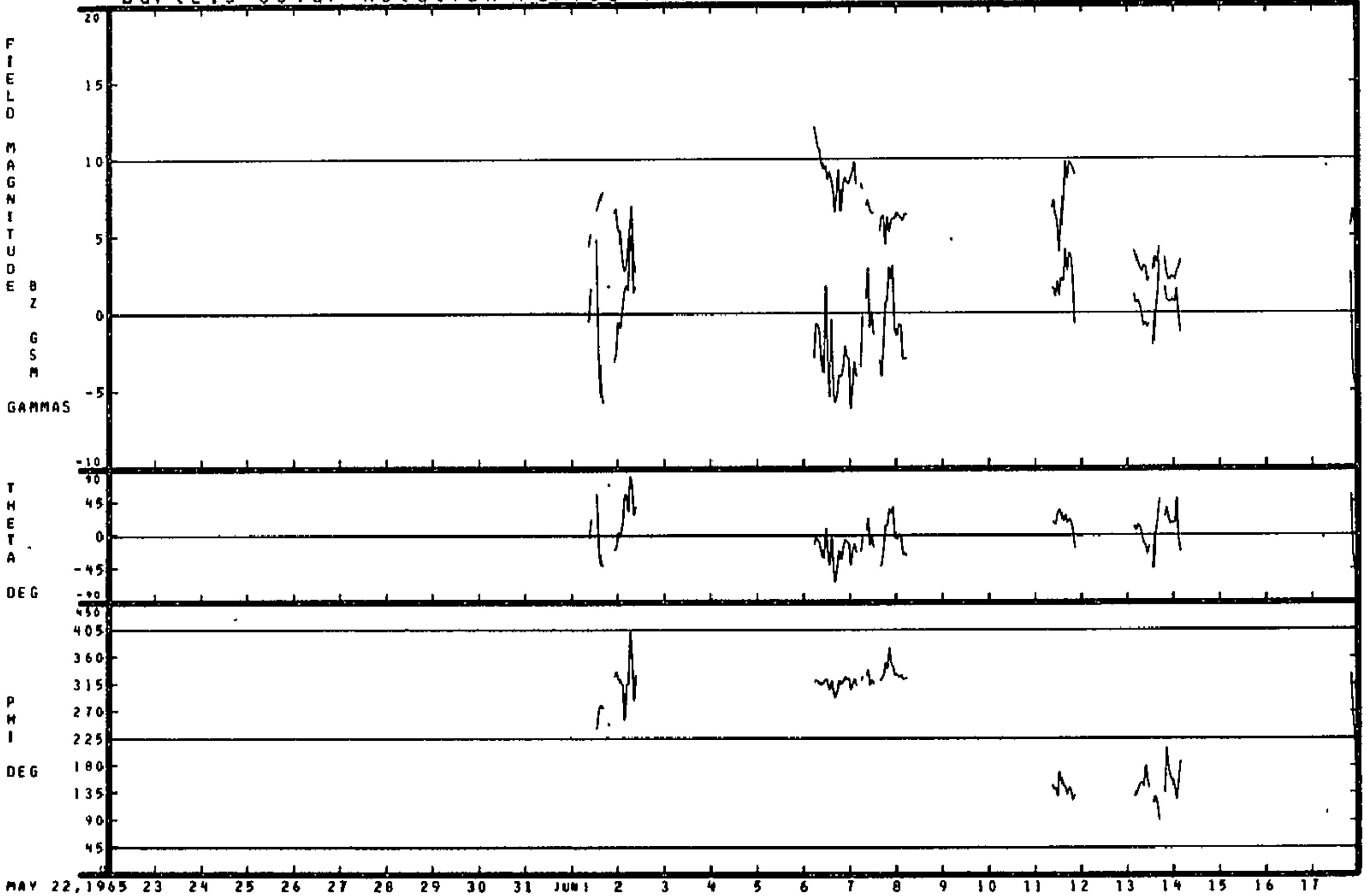
04/25/65 - 05/21/65

Bartels Solar Rotation Period : 1804



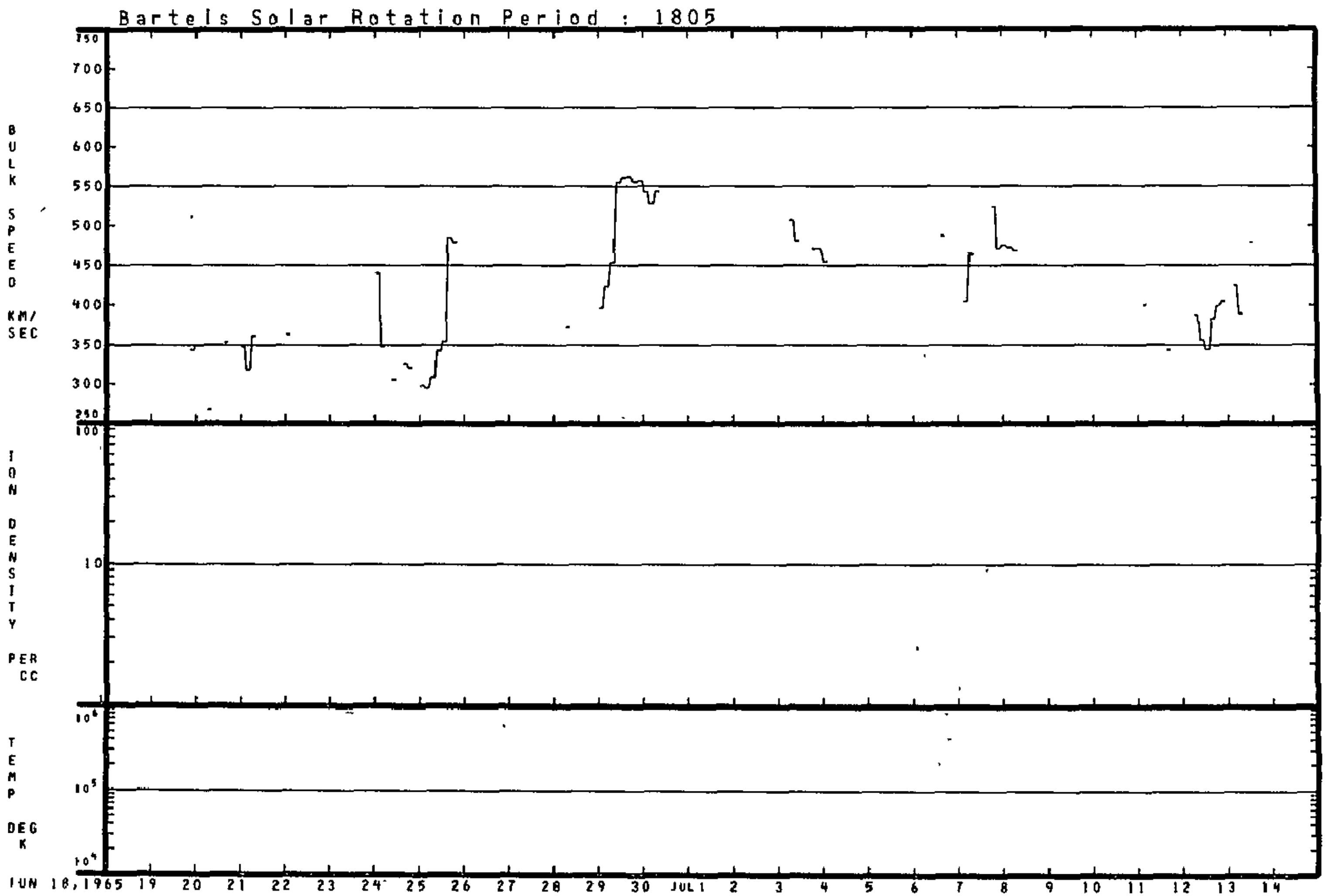
05/22/65 - 06/17/65

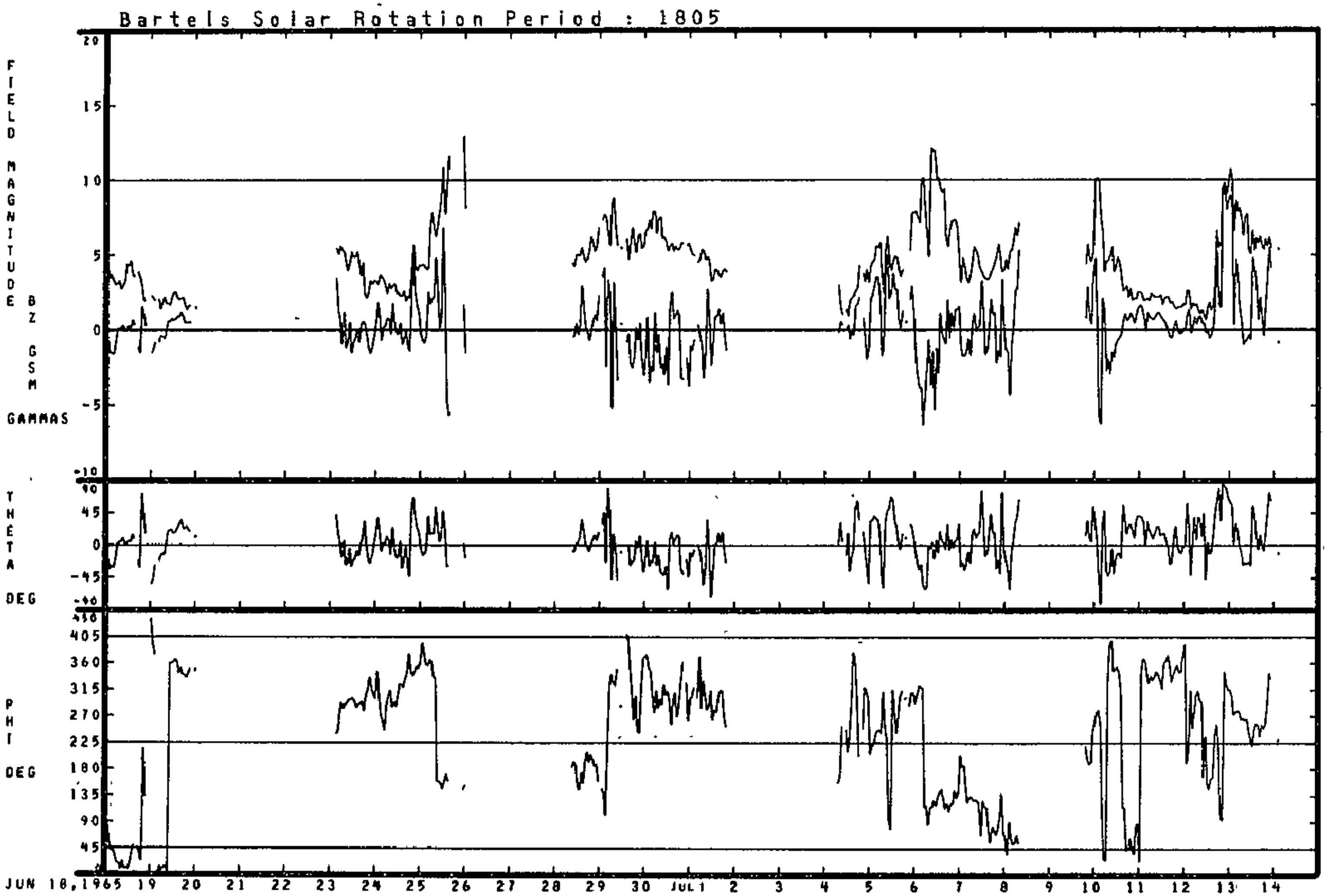
Bartels Solar Rotation Period : 1804



05/22/65 - 06/17/65

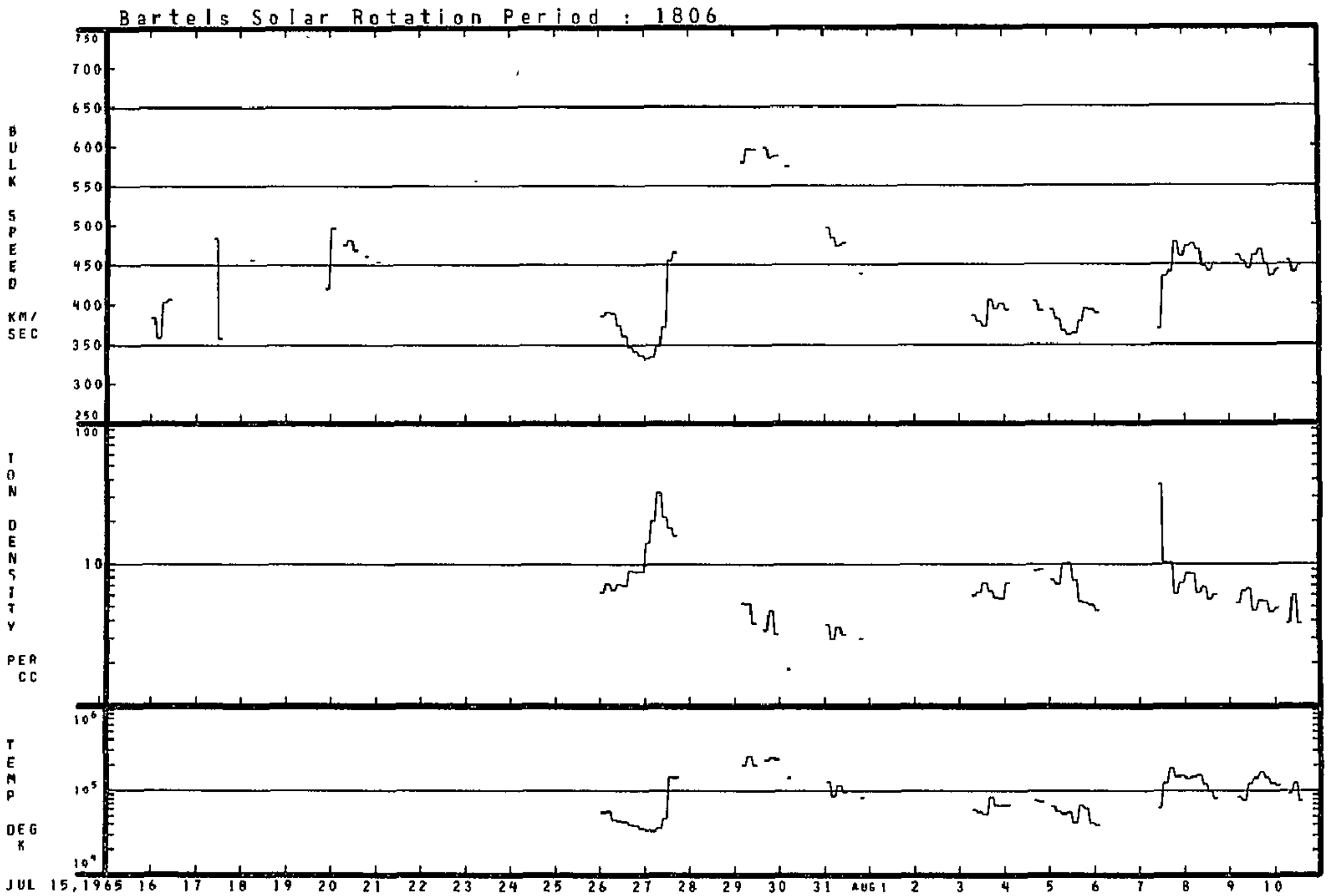
06/18/65 - 07/14/65



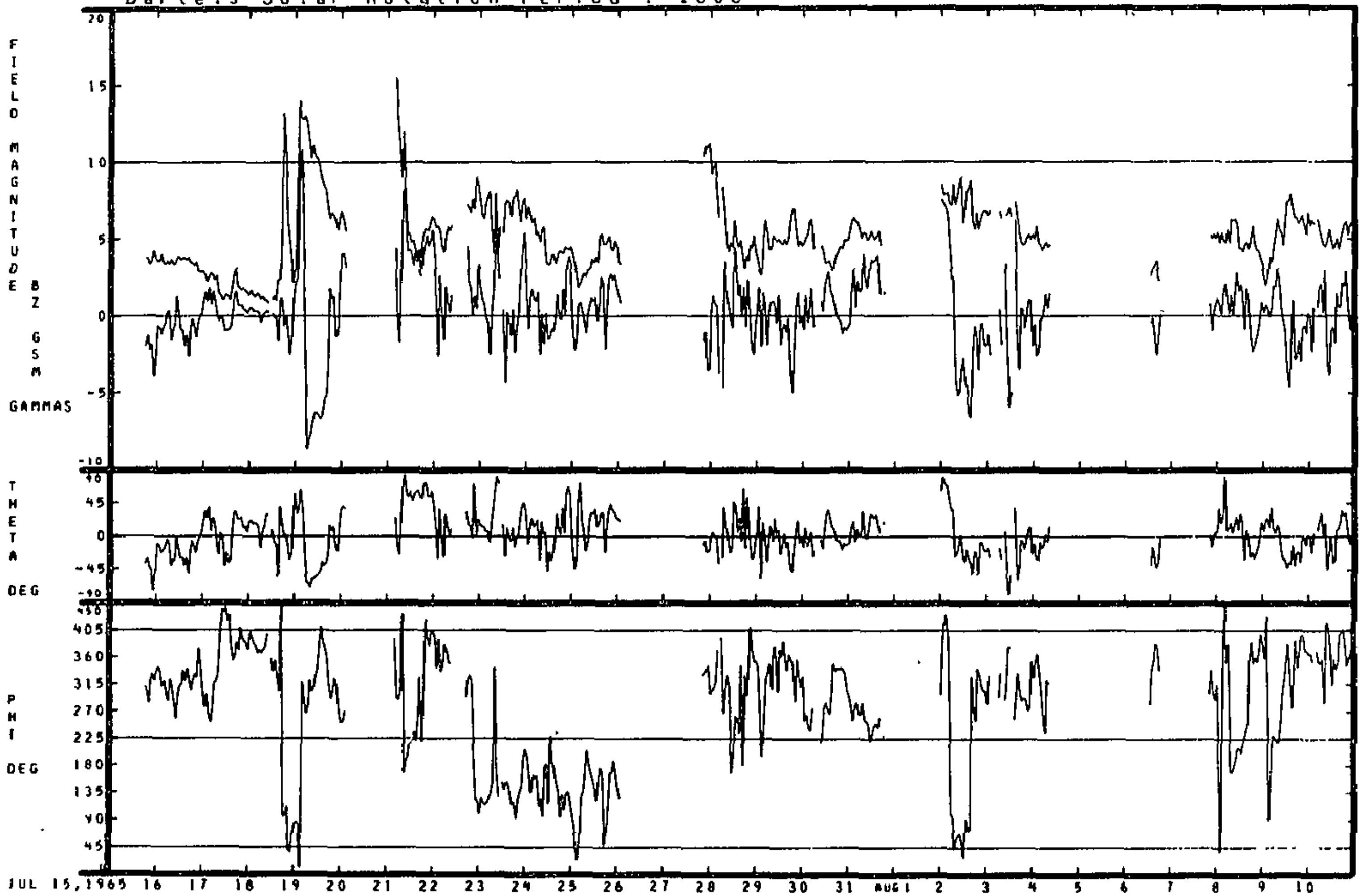


06/18/65 - 07/14/65

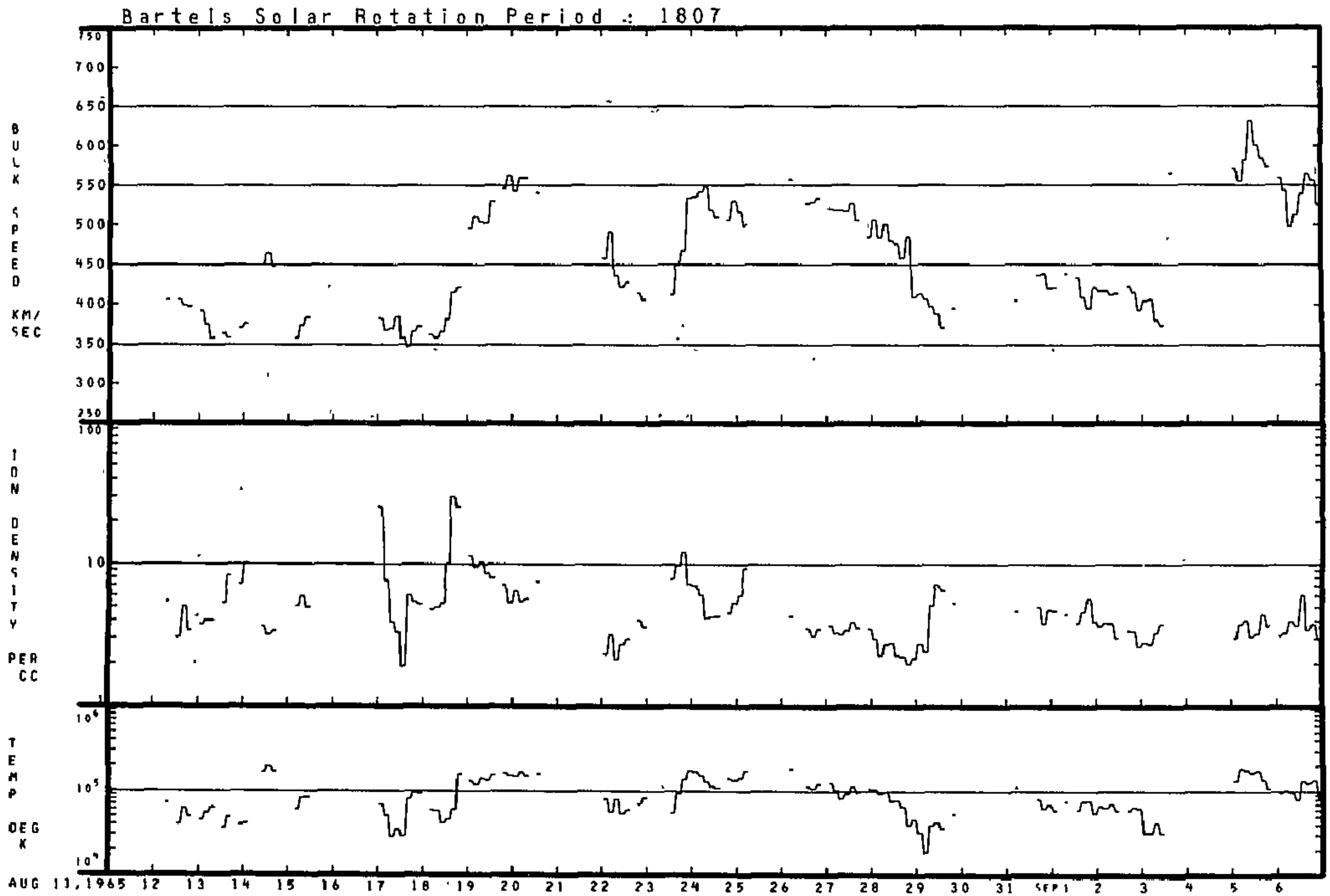
07/15/65 - 08/10/65



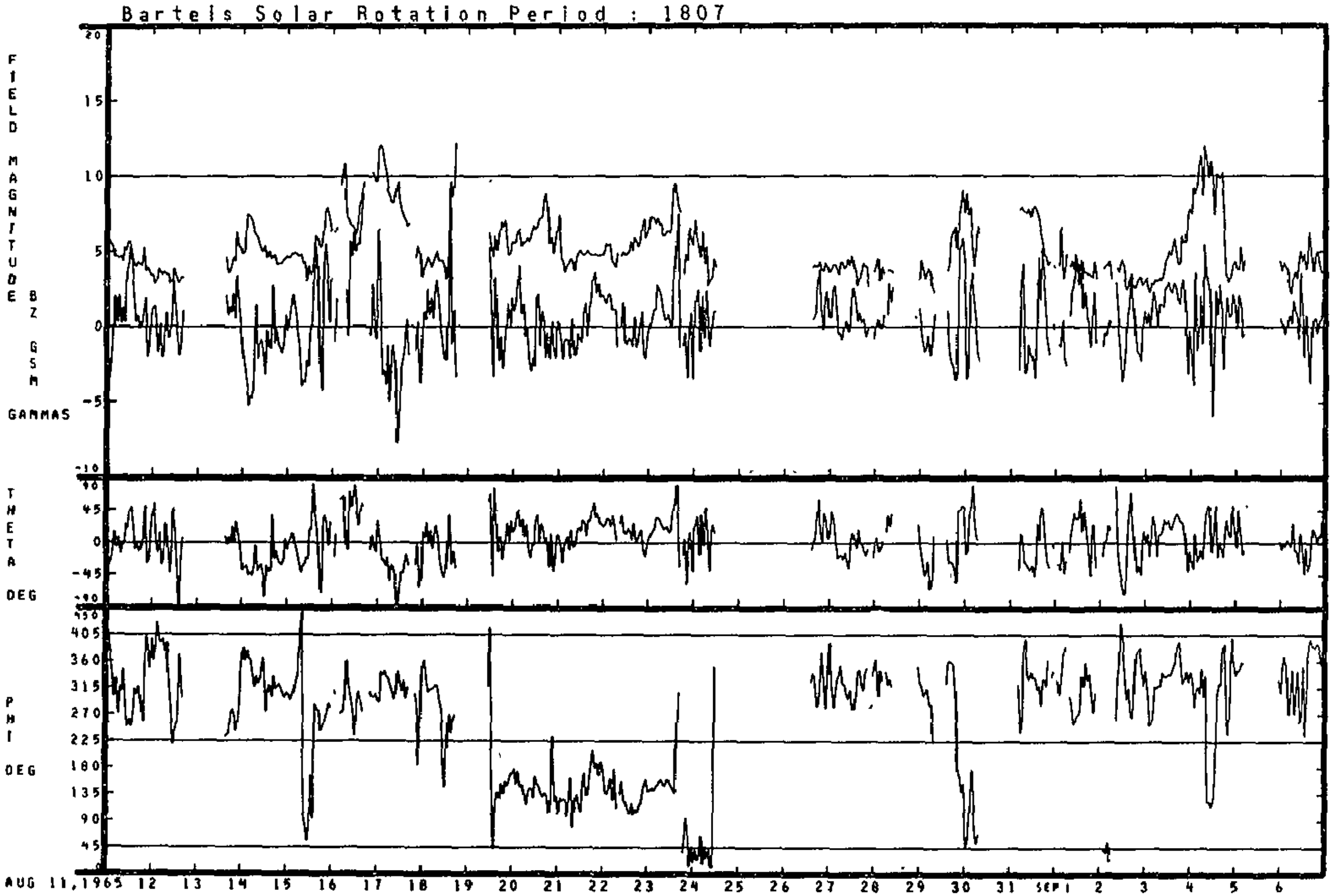
Bartels Solar Rotation Period : 1806



07/15/65 - 08/10/65



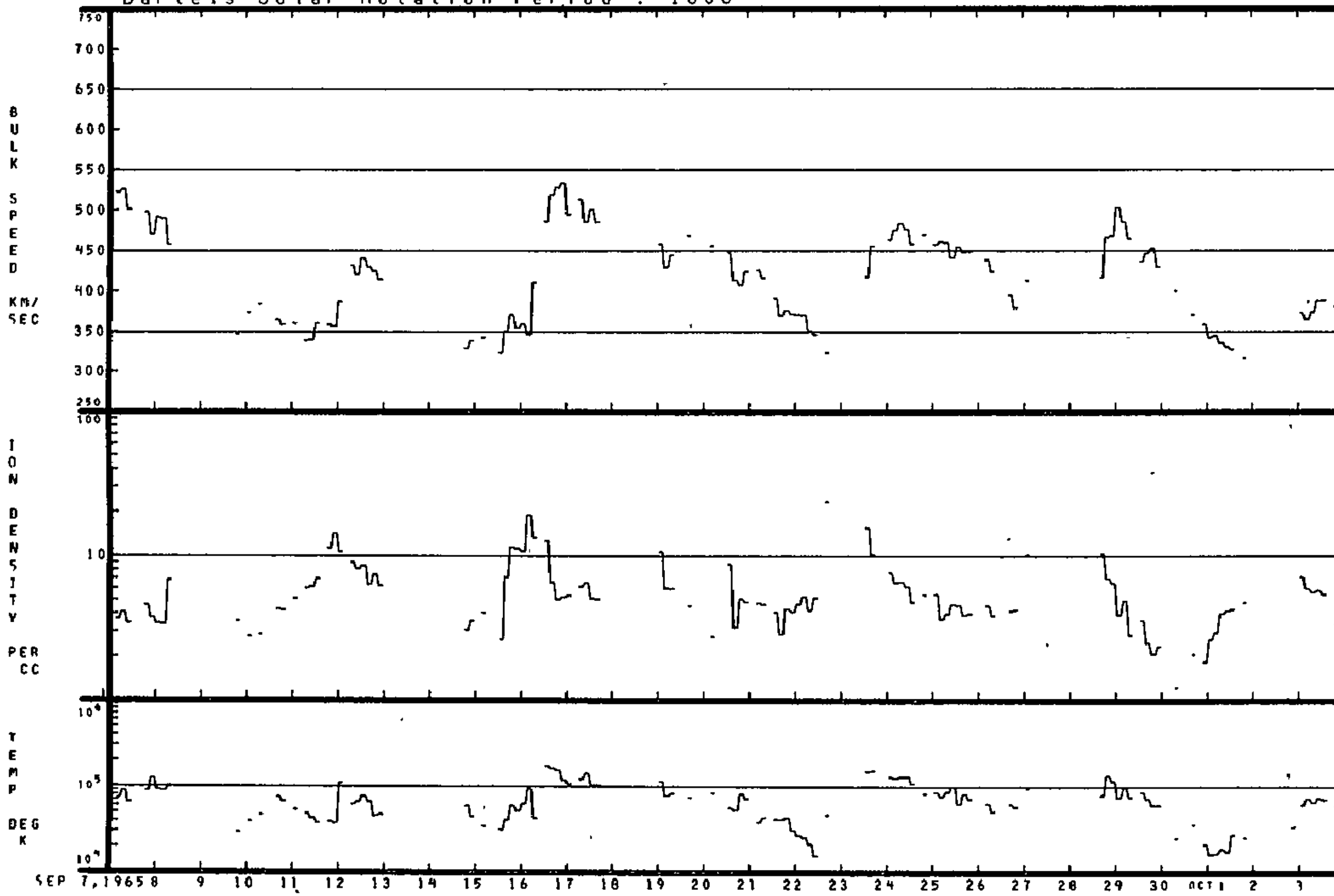
08/11/65 - 09/06/65

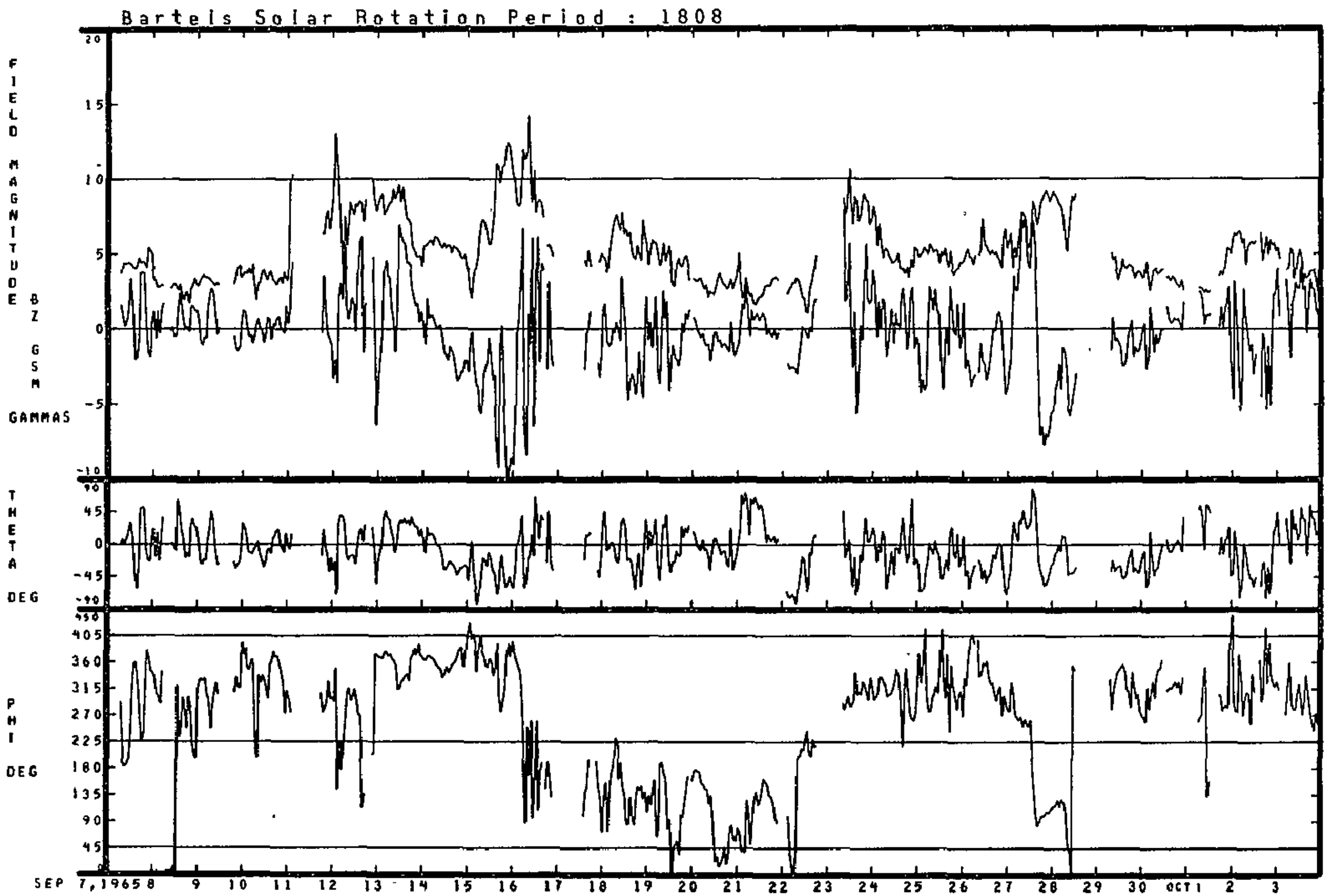


08/11/65 - 09/06/65

09/07/65 - 10/03/65

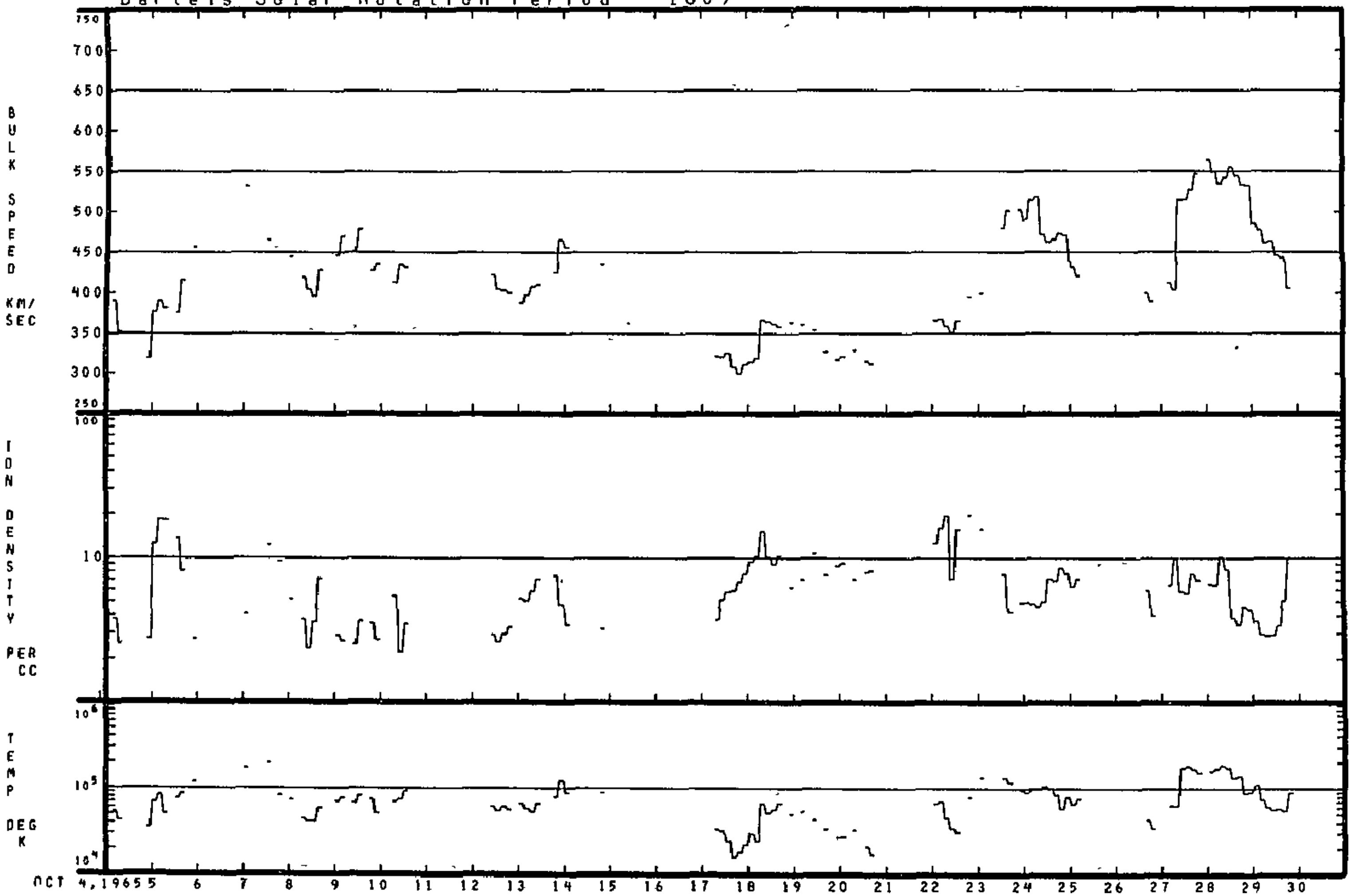
Bartels Solar Rotation Period : 1808





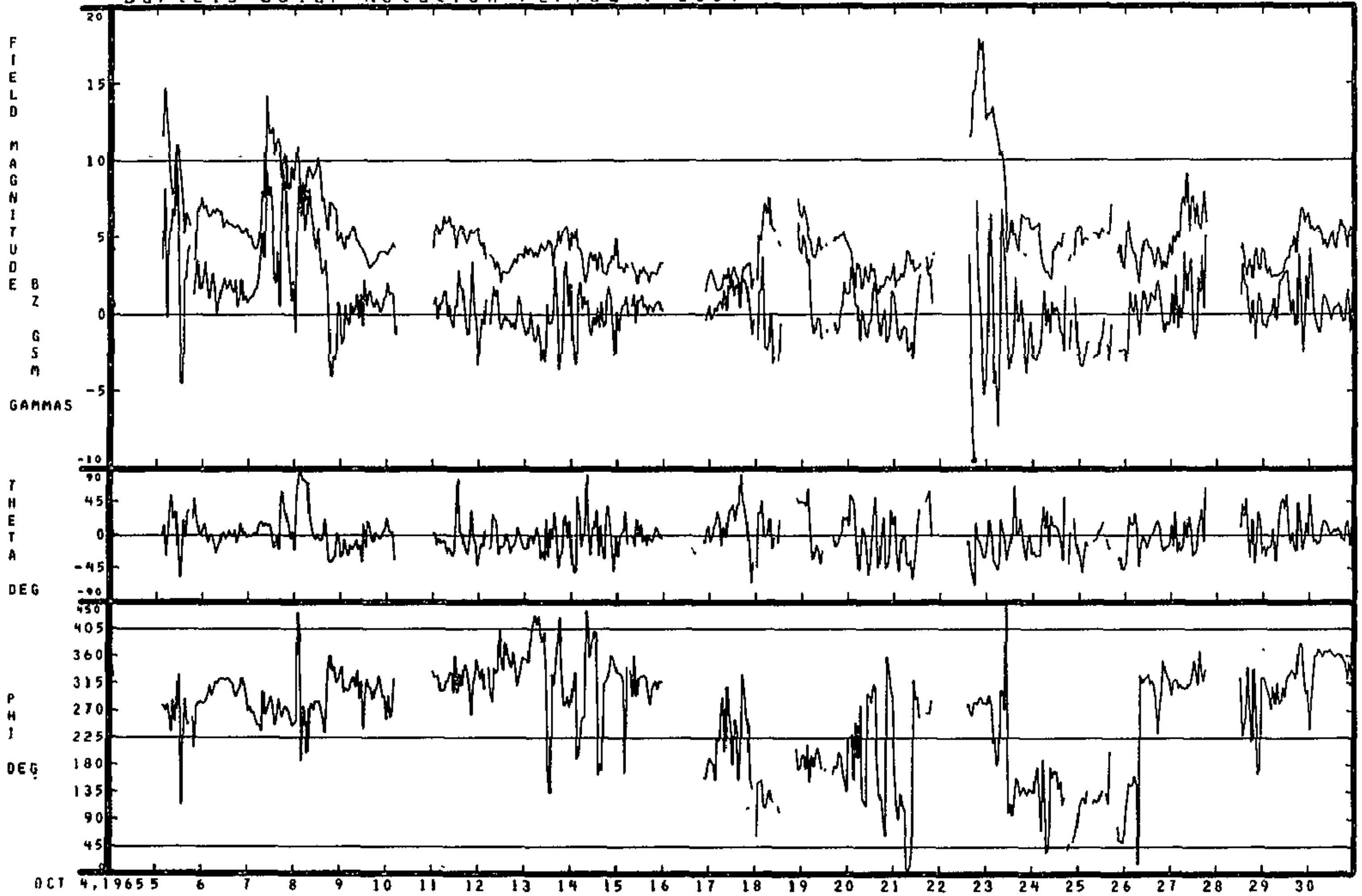
09/07/65 - 10/03/65

Bartels Solar Rotation Period - 1809

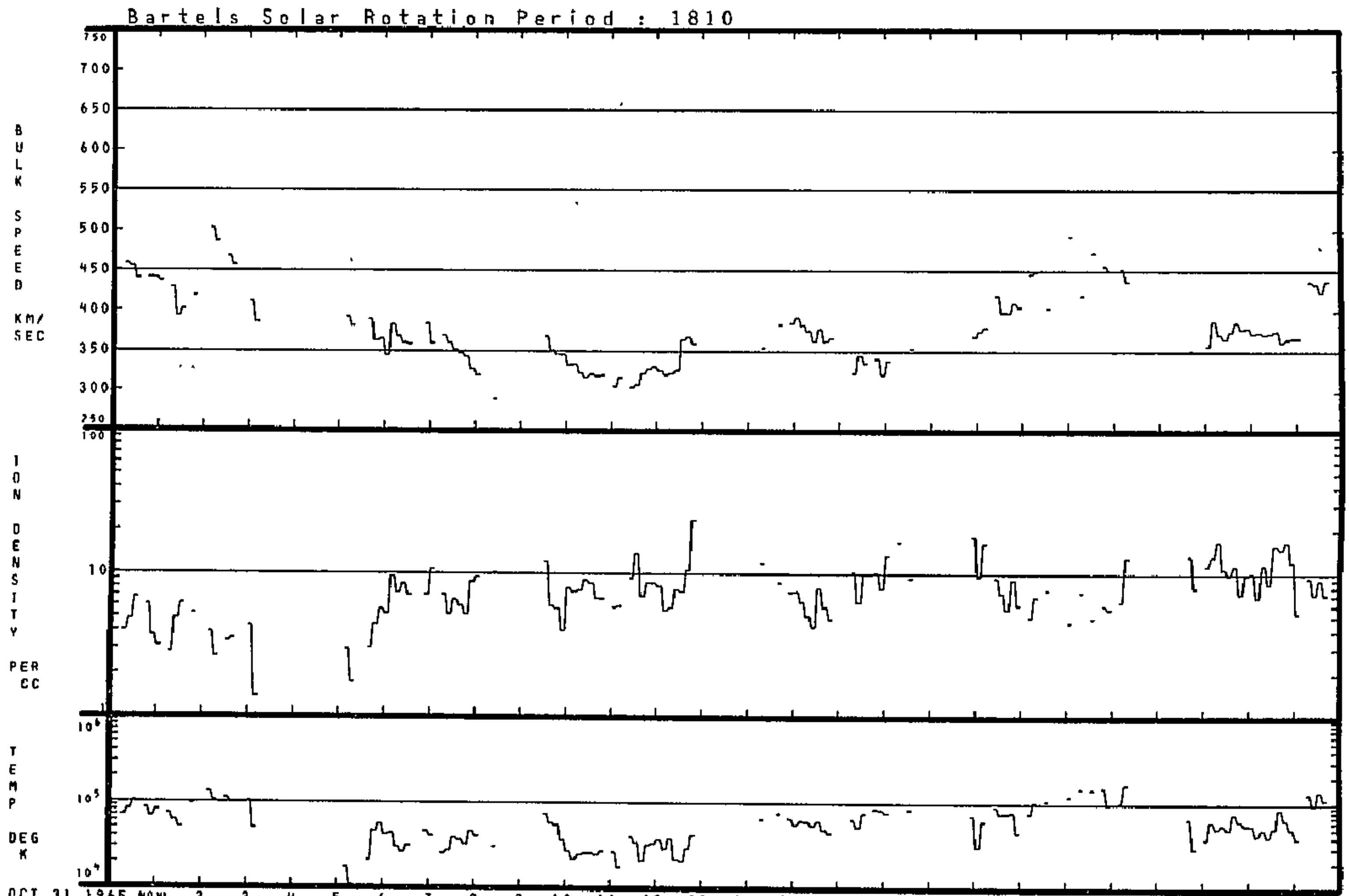


10/04/65 - 10/30/65

Bartels Solar Rotation Period : 1809

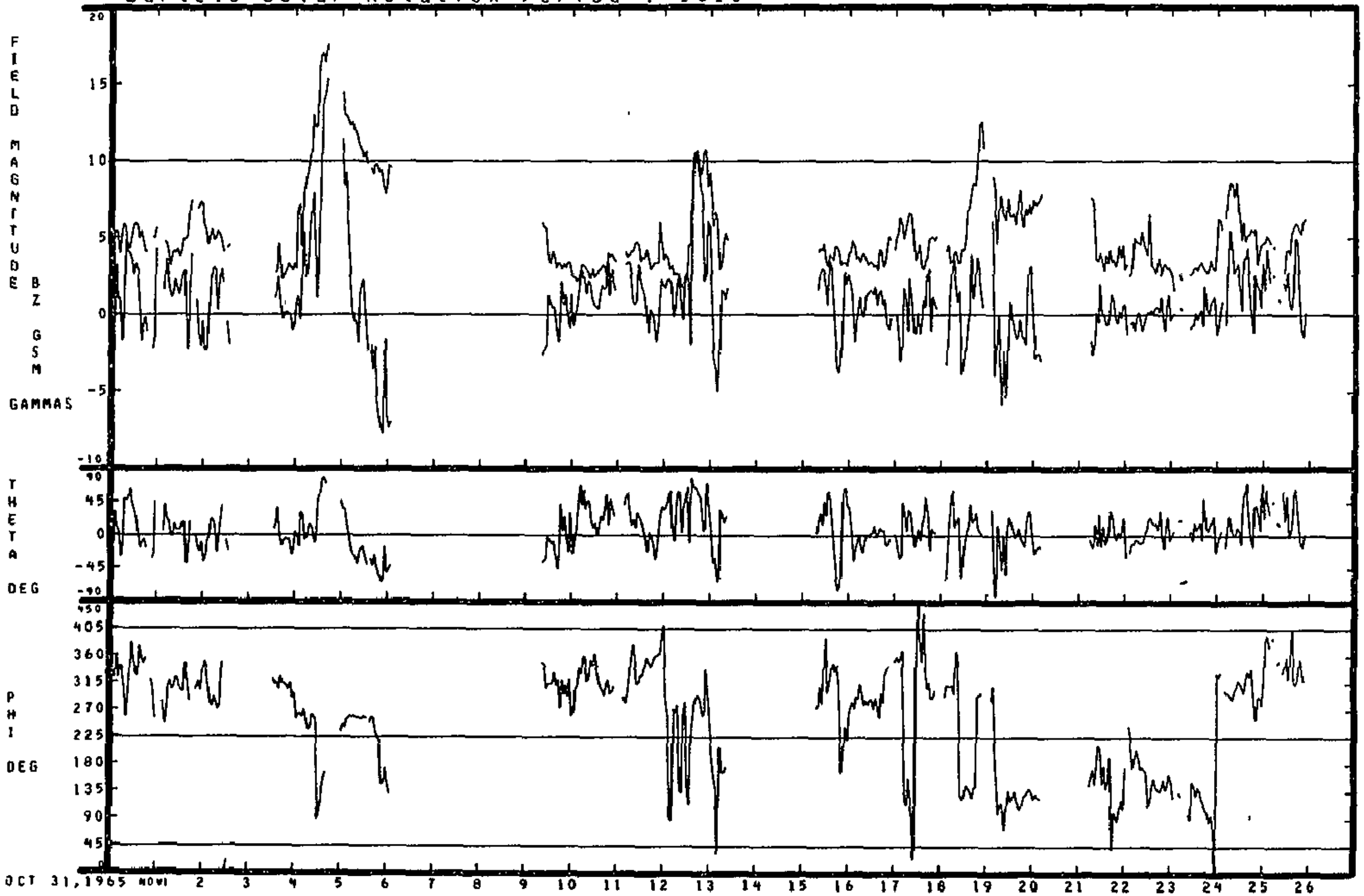


10/04/65 - 10/30/65



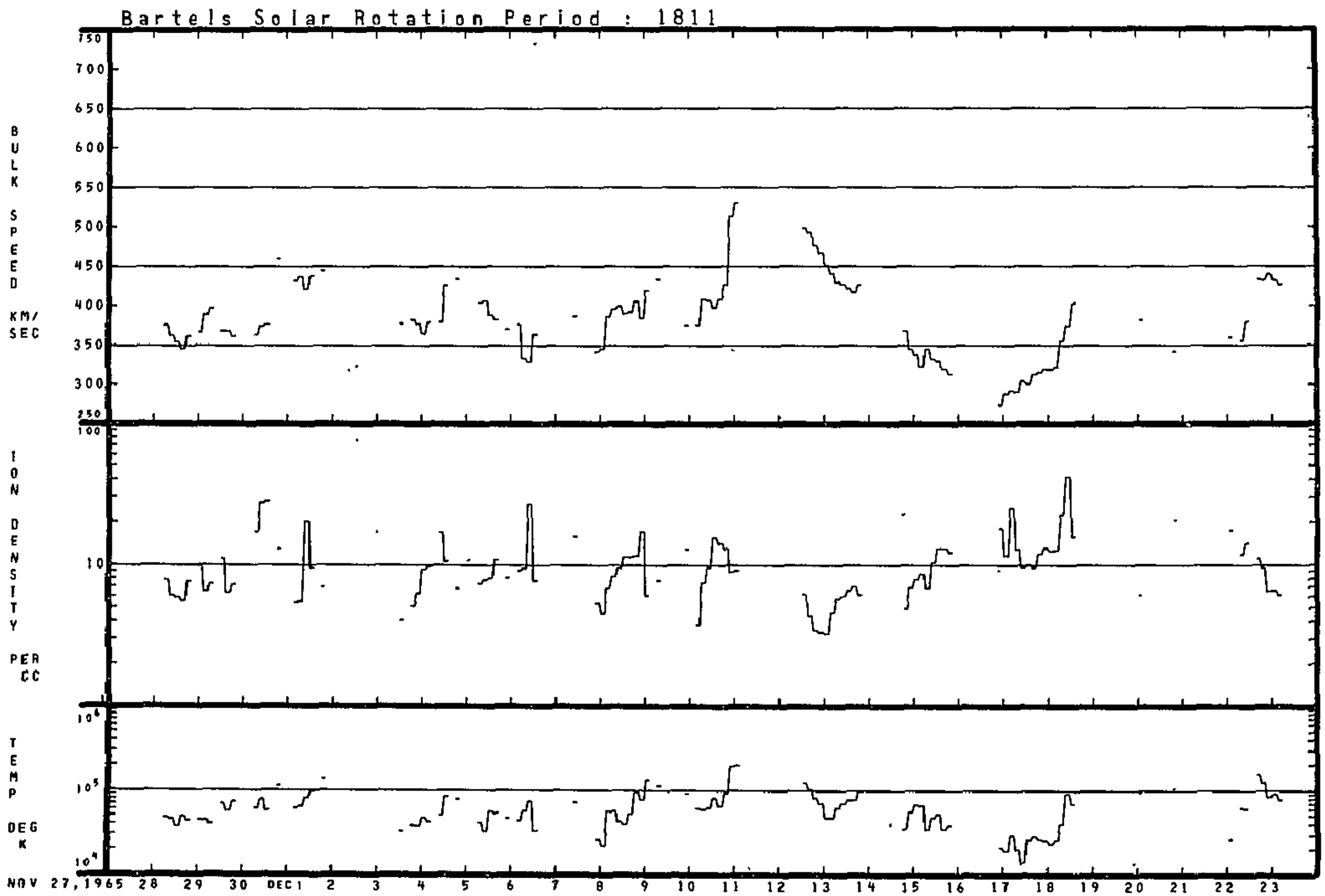
10/31/65 - 11/26/65

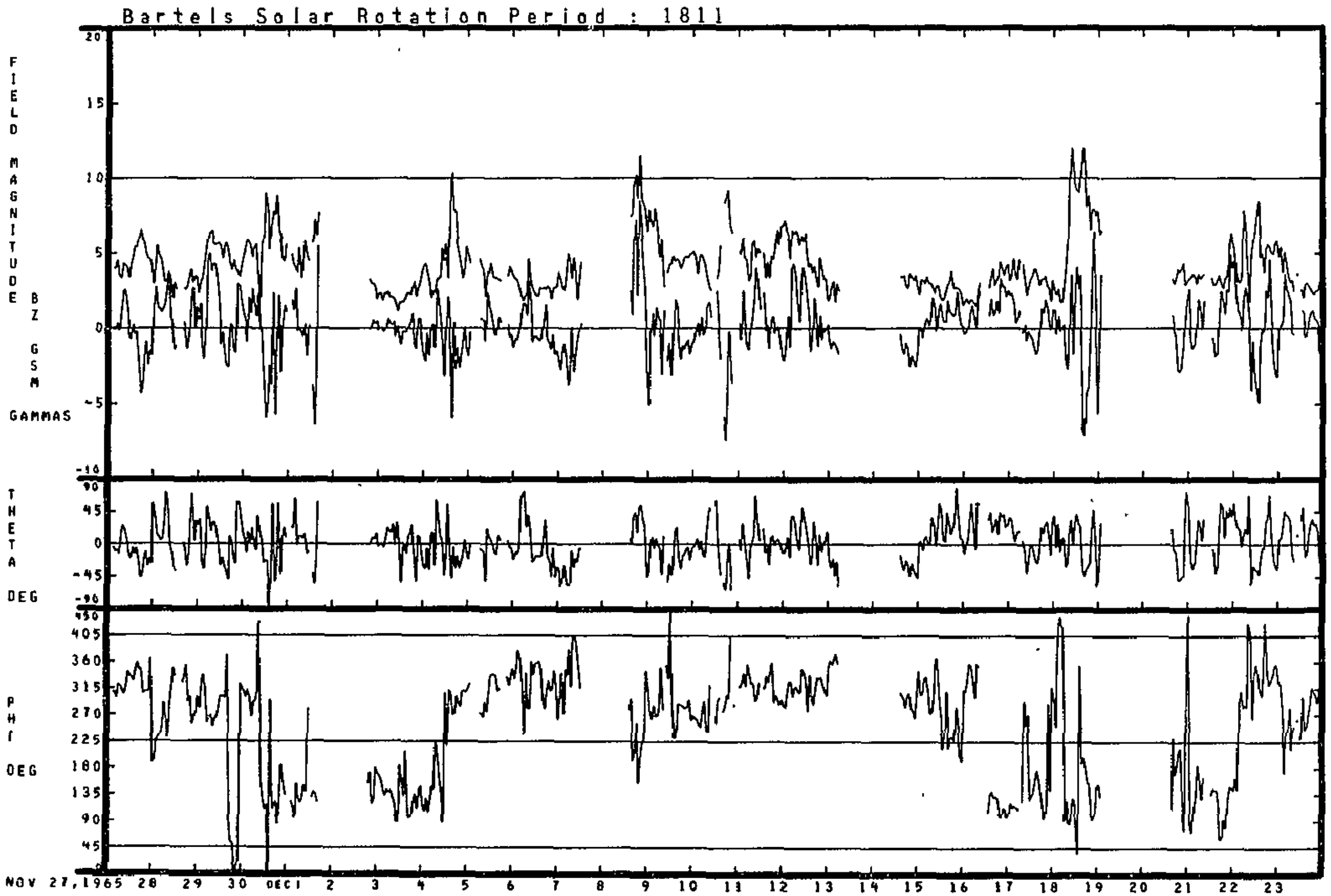
Bartels Solar Rotation Period : 1810



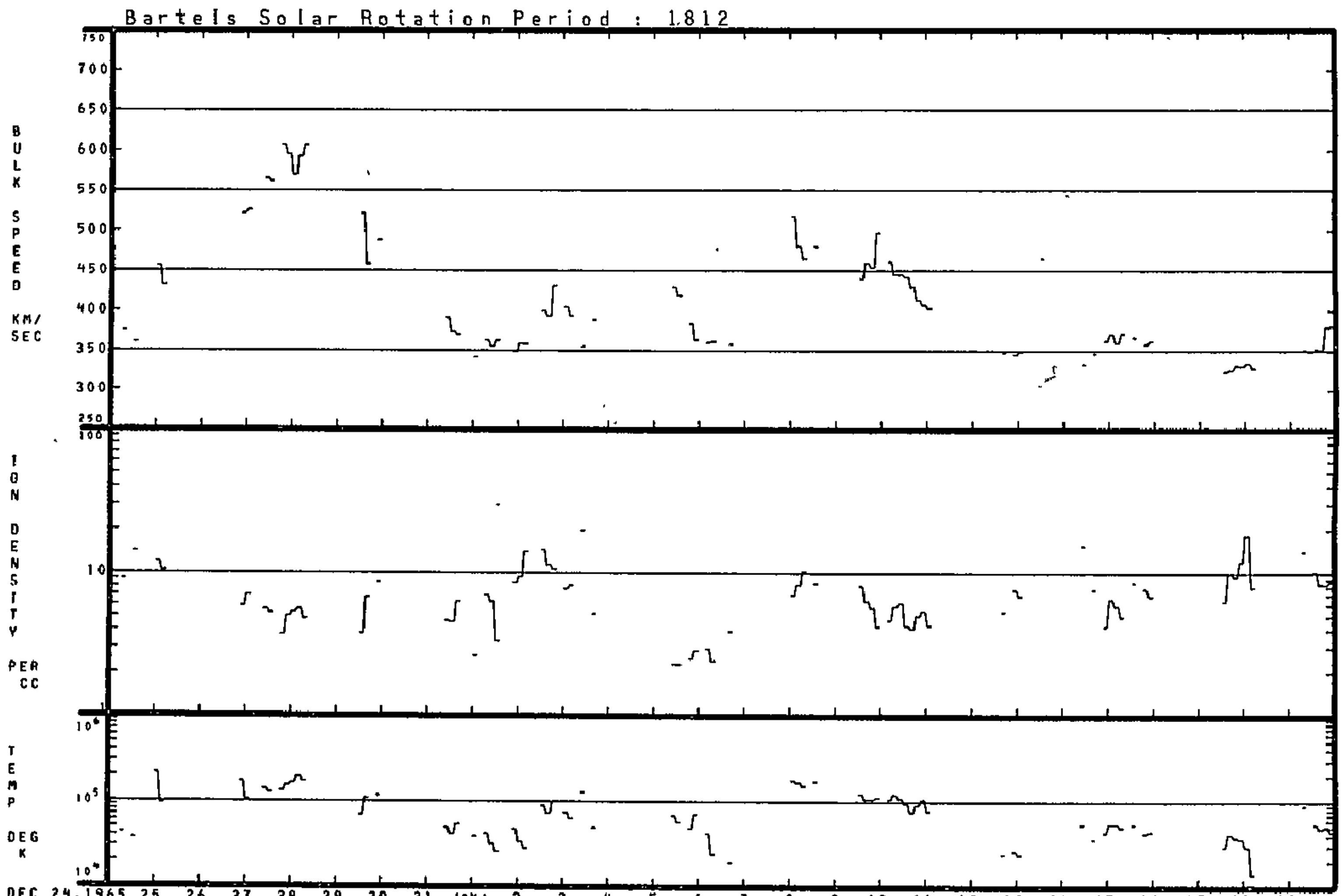
10/31/65 - 11/26/65

11/27/65 - 12/23/65

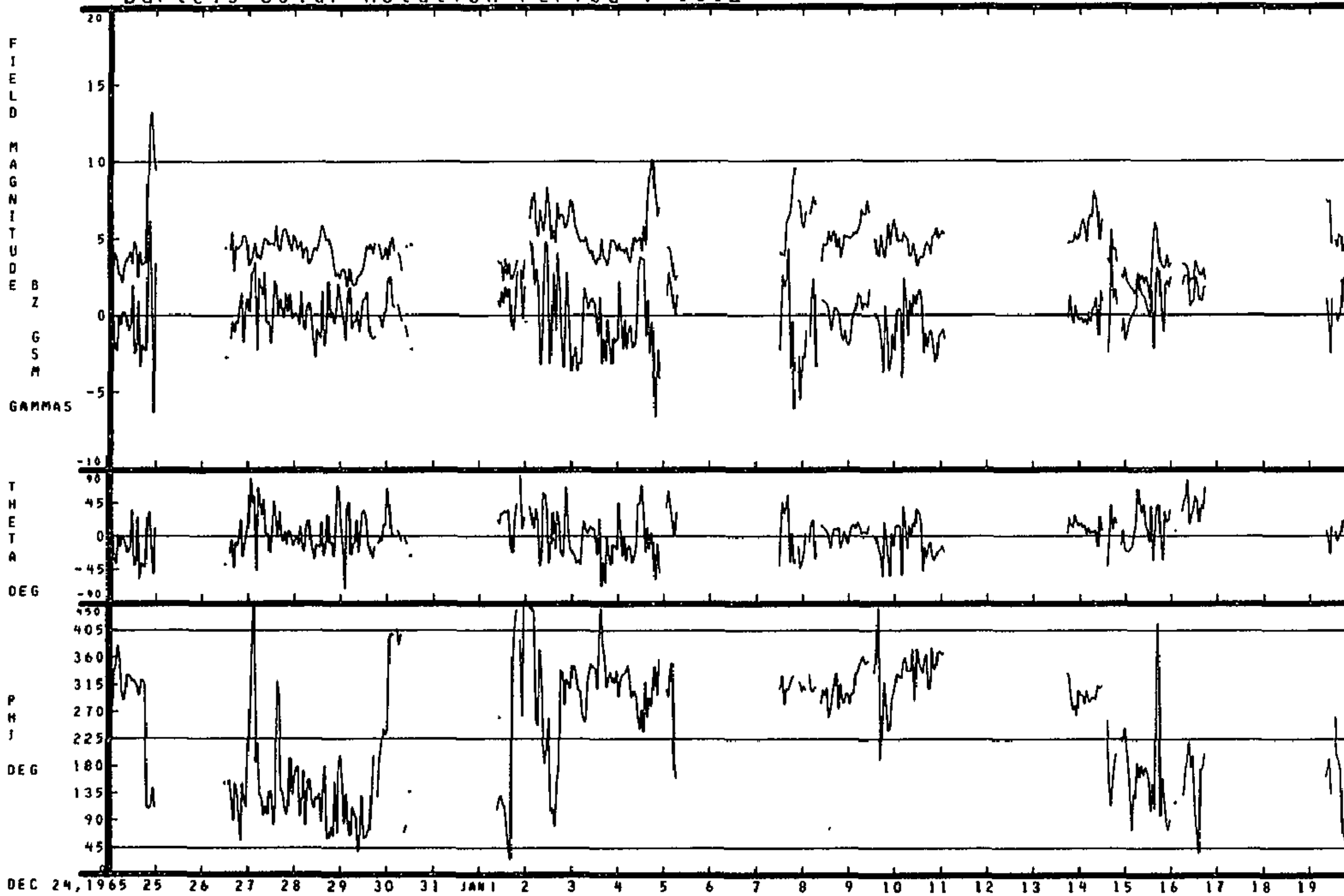




11/27/65 - 12/23/65

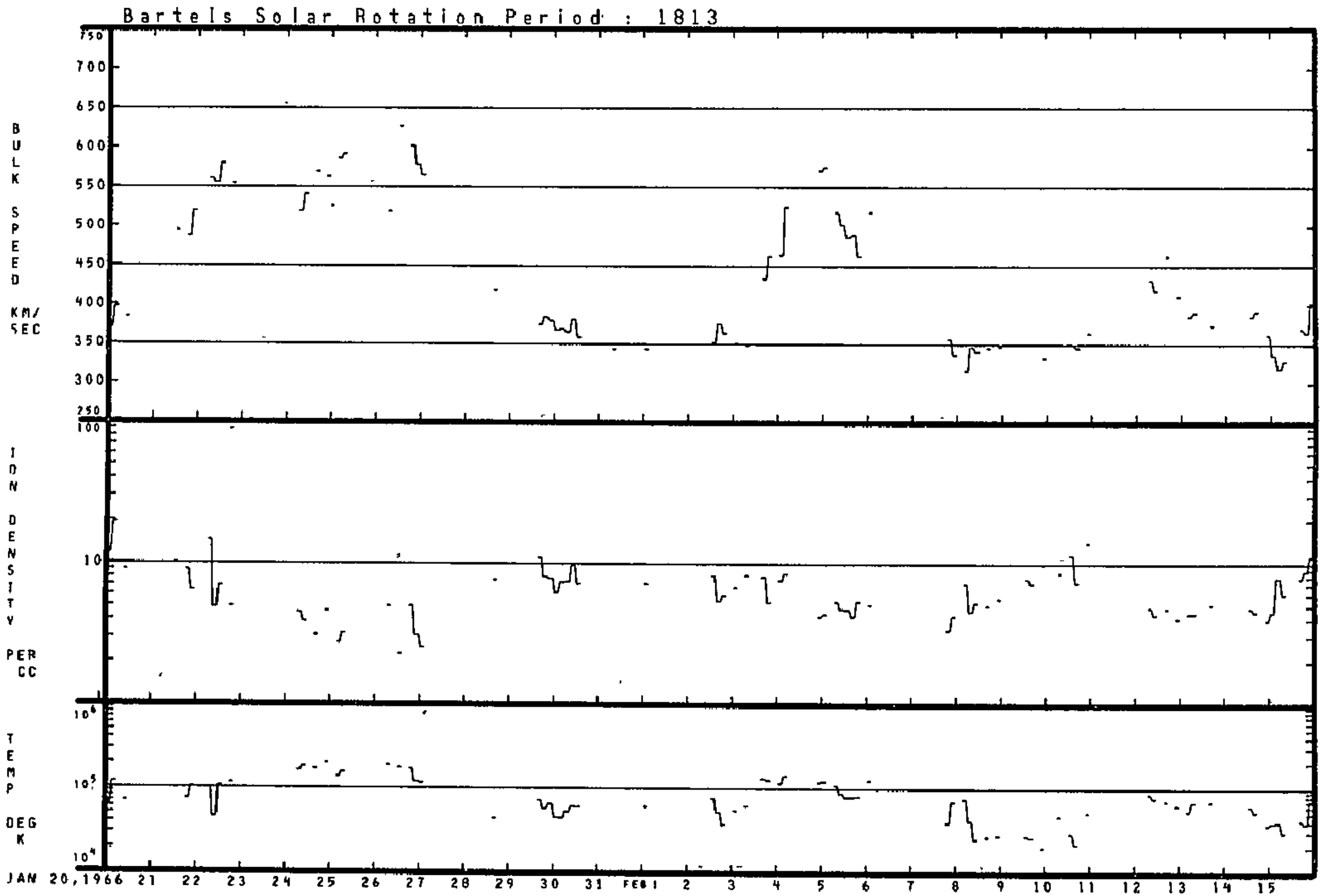

Bartels Solar Rotation Period : 1812
BULK SPEED KM/SEC
750
700
650
600
550
500
450
400
350
300
250
ION DENSITY PER CC
100
10
1
TEMP DEG K
10^6
10^5
10^4
DEC 24,1965 25 26 27 28 29 30 31 JAN 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19
12/24/65 - 01/19/66

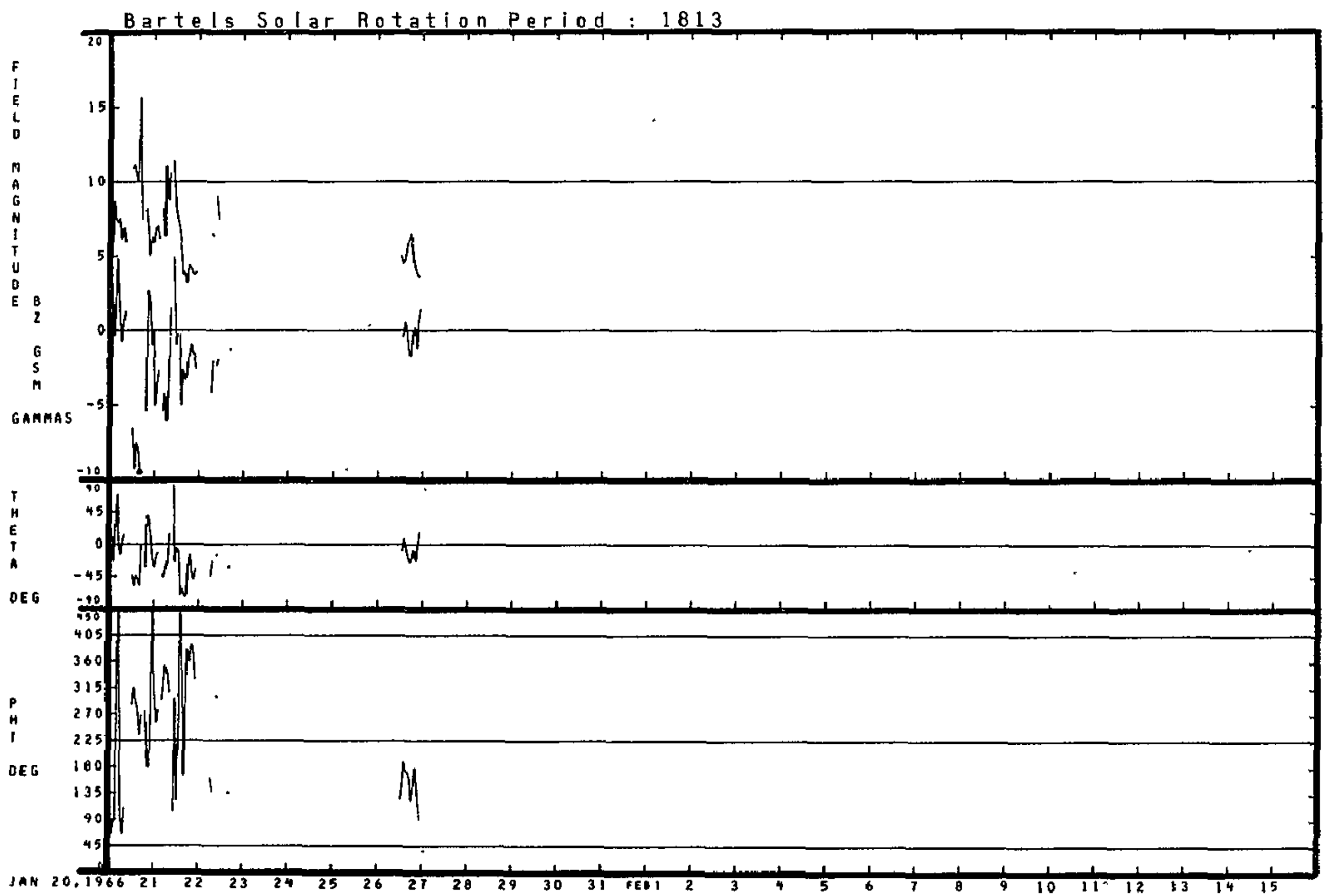
Bartels Solar Rotation Period : 1812



12/24/65 - 01/19/66

01/20/66 - 02/15/66

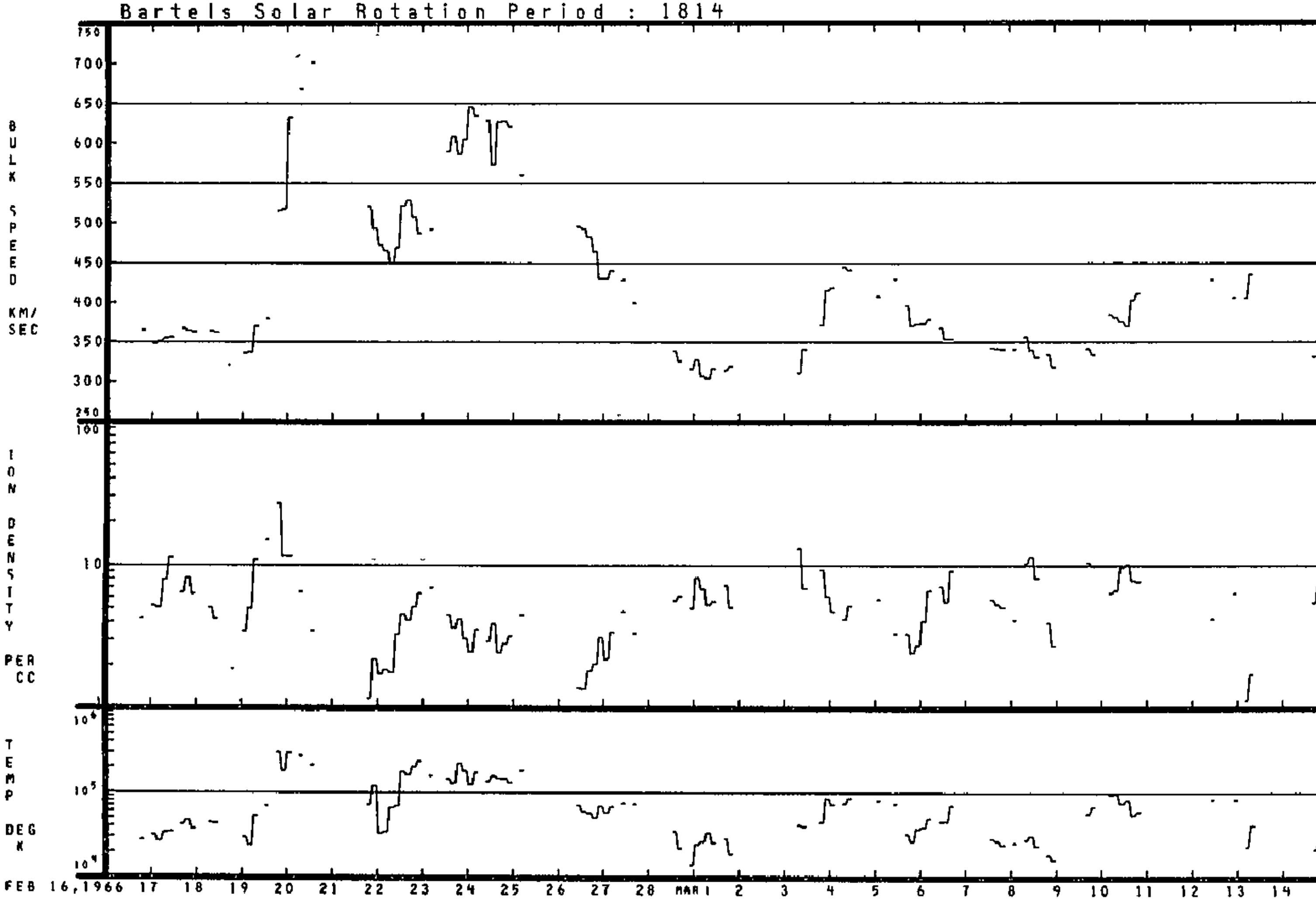


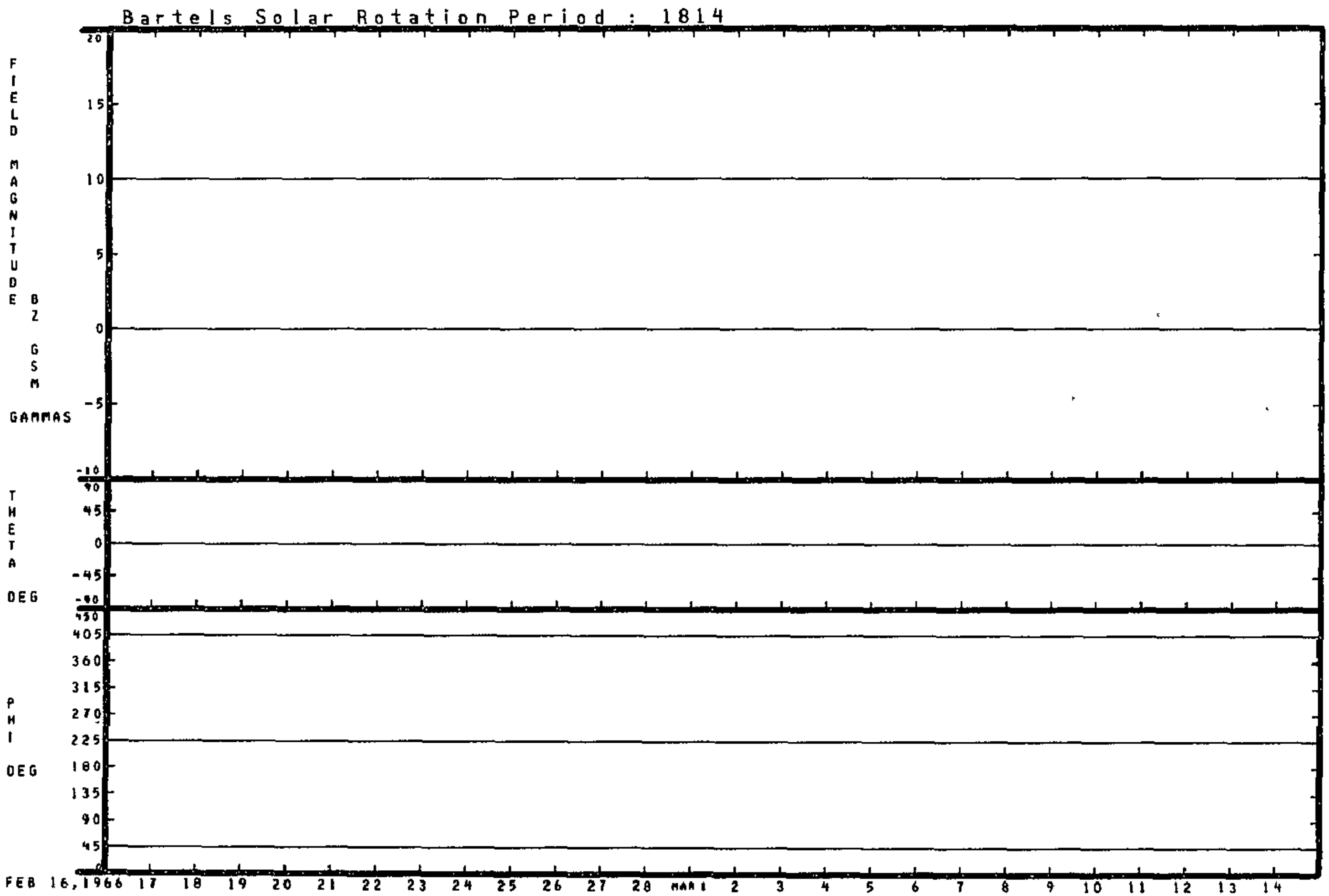


01/20/66 - 02/15/66

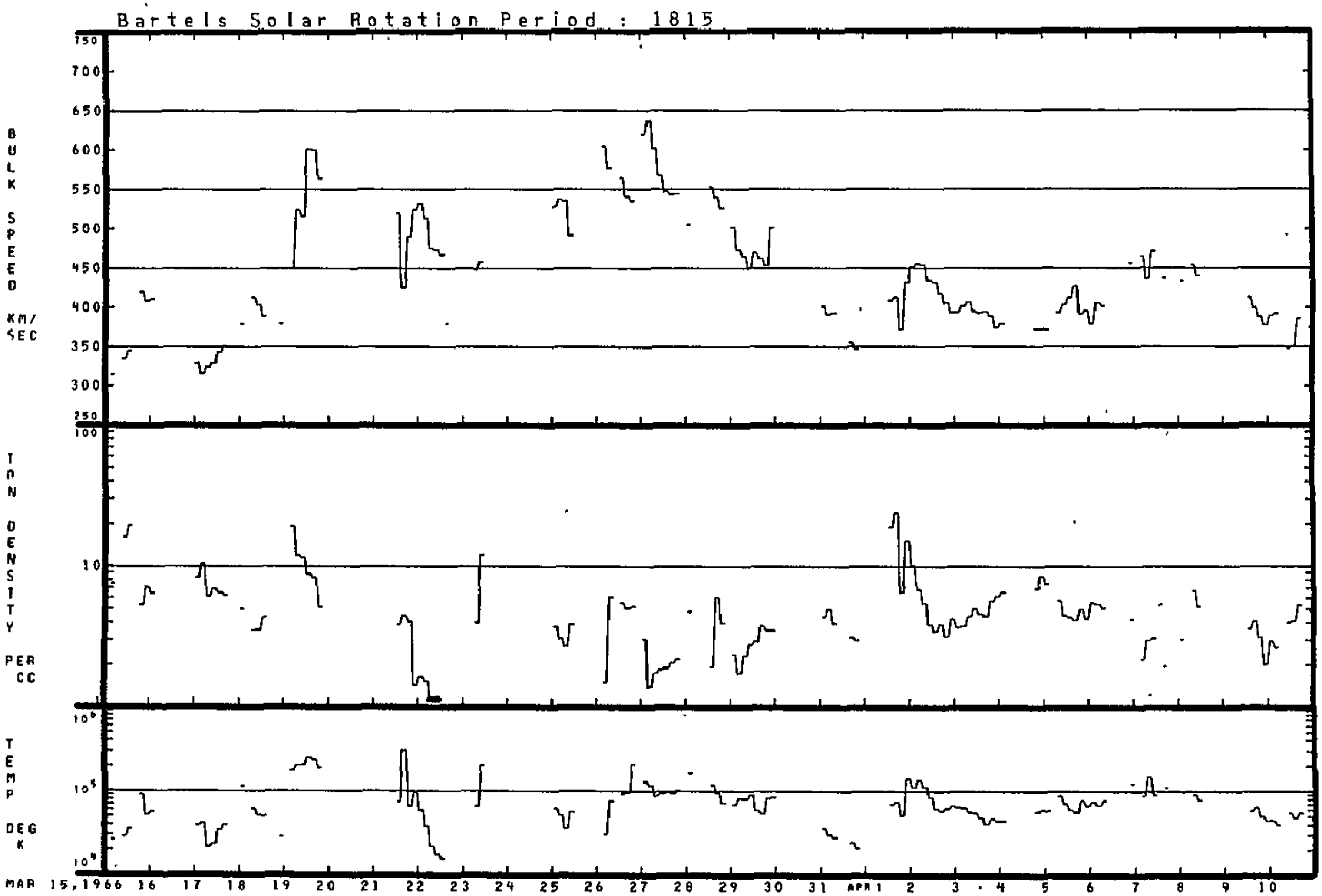
02/16/66 - 03/14/66

Bartels Solar Rotation Period : 1814



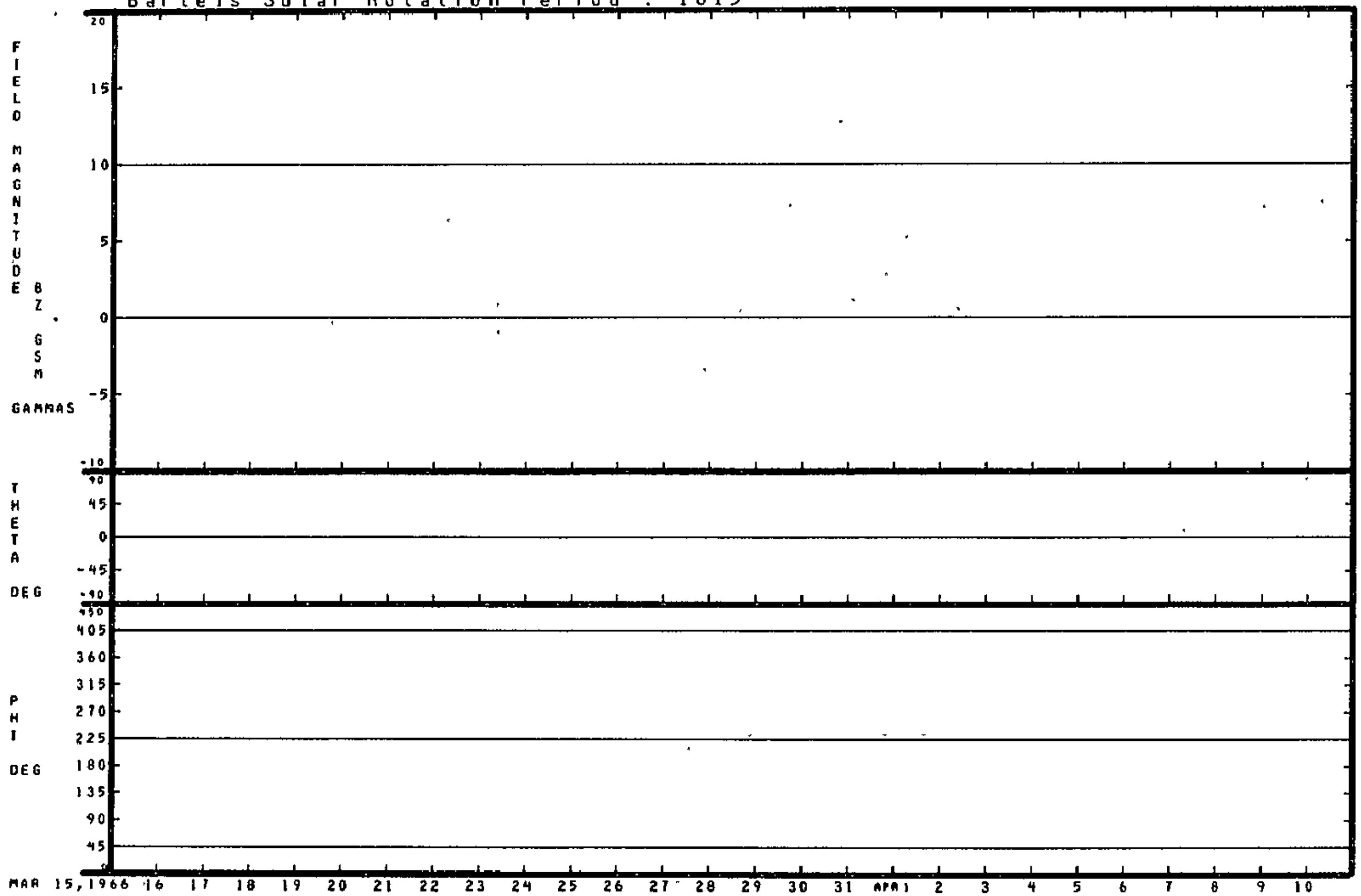


02/16/66 - 03/14/66



03/15/66 - 04/10/66

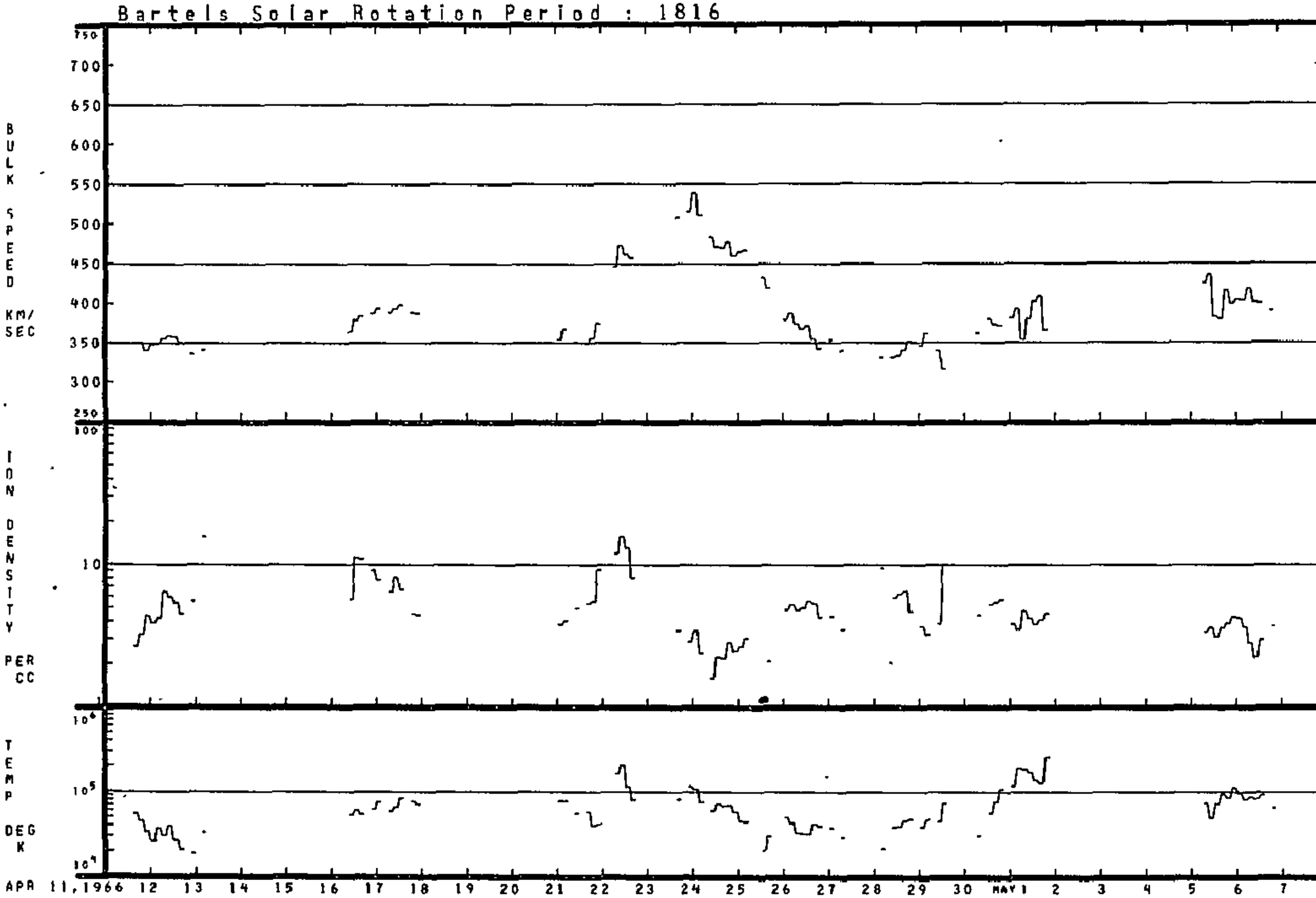
Bartels Solar Rotation Period : 1815

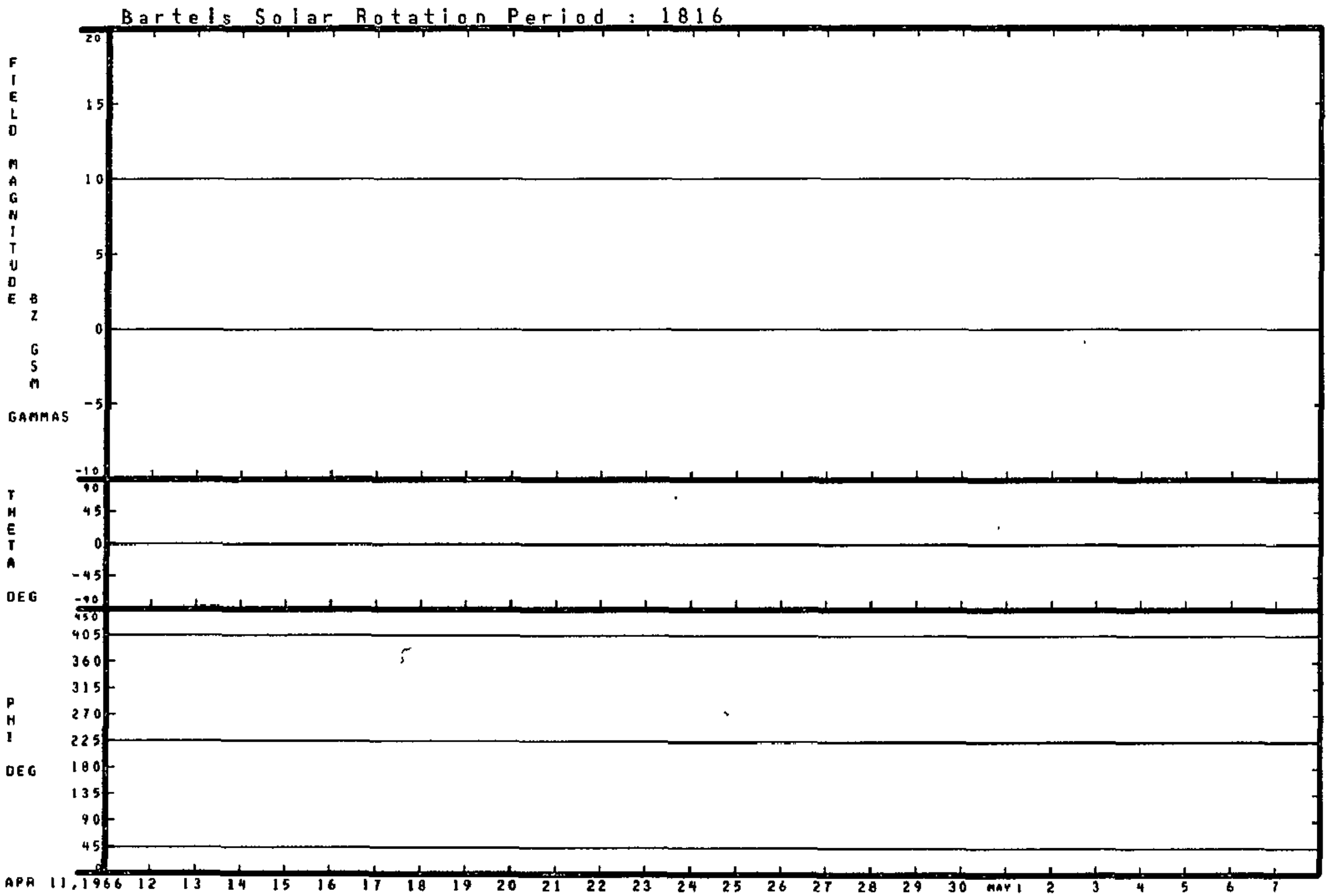


03/15/66 - 04/10/66

04/11/66 - 05/07/66

Bartels Solar Rotation Period : 1816

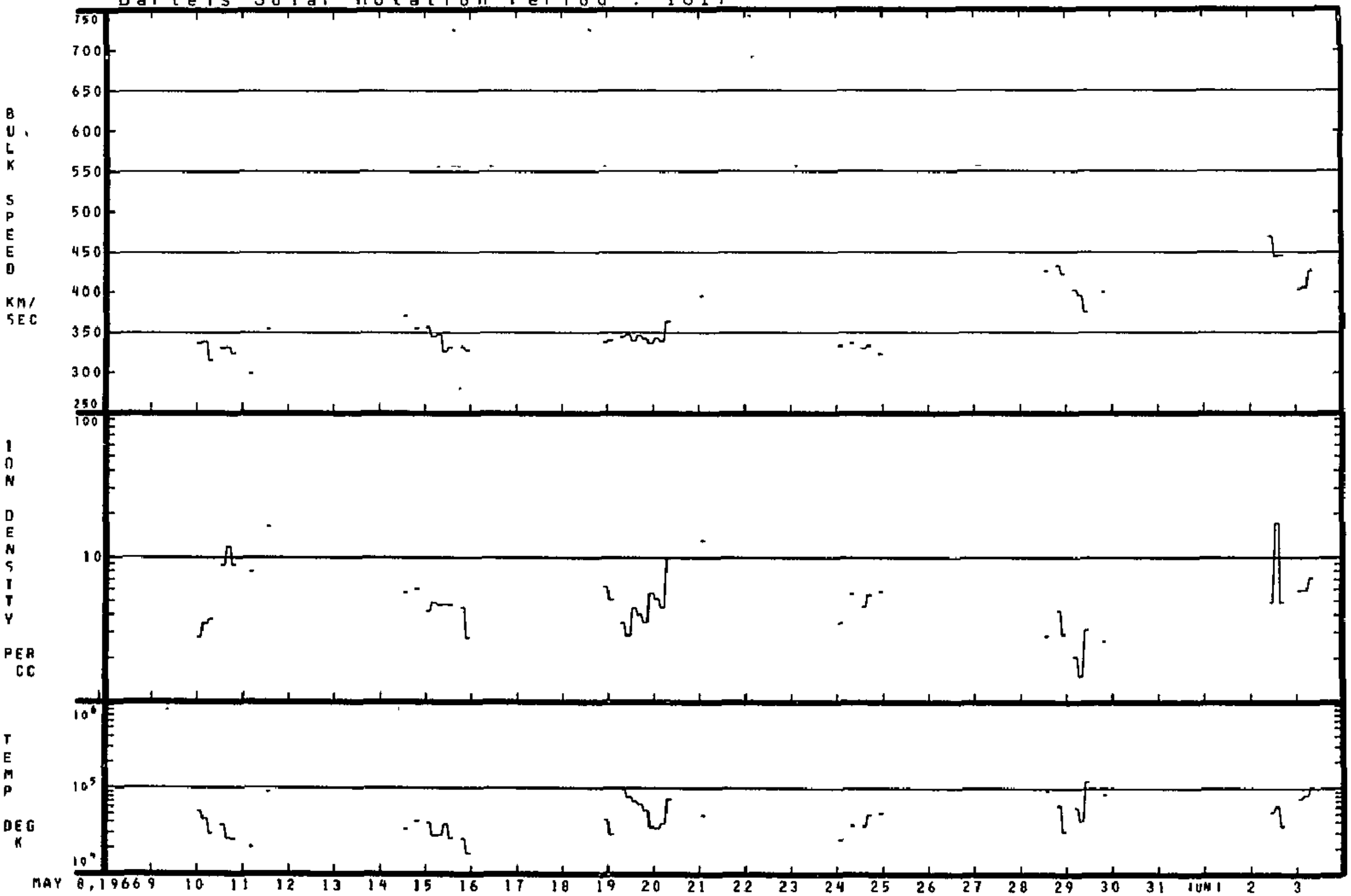


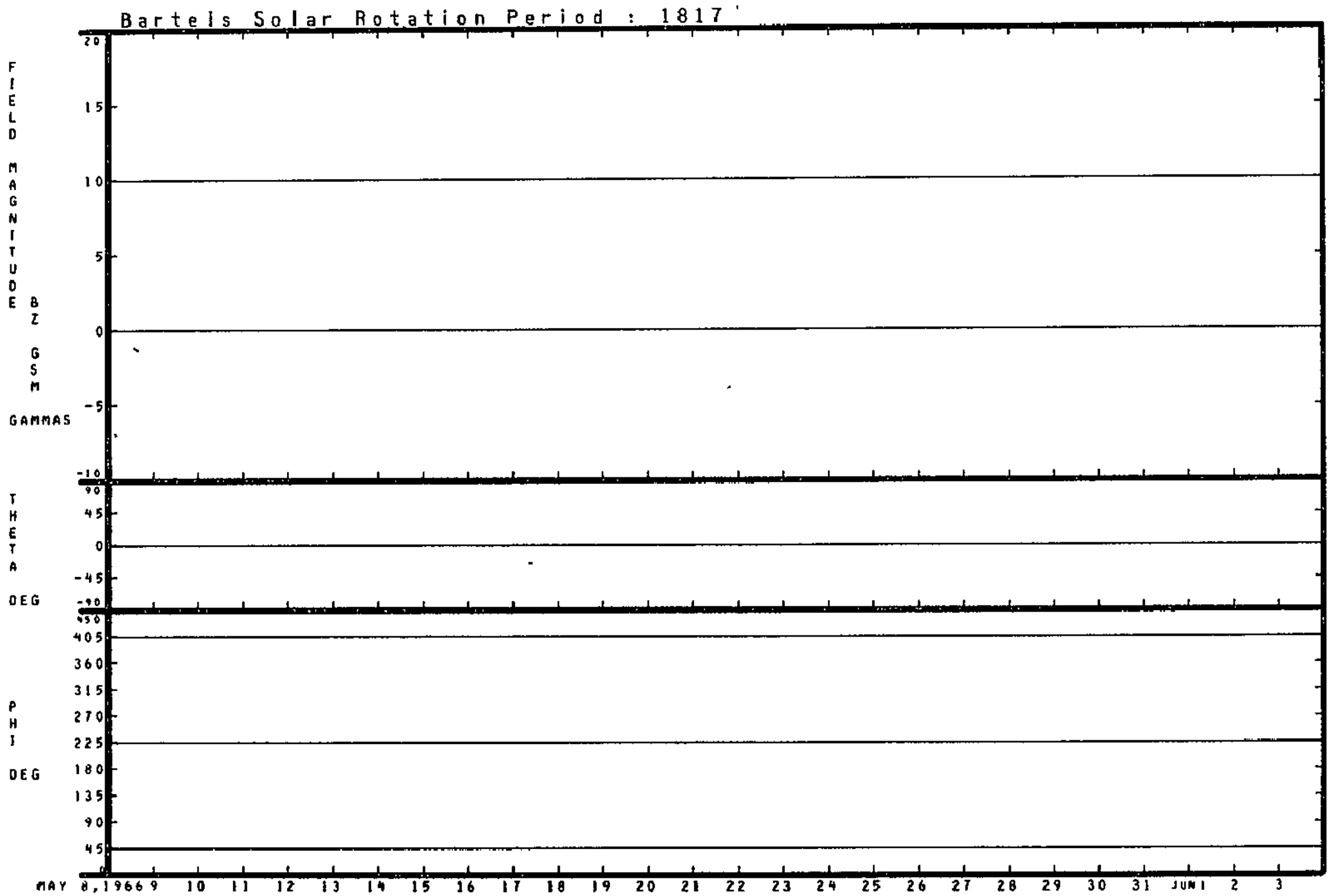


04/11/66 - 05/07/66

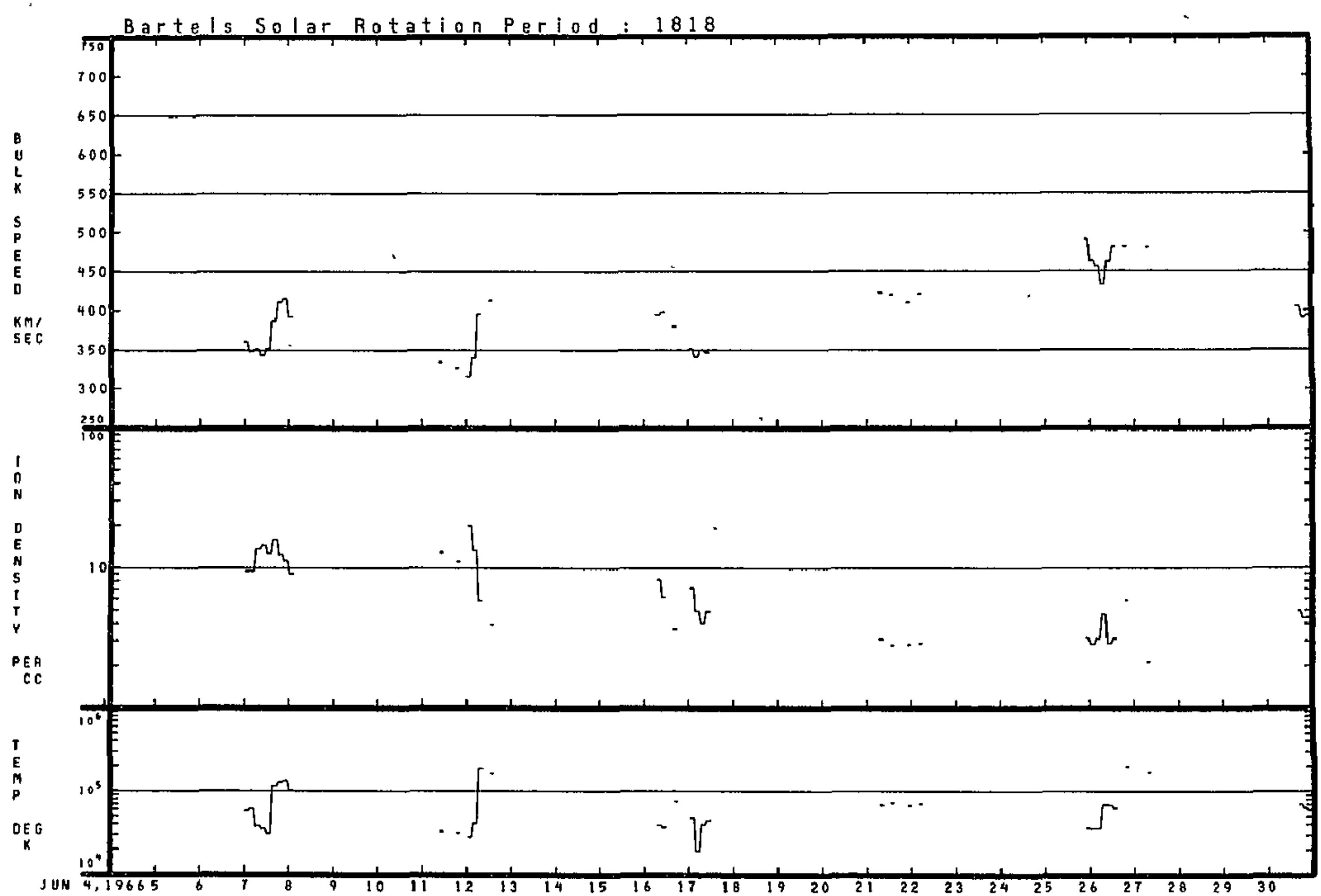
05/08/66 - 06/03/66

Bartels Solar Rotation Period : 1817



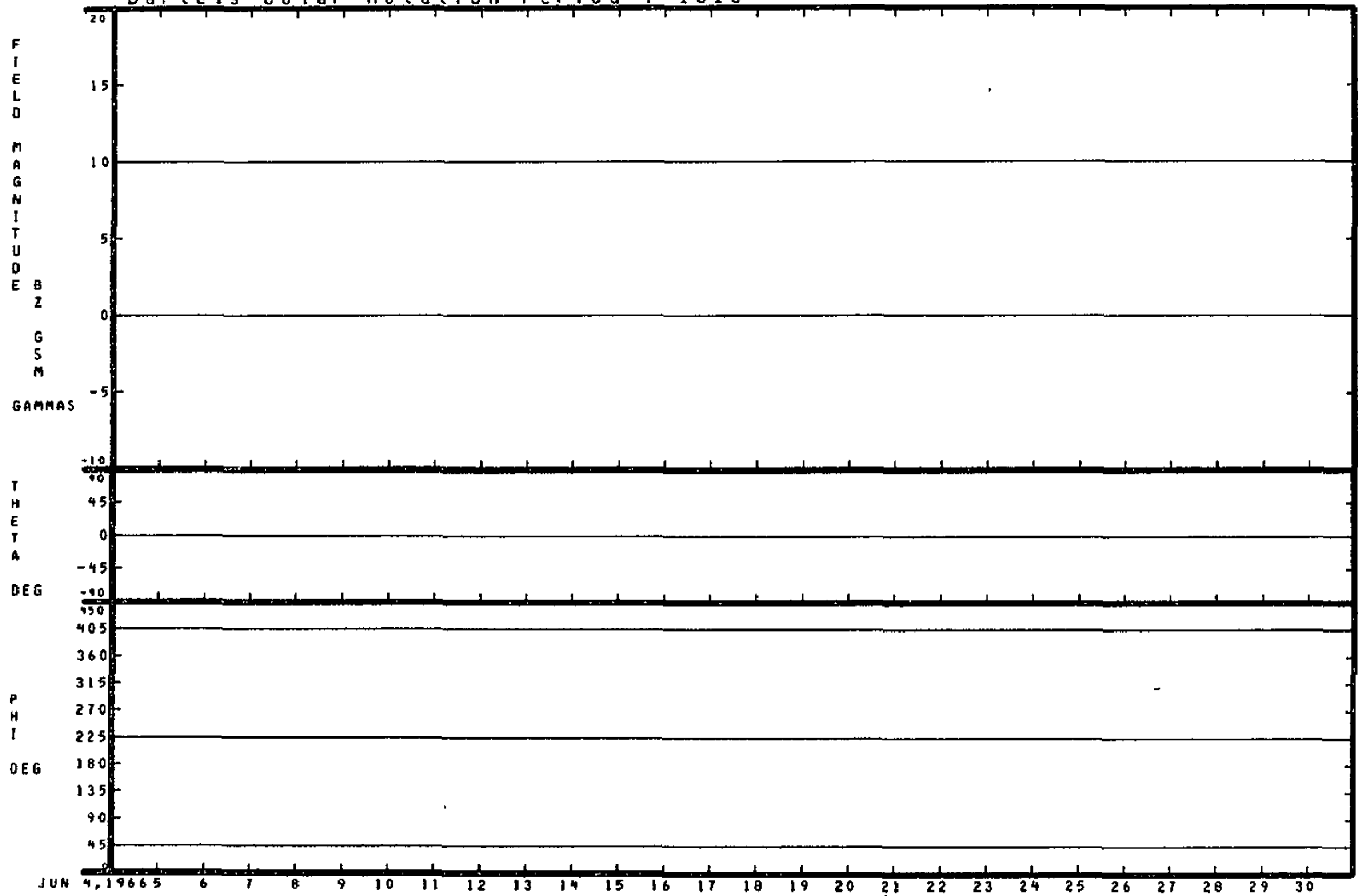


05/08/66 - 06/03/66



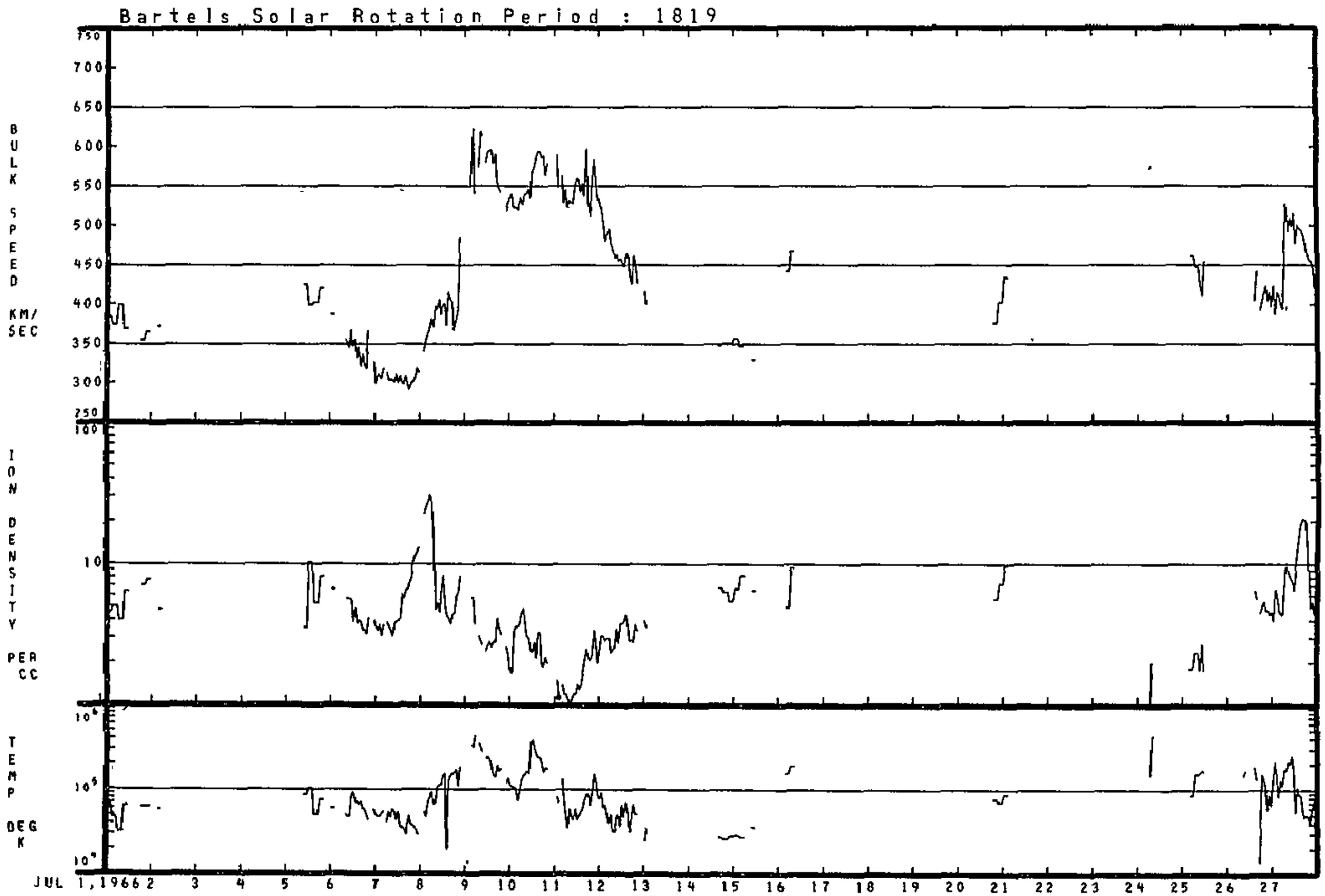
06/04/66 - 06/30/66

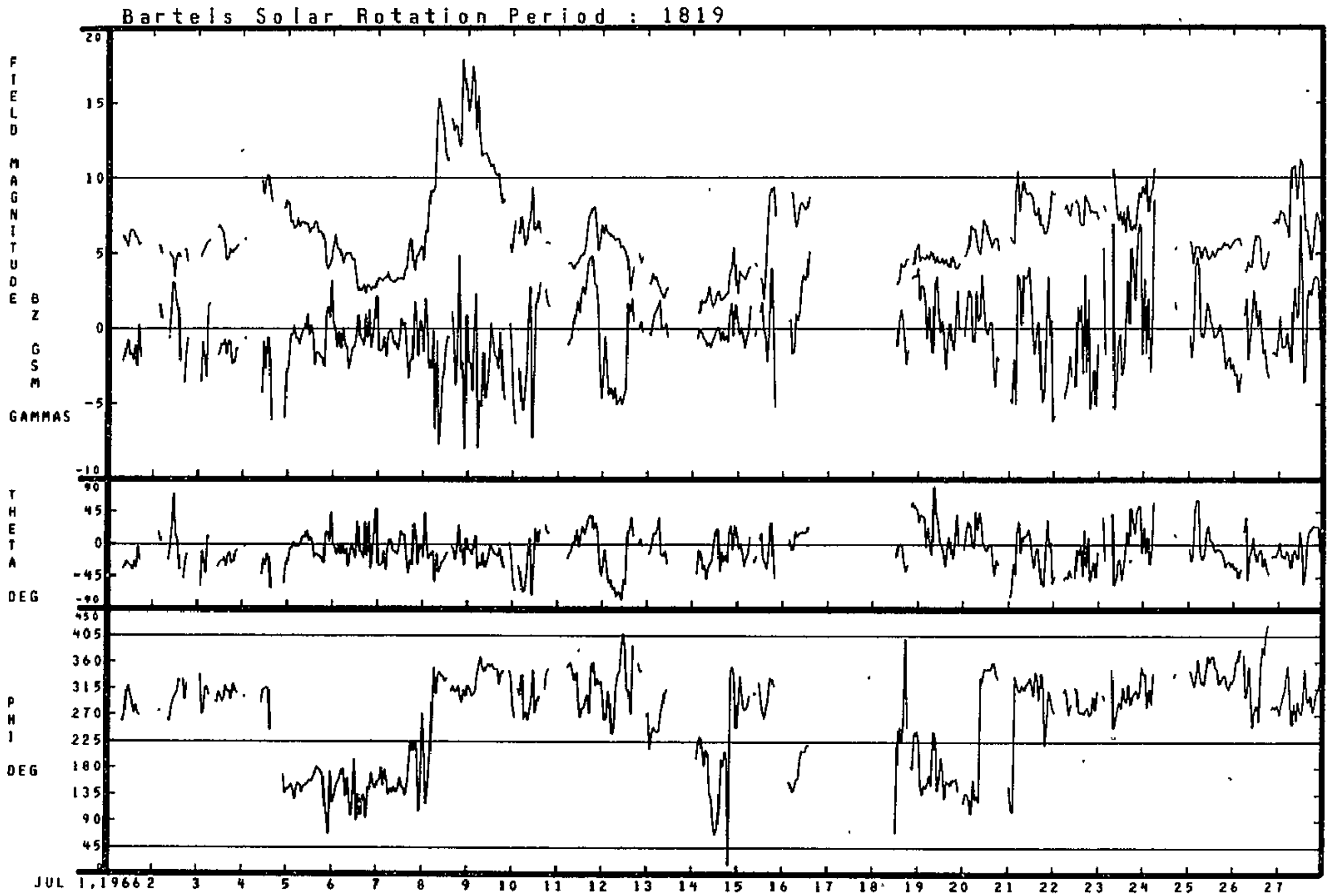
Bartels Solar Rotation Period : 1818



06/04/66 - 06/30/66

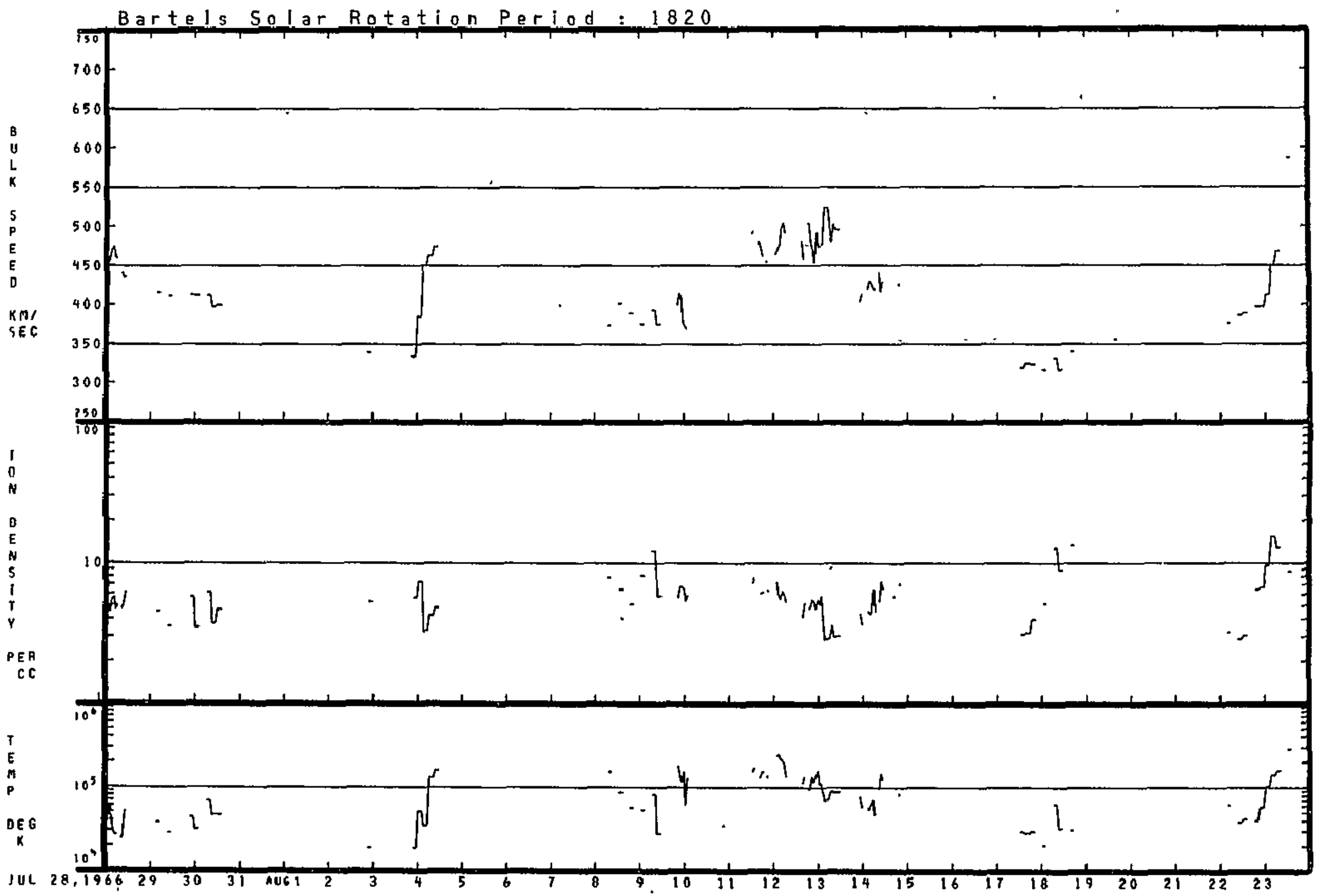
07/01/66 - 07/27/66



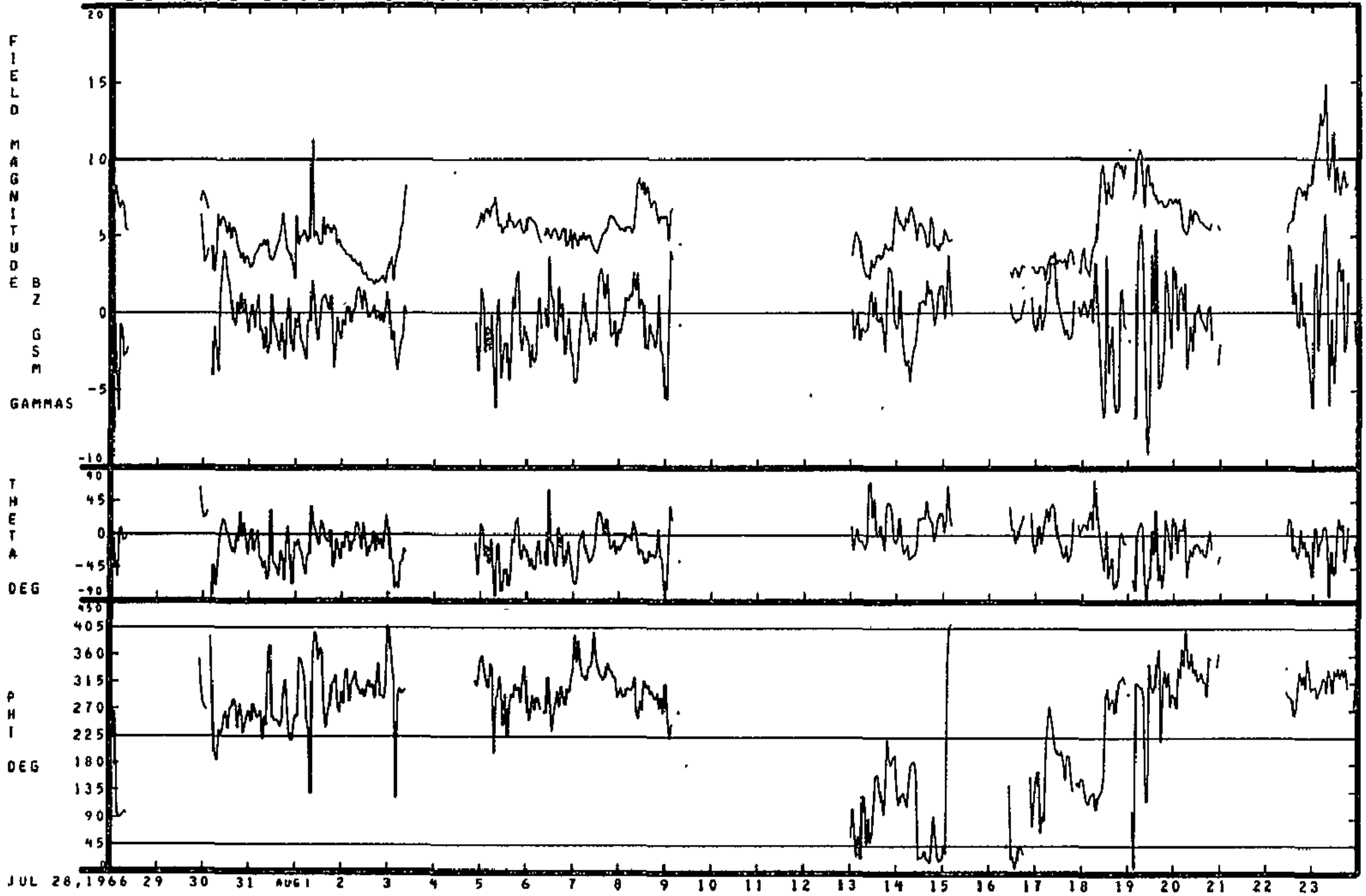


07/01/66 - 07/27/66

07/28/66 - 08/23/66



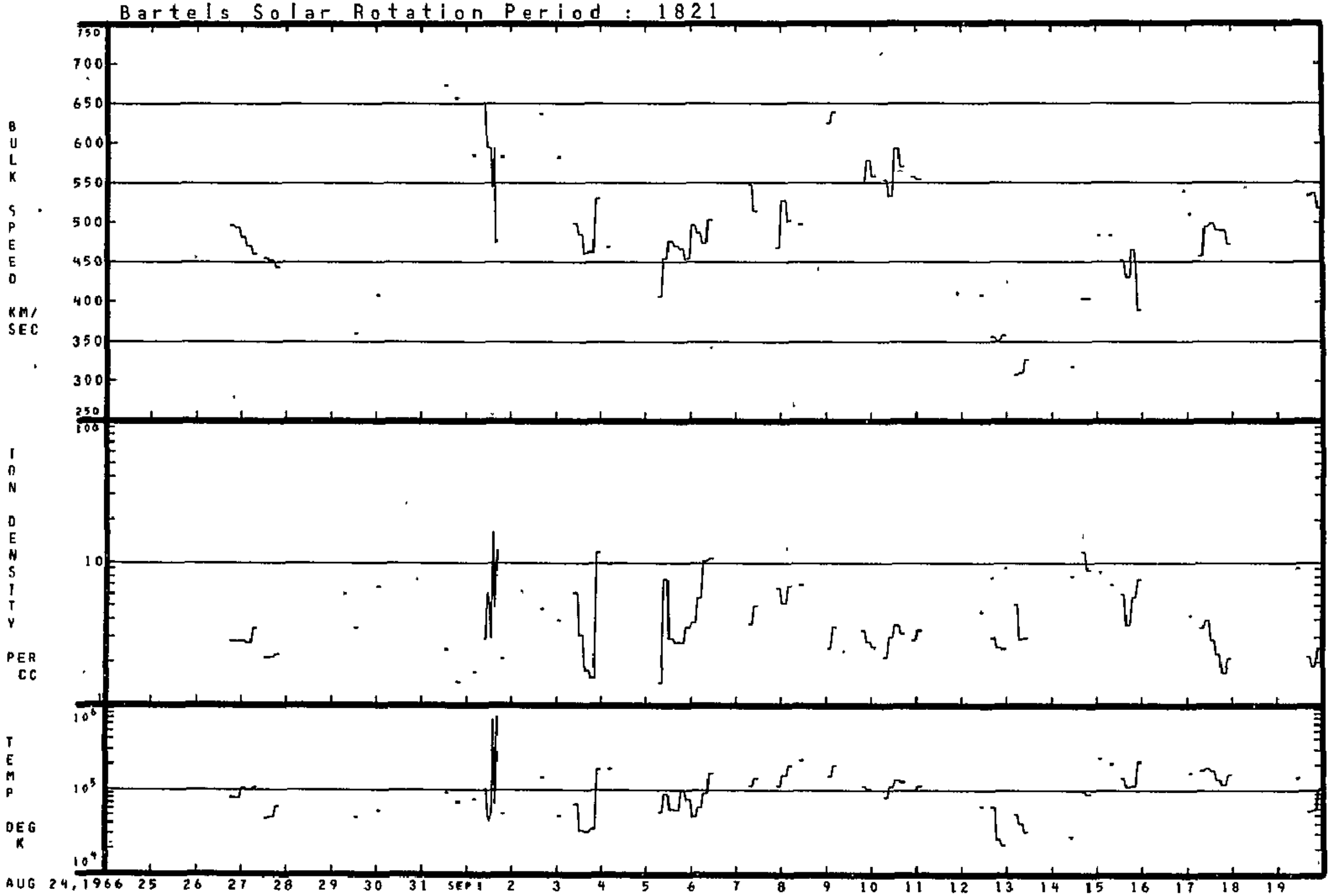
Bartels Solar Rotation Period : 1820

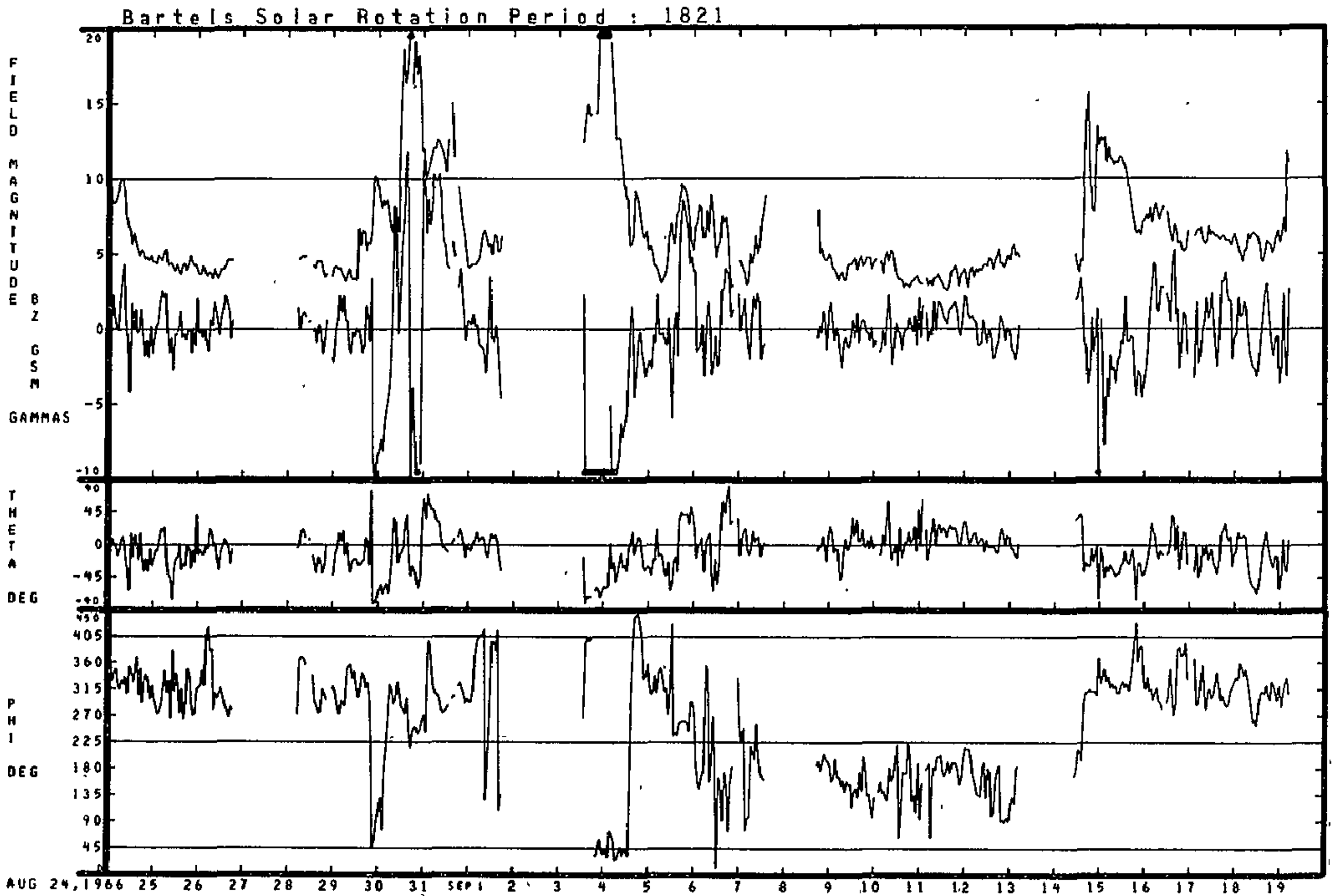


07/28/66 - 08/23/66

08/24/66 - 09/19/66

Bartels Solar Rotation Period : 1821

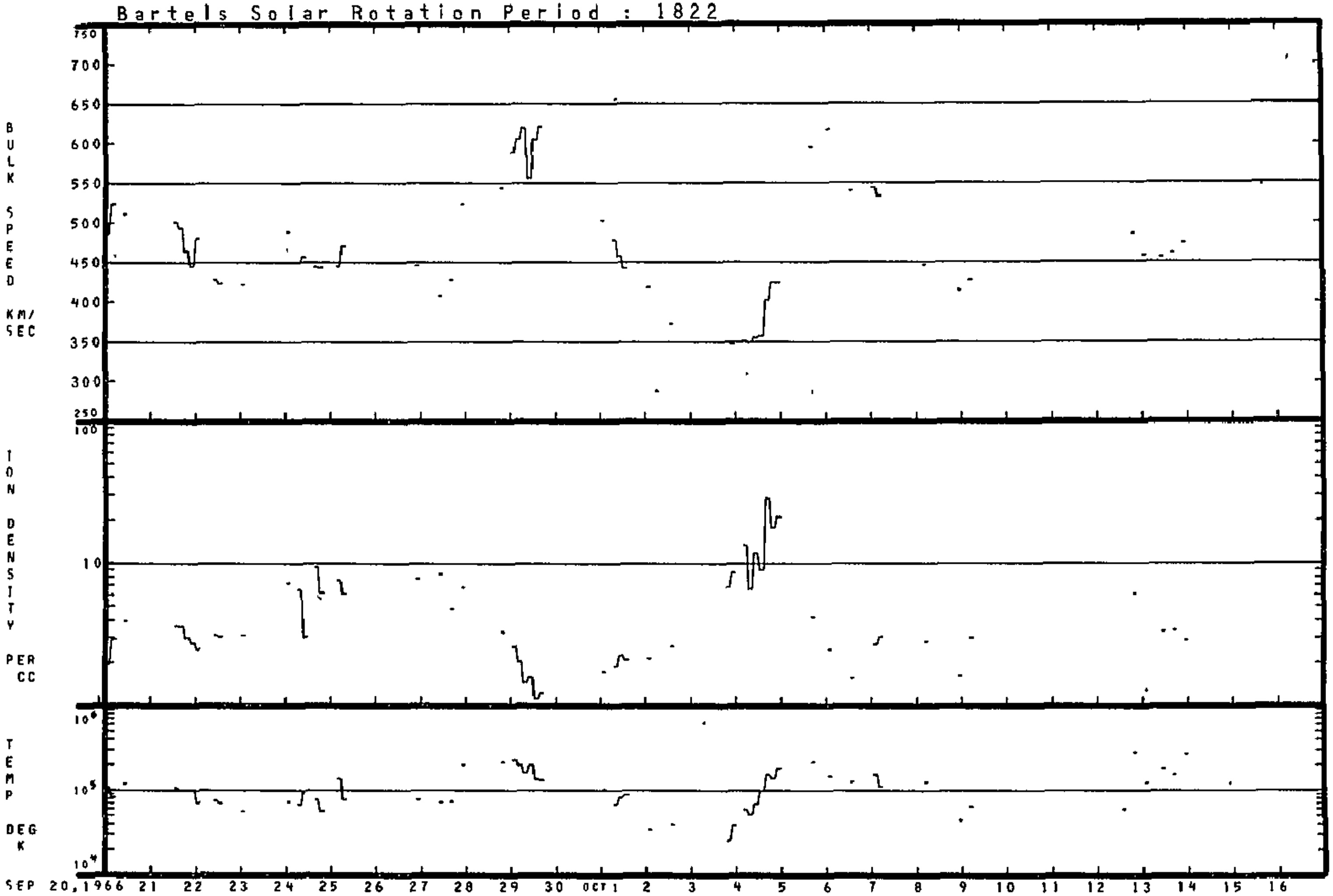




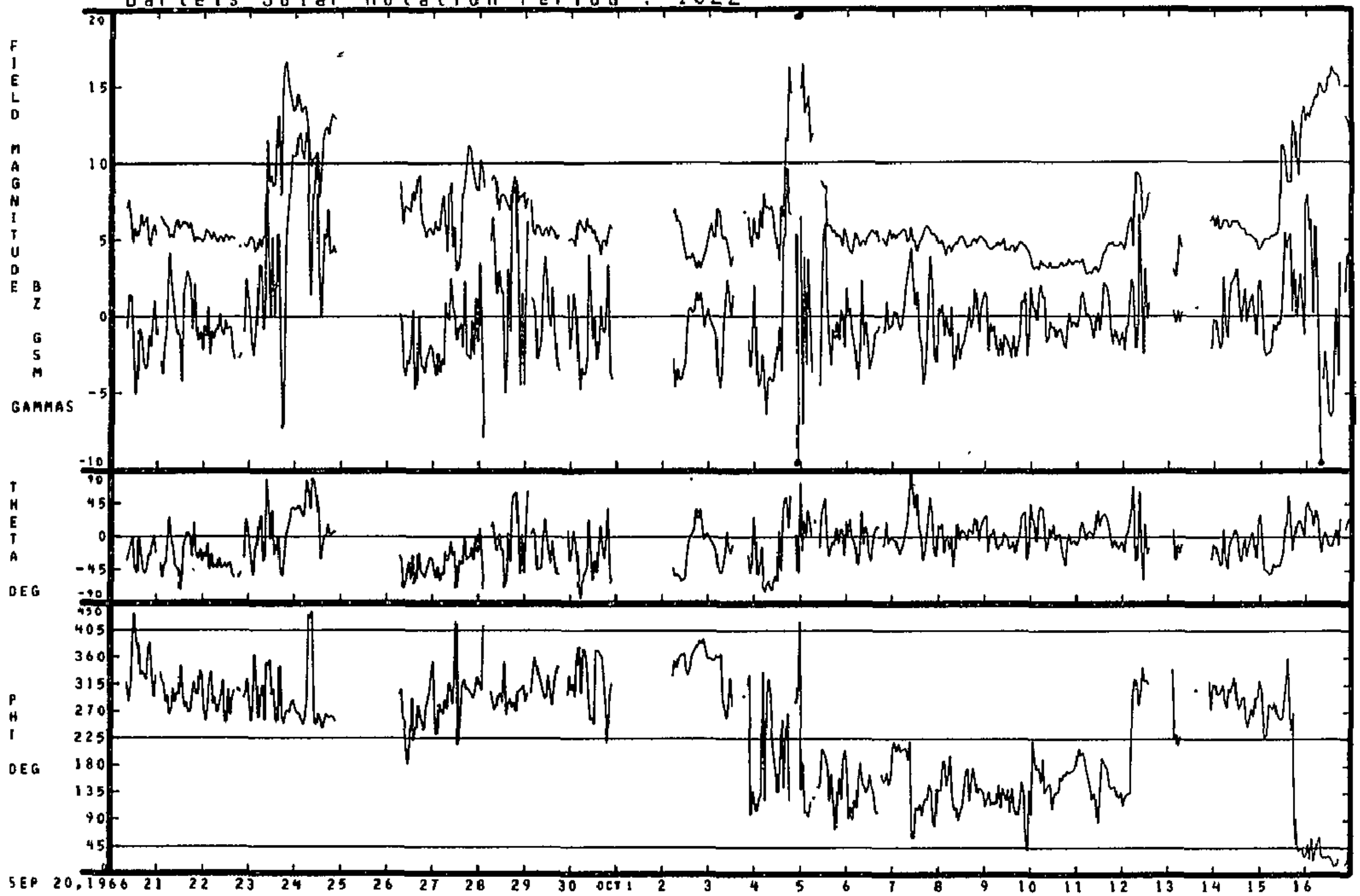
08/24/66 - 09/19/66

09/20/66 - 10/16/66

Bartels Solar Rotation Period : 1822



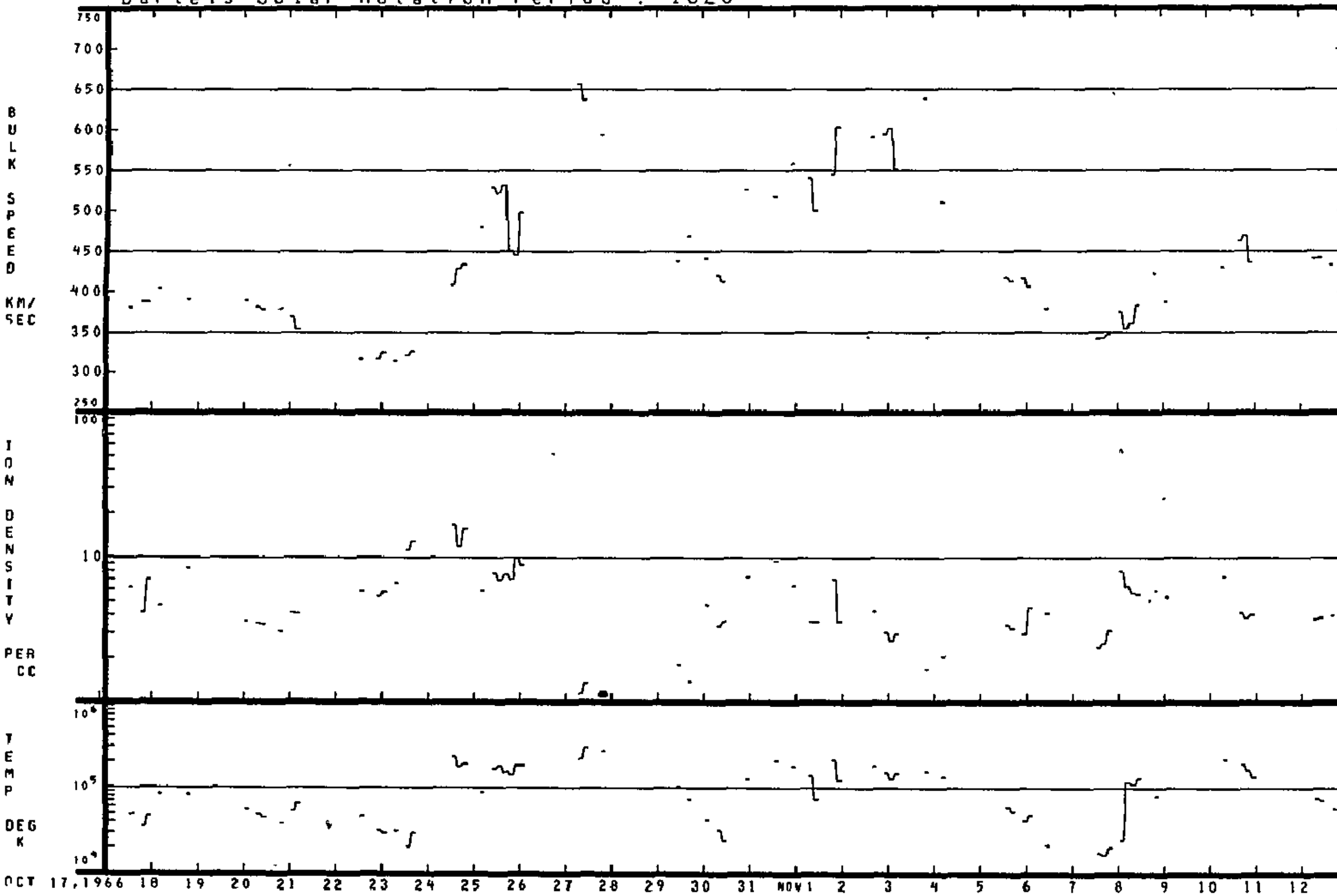
Bartels Solar Rotation Period : 1822

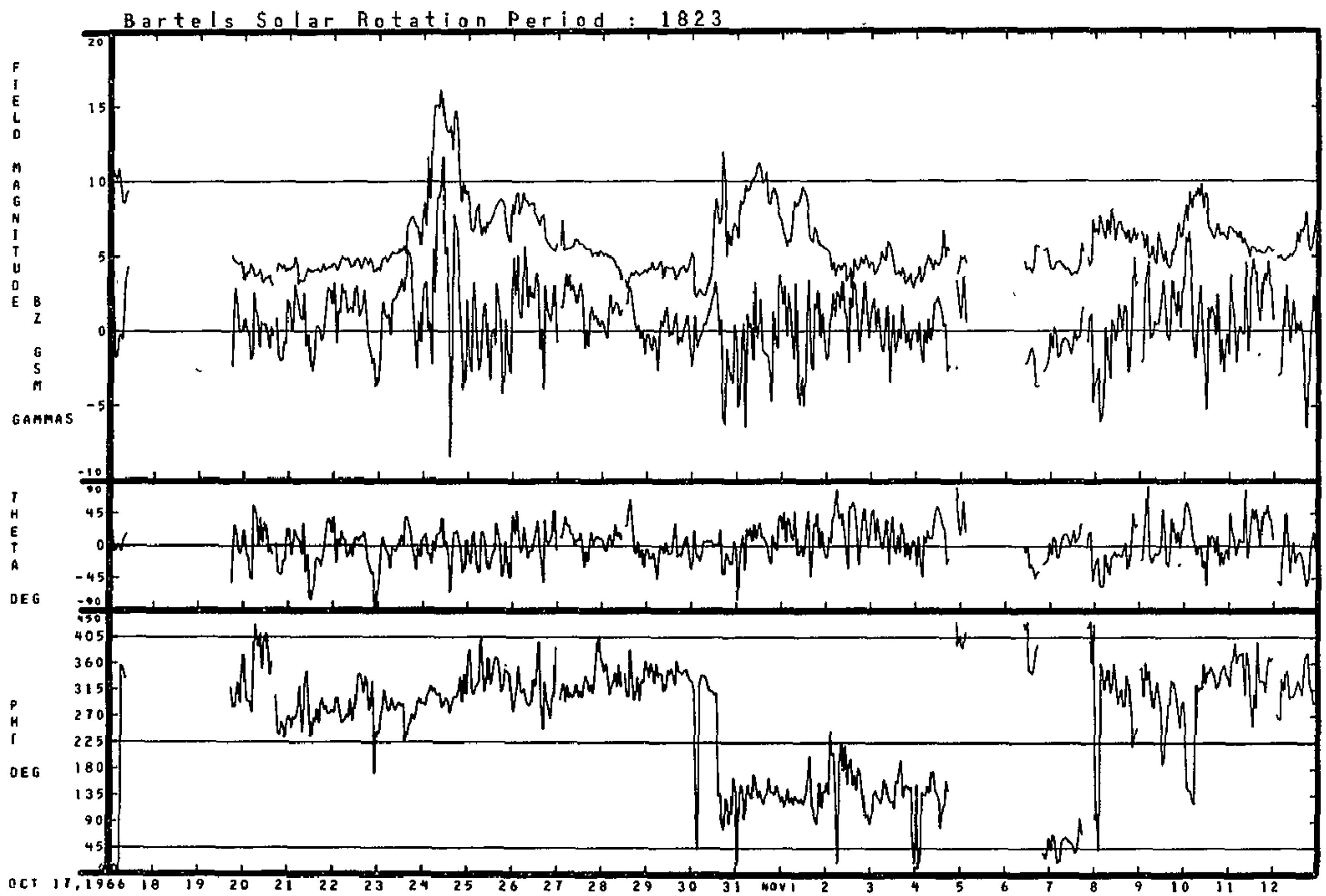


09/20/66 - 10/16/66

10/17/66 - 11/12/66

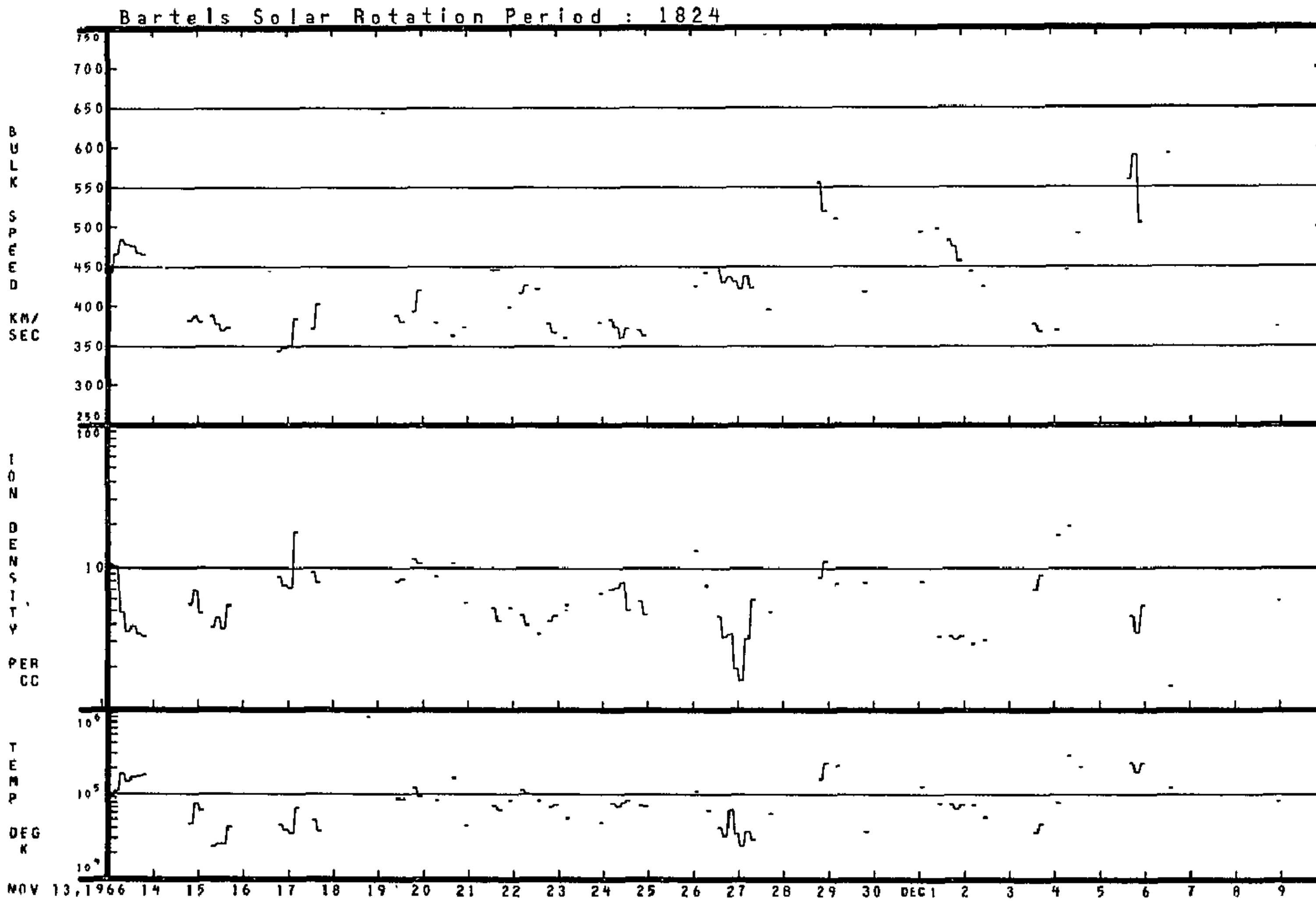
Bartels Solar Rotation Period : 1823

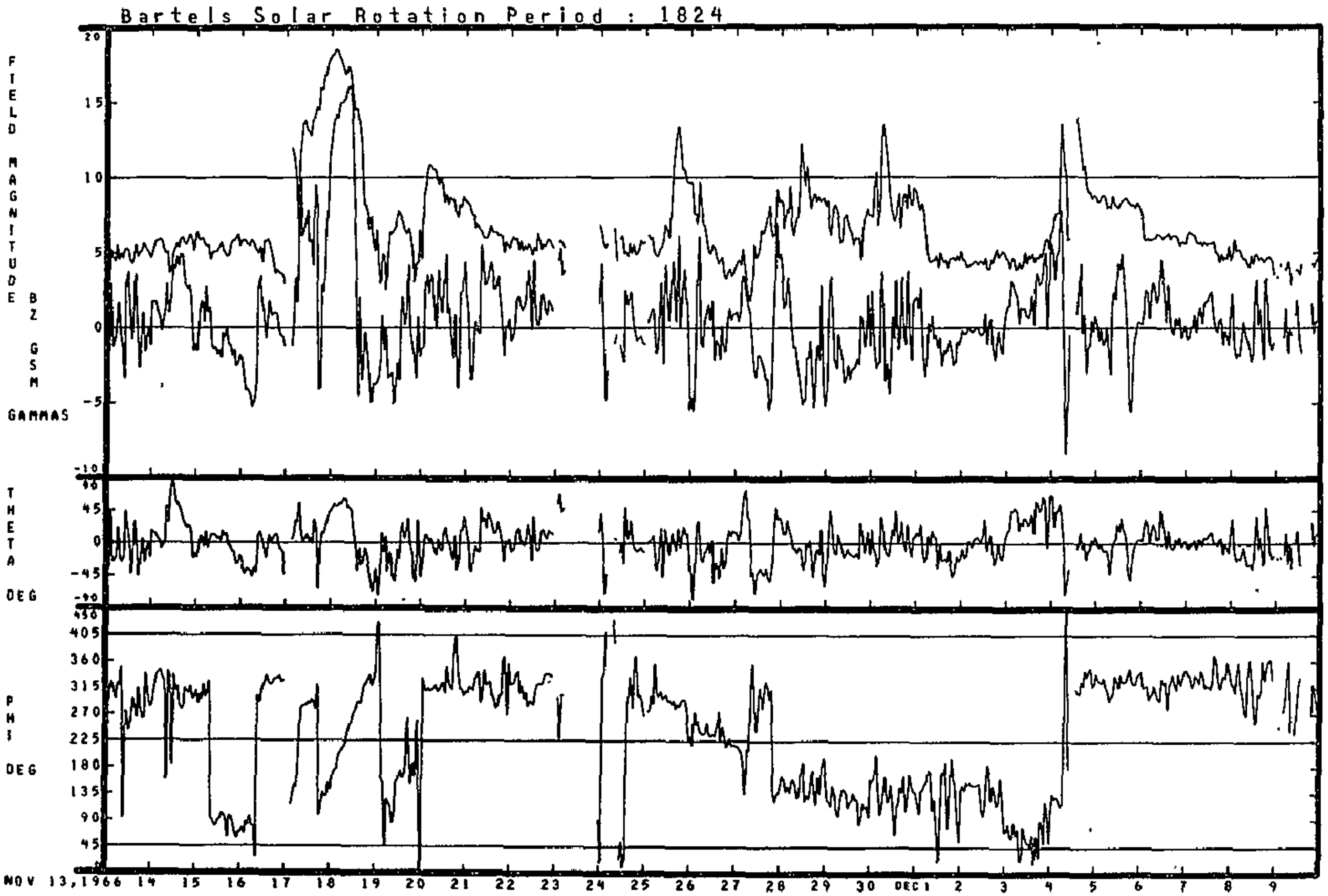




10/17/66 - 11/12/66

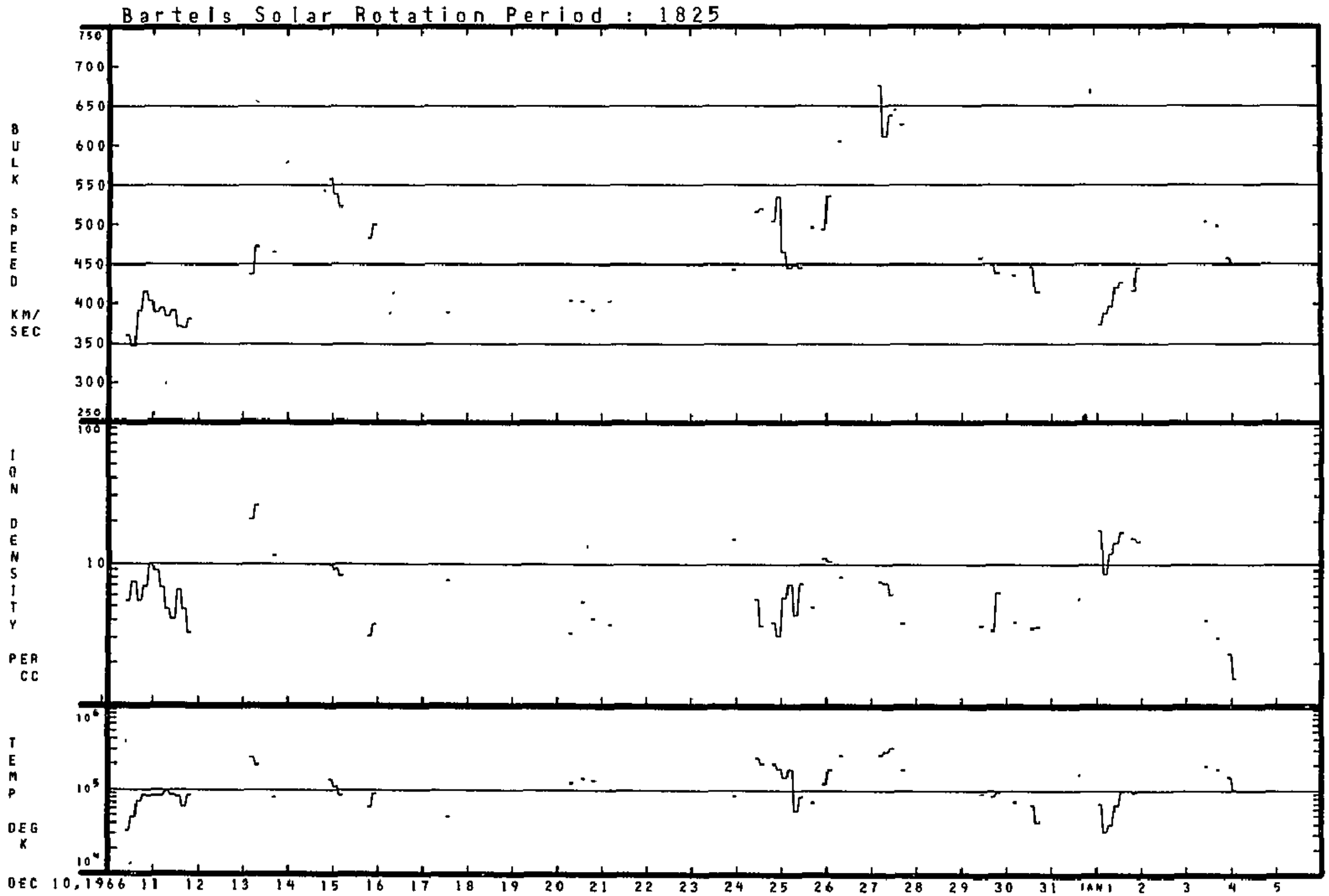
11/13/66 - 12/09/66



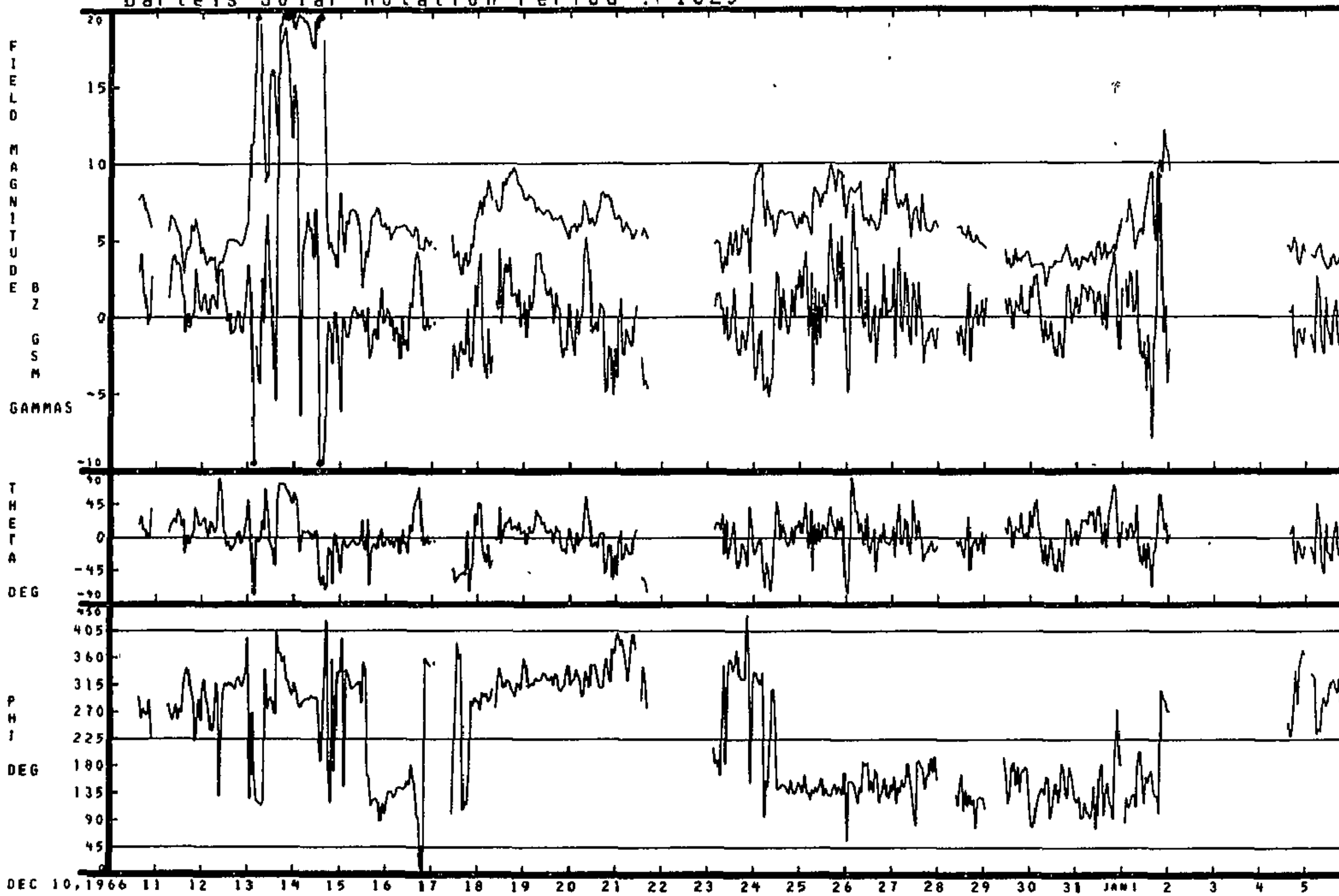


11/13/66 - 12/09/66

12/10/66 - 01/05/67

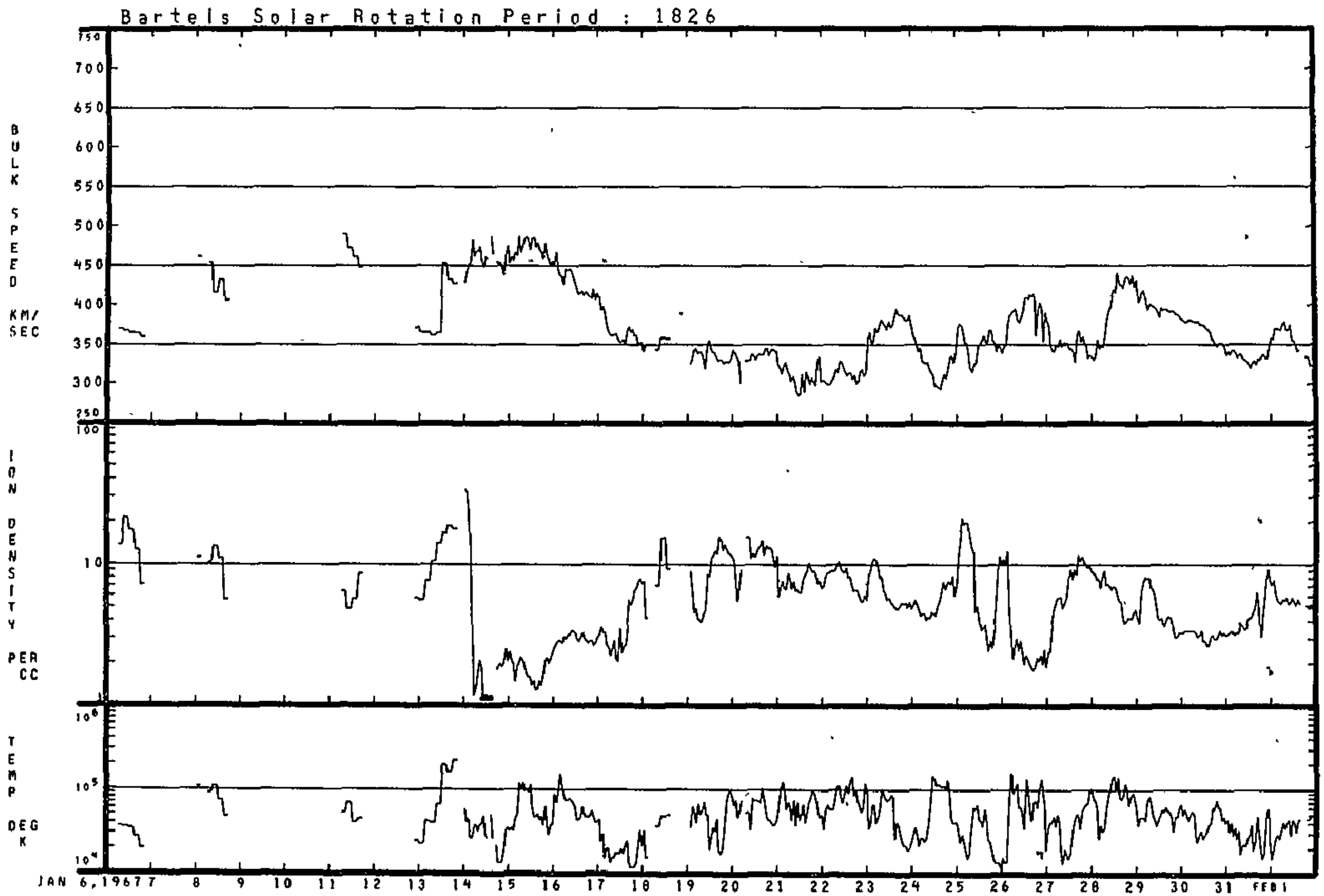


Bartels Solar Rotation Period :\ 1825

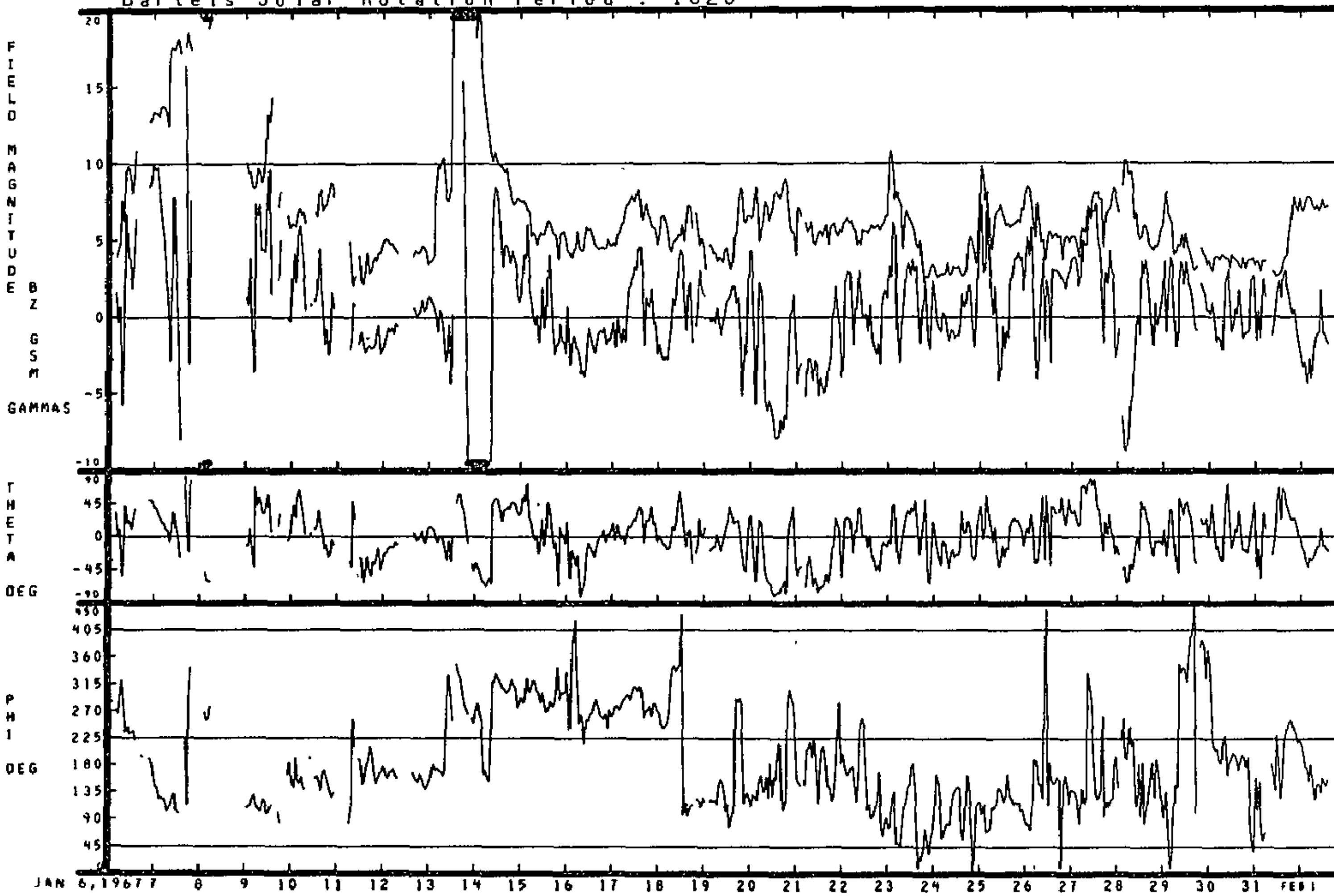


12/10/66 - 01/05/67

01/06/67 - 02/01/67

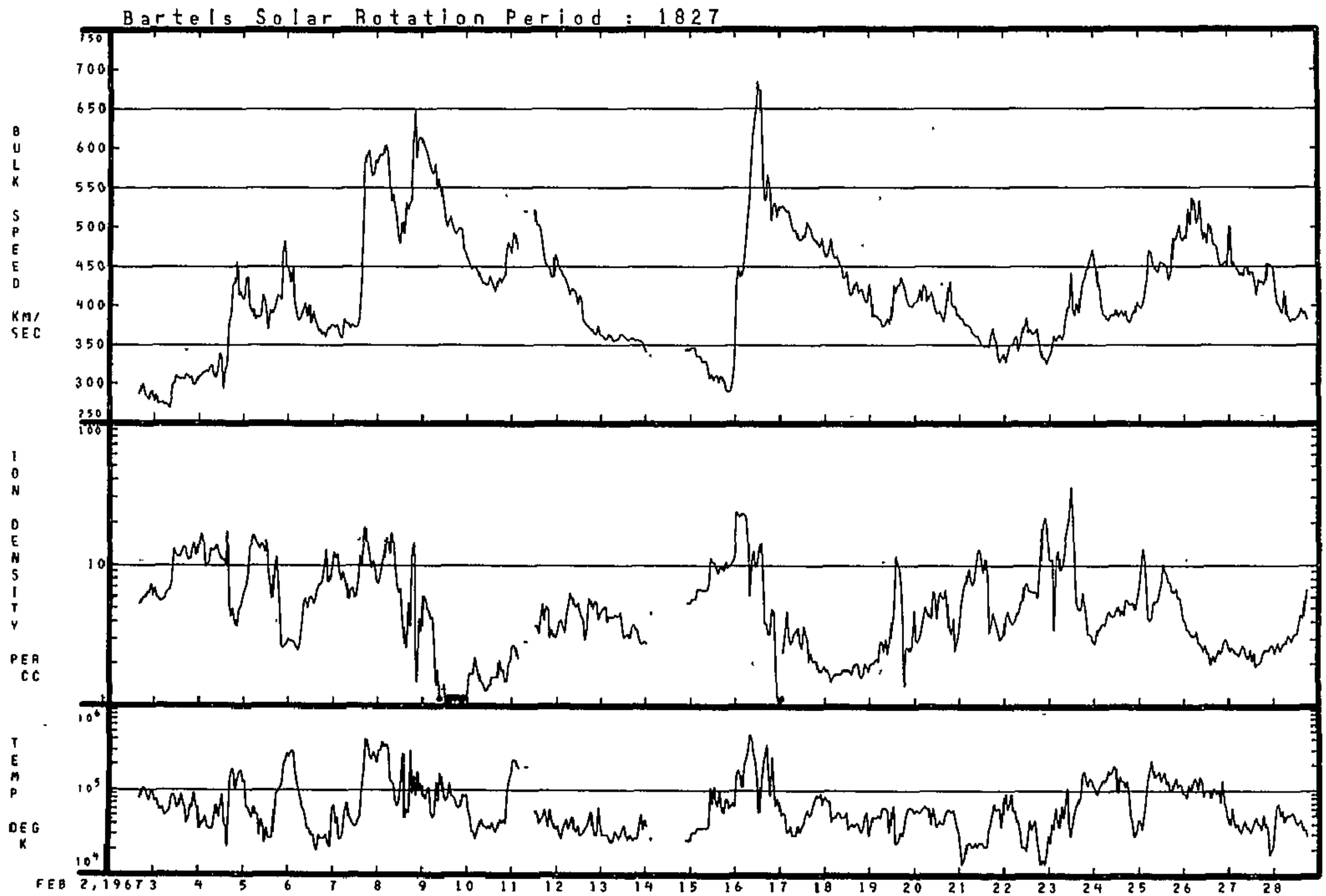


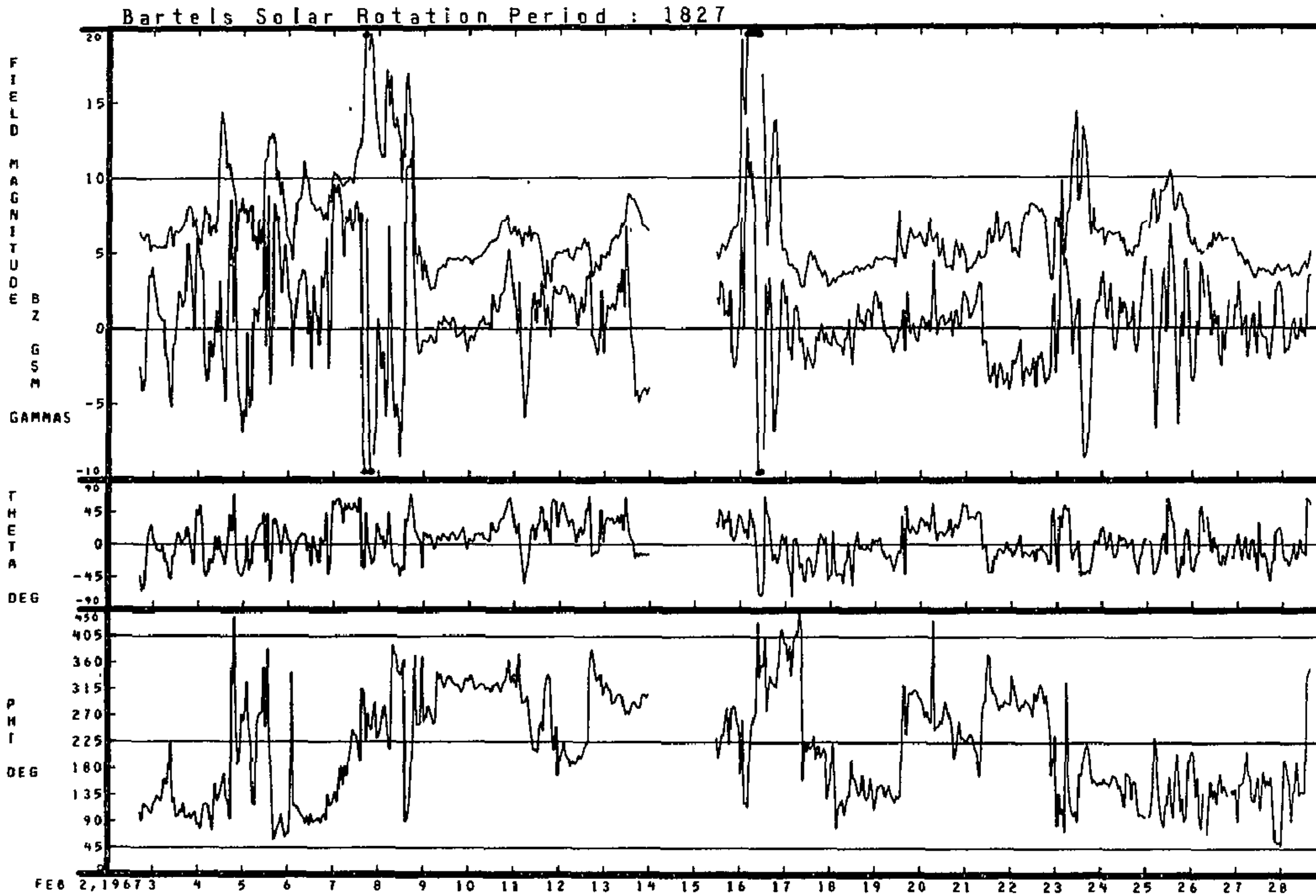
Bartels Solar Rotation Period : 1826



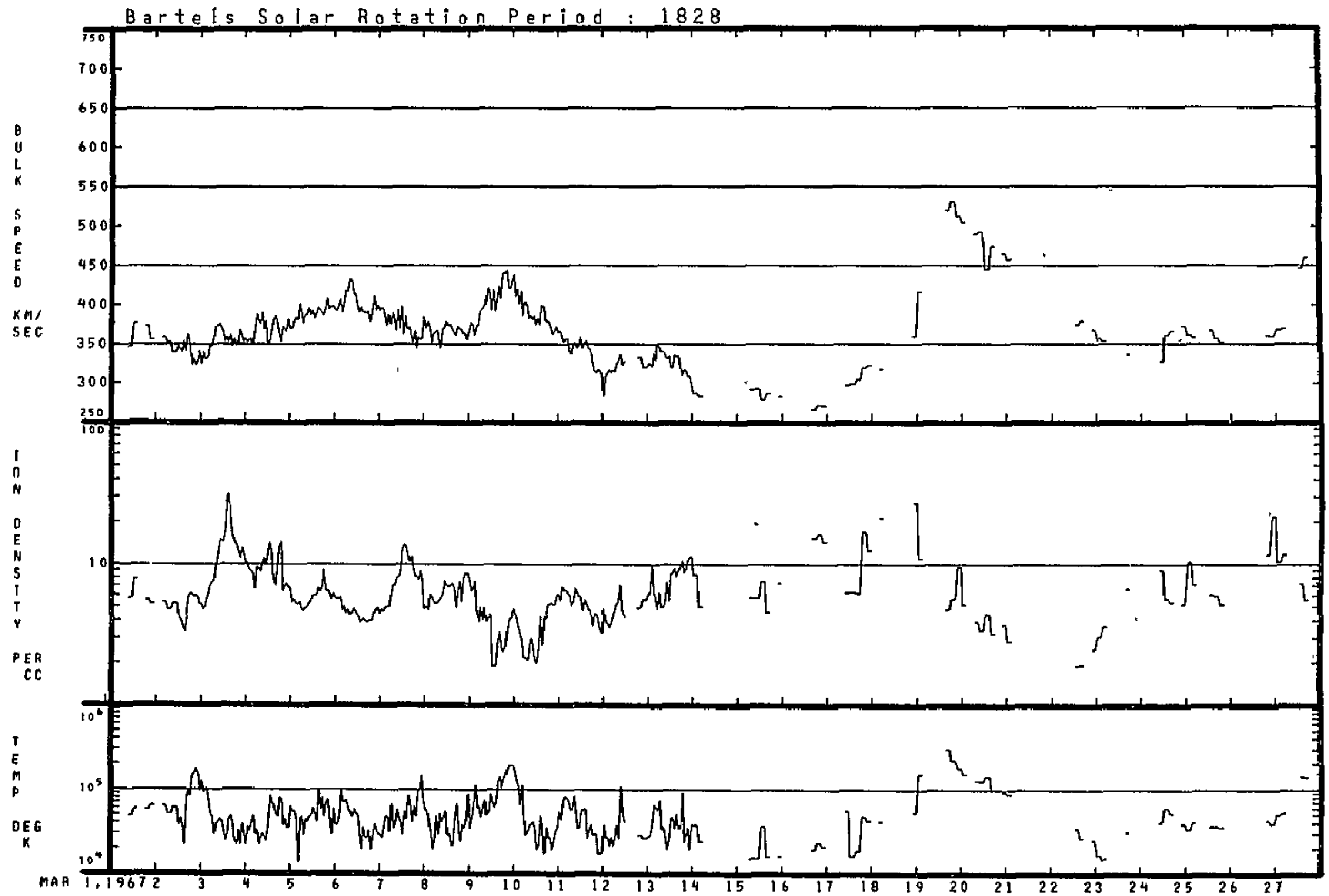
01/06/67 - 02/01/67

02/02/67 - 02/28/67

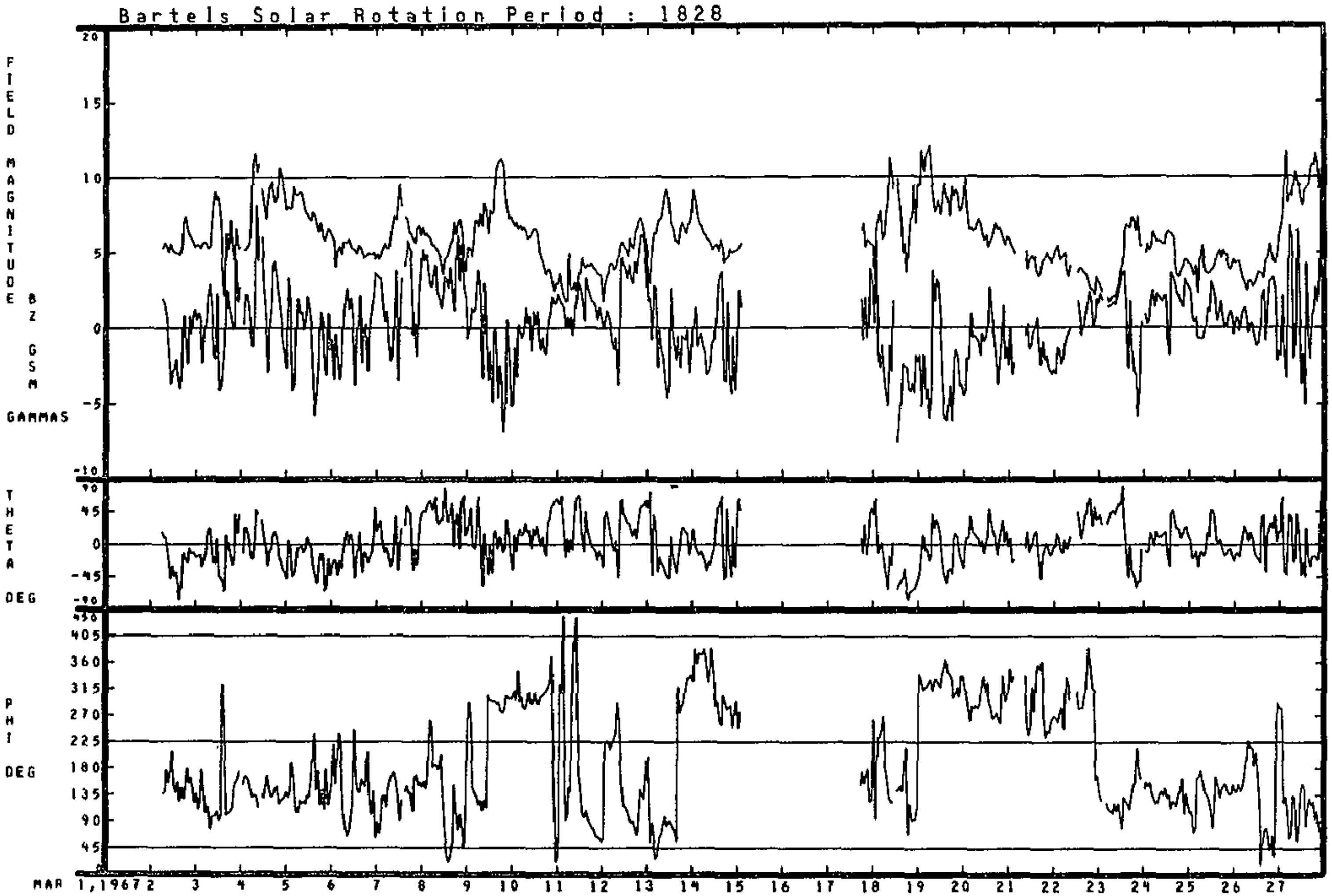




02/02/67 - 02/28/67



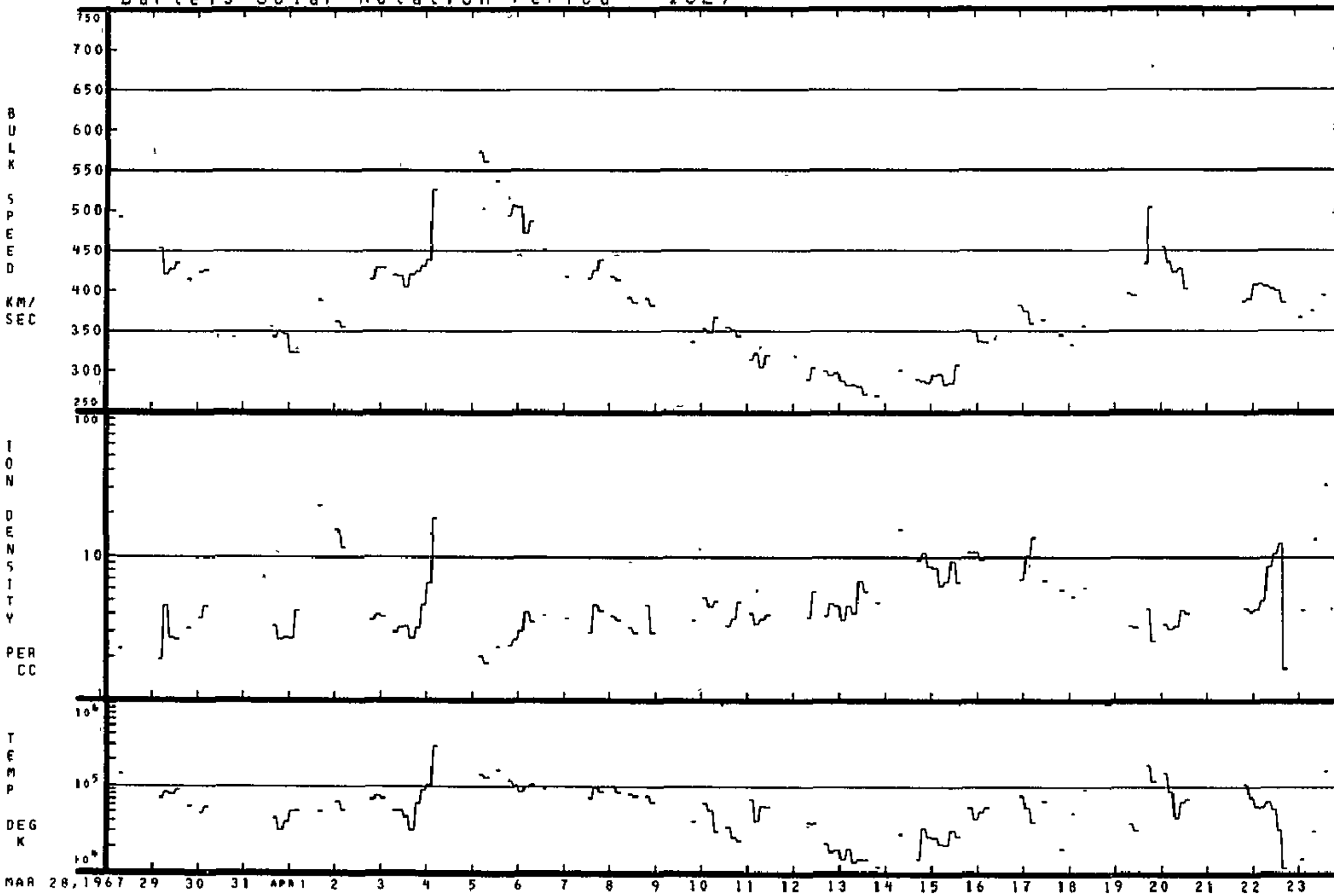
03/01/67 - 03/27/67

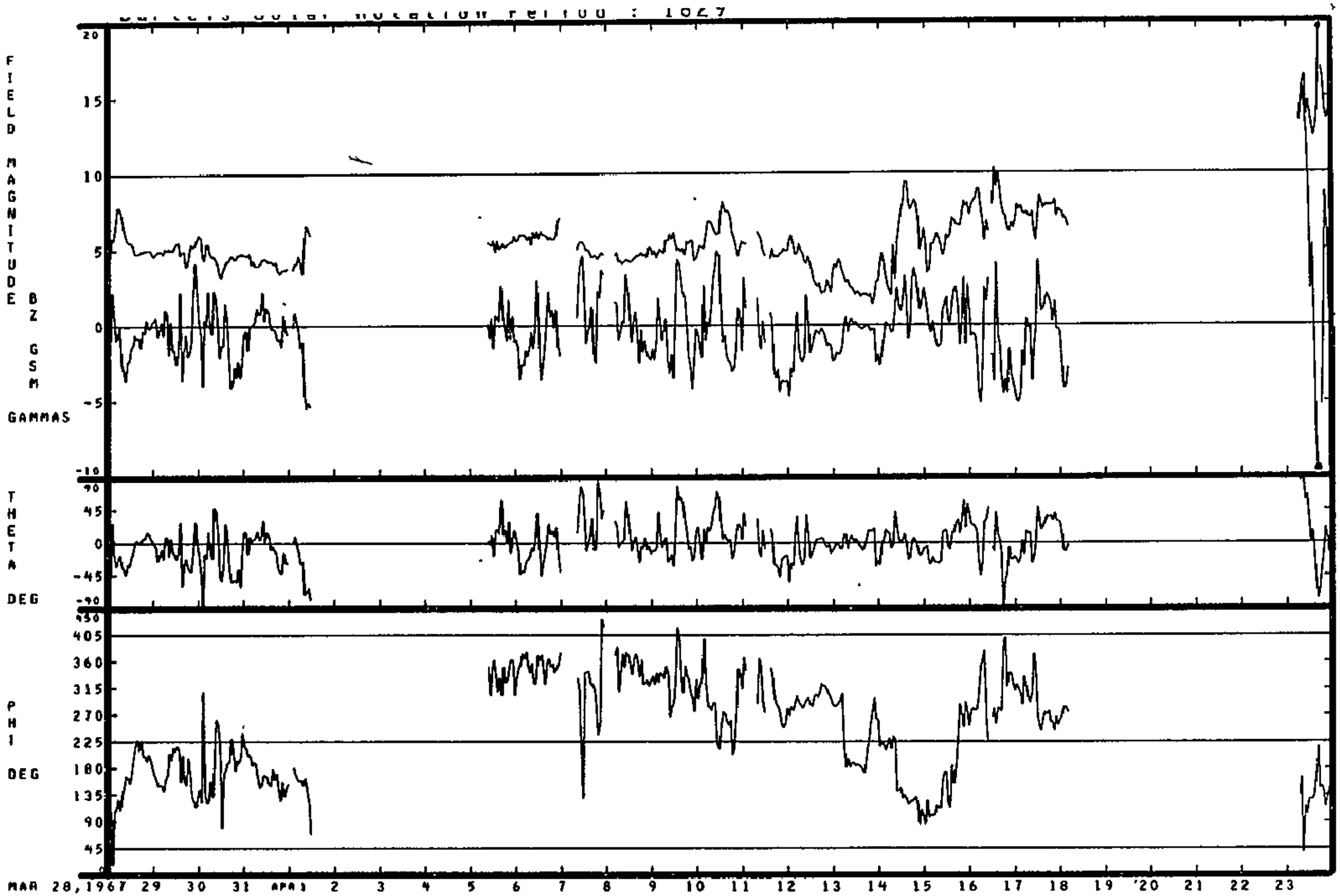


03/01/67 - 03/27/67

03/28/67 - 04/23/67

Bartels Solar Rotation Period · 1829





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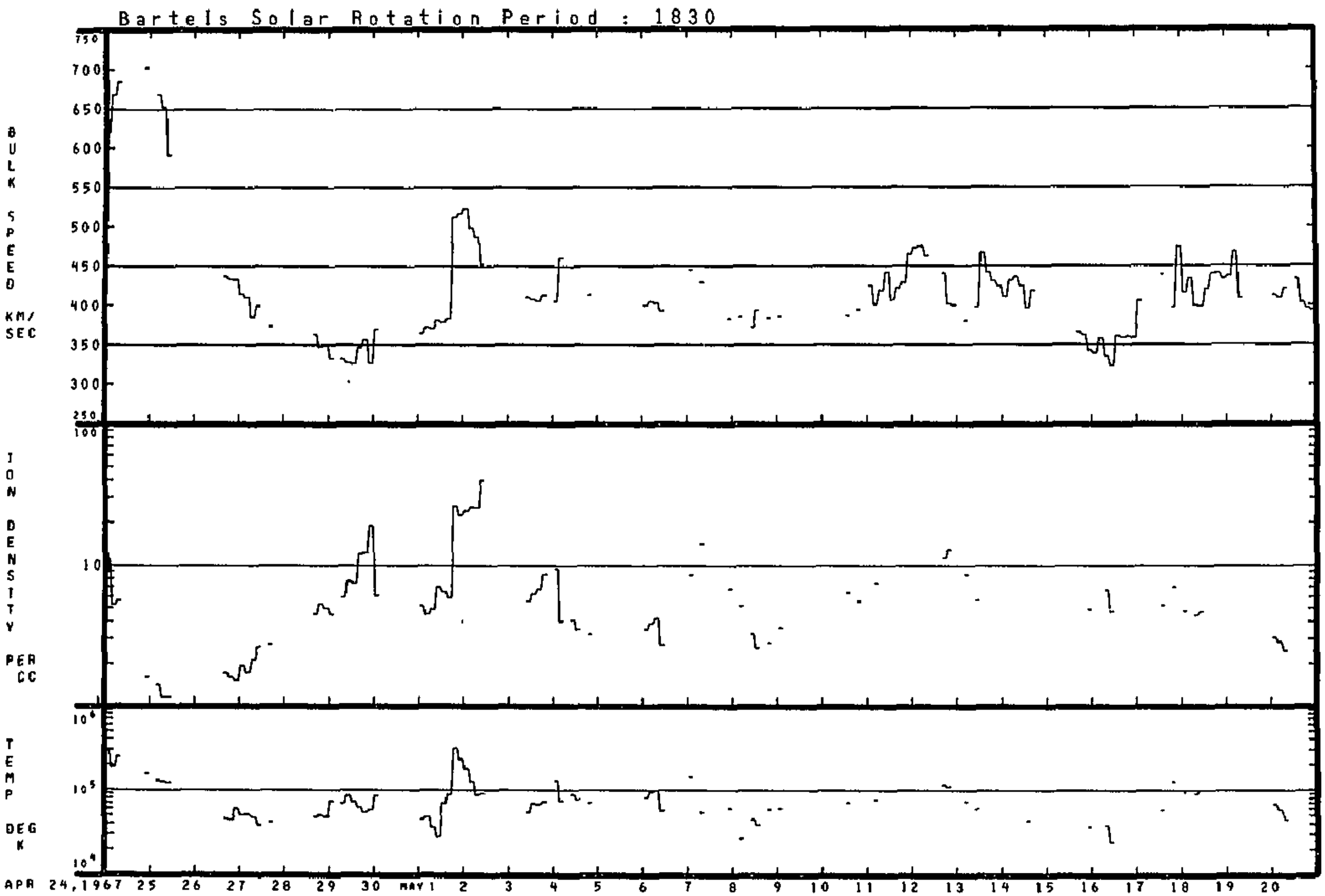
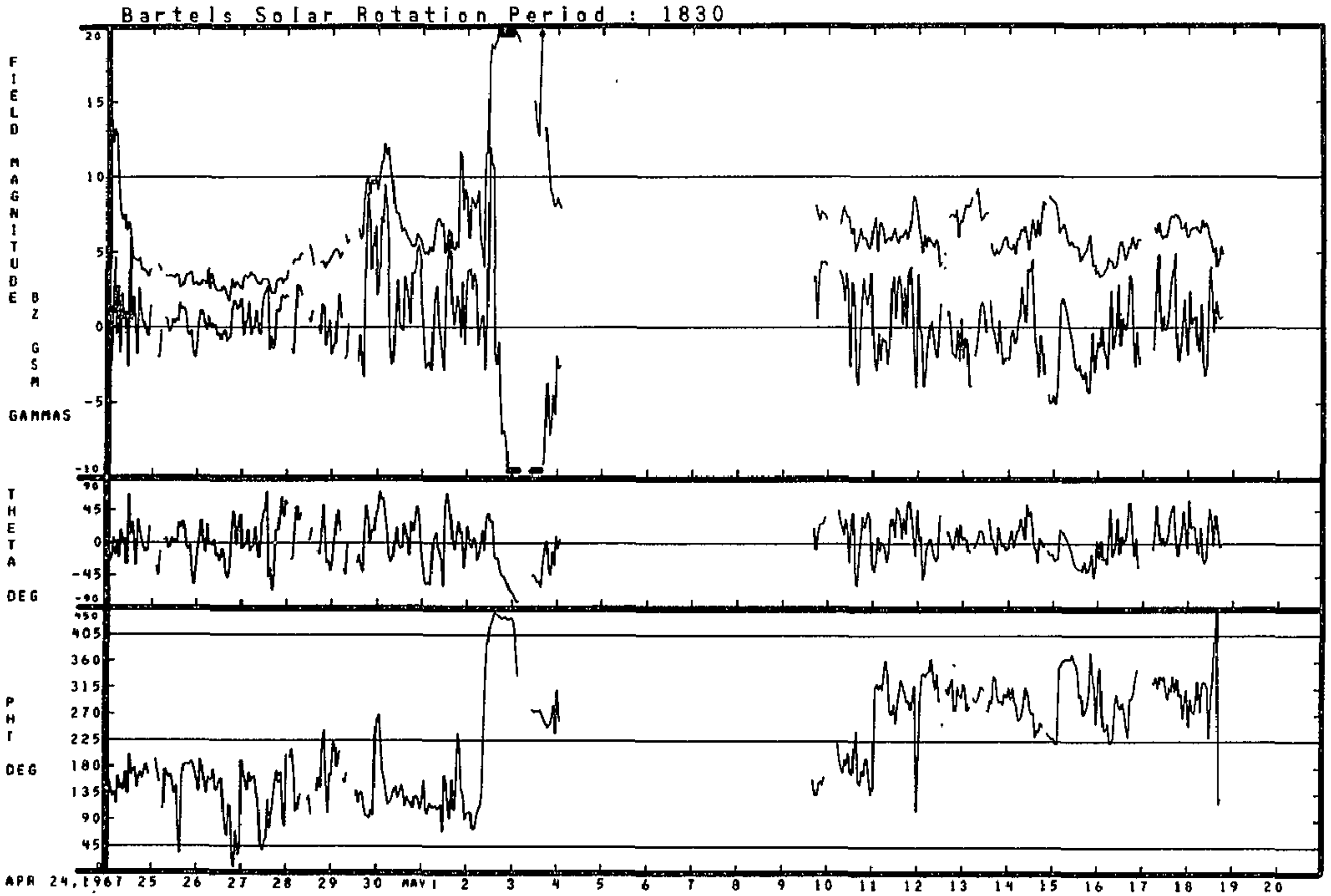
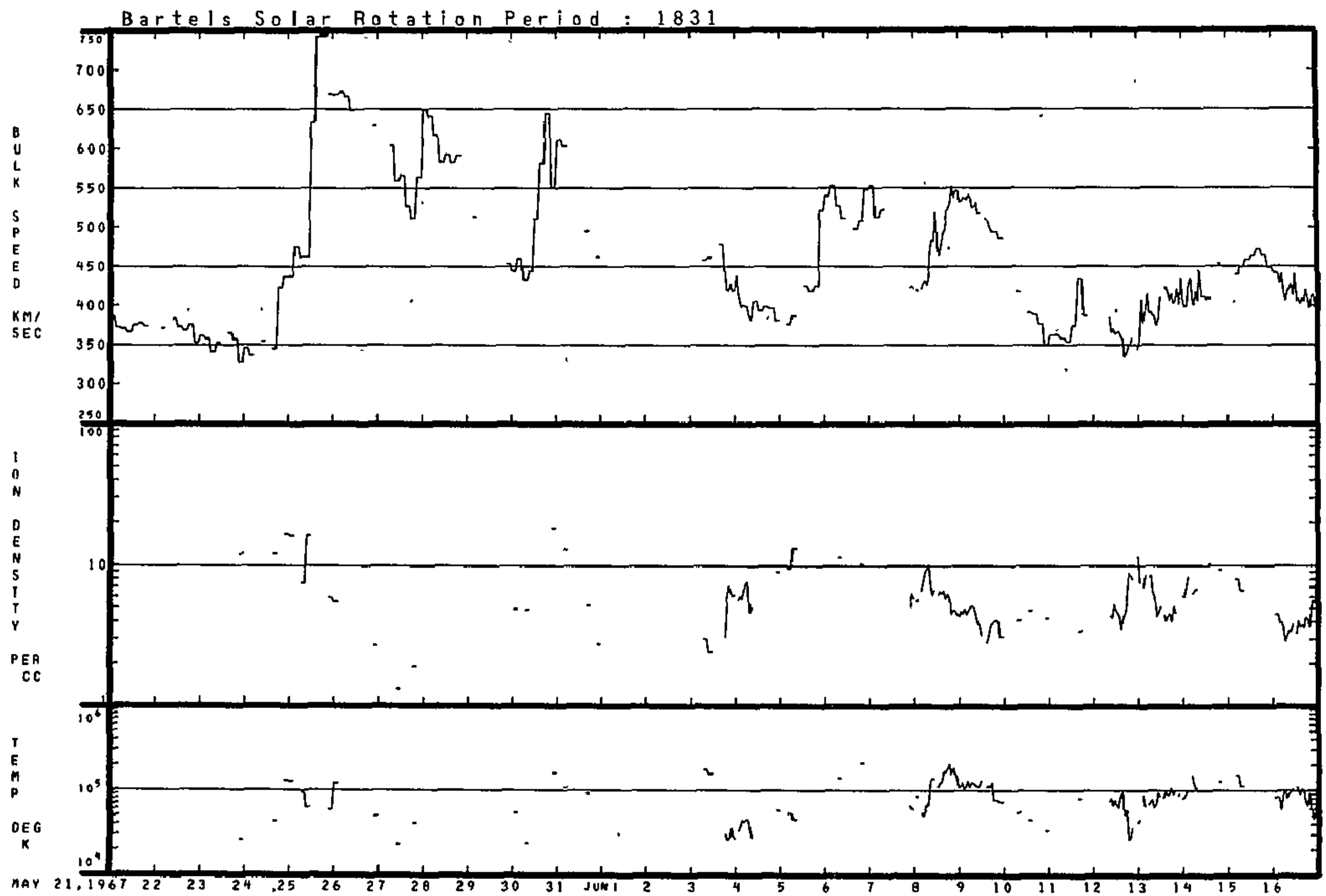
03/28/67 - 04/23/67

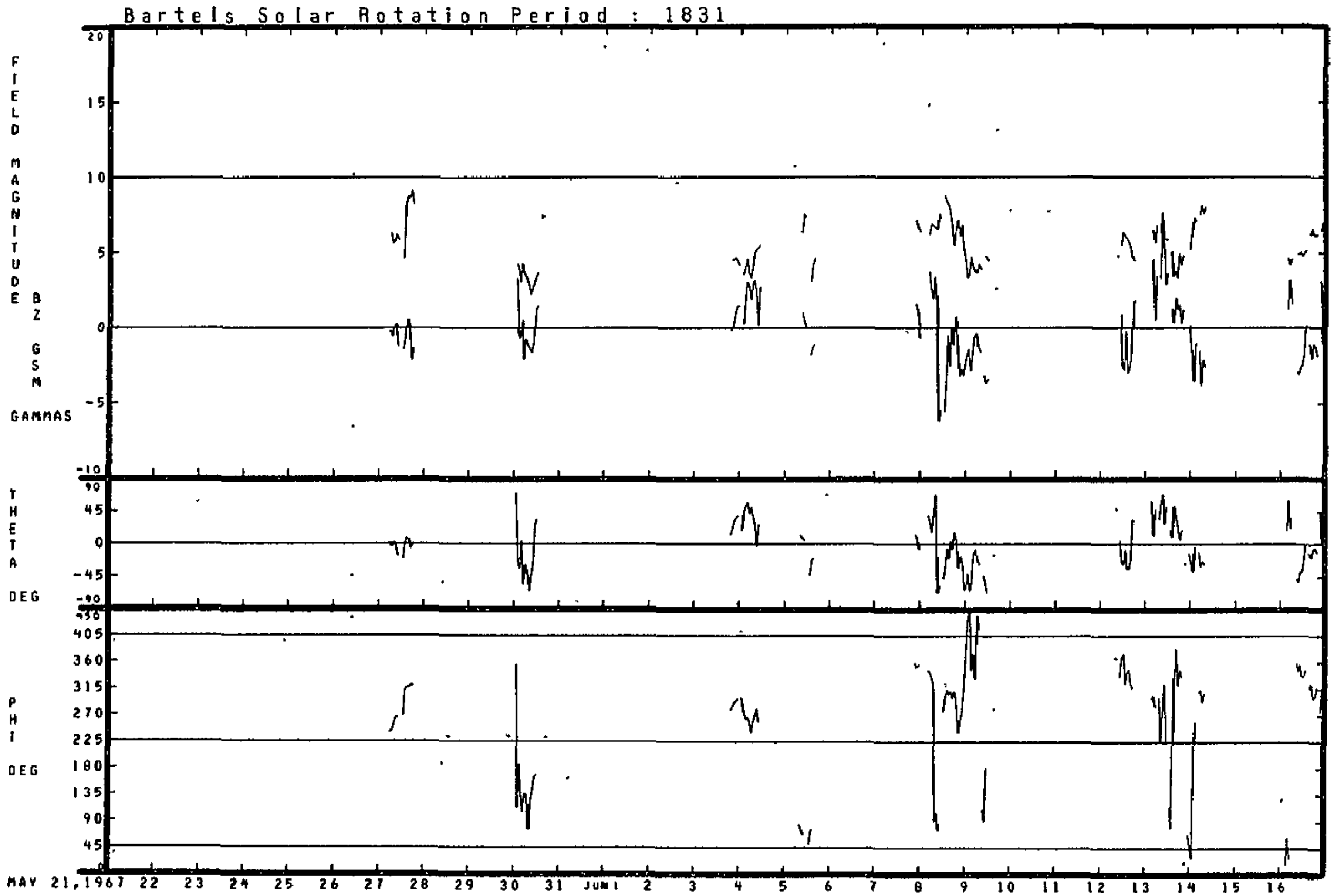
04/24/67 - 05/20/67





04/24/67 - 05/20/67

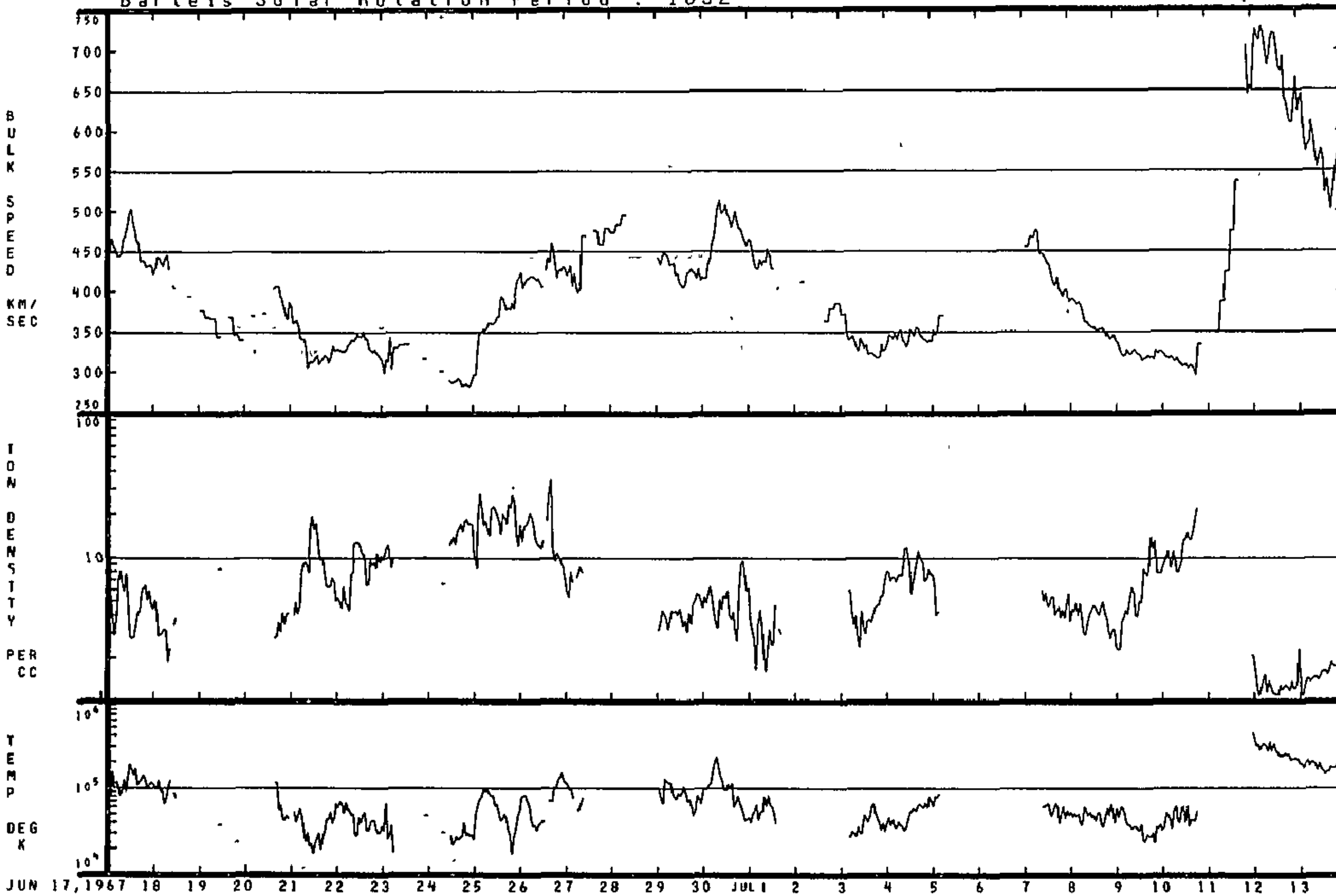

Bartels Solar Rotation Period : 1831
BULK SPEED KM/SEC
750
700
650
600
550
500
450
400
350
300
250
ION DENSITY PER CC
100
10
TEMP DEG K
10^6
10^5
10^4
MAY 21,1967 22 23 24 25 26 27 28 29 30 31 JUN 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16



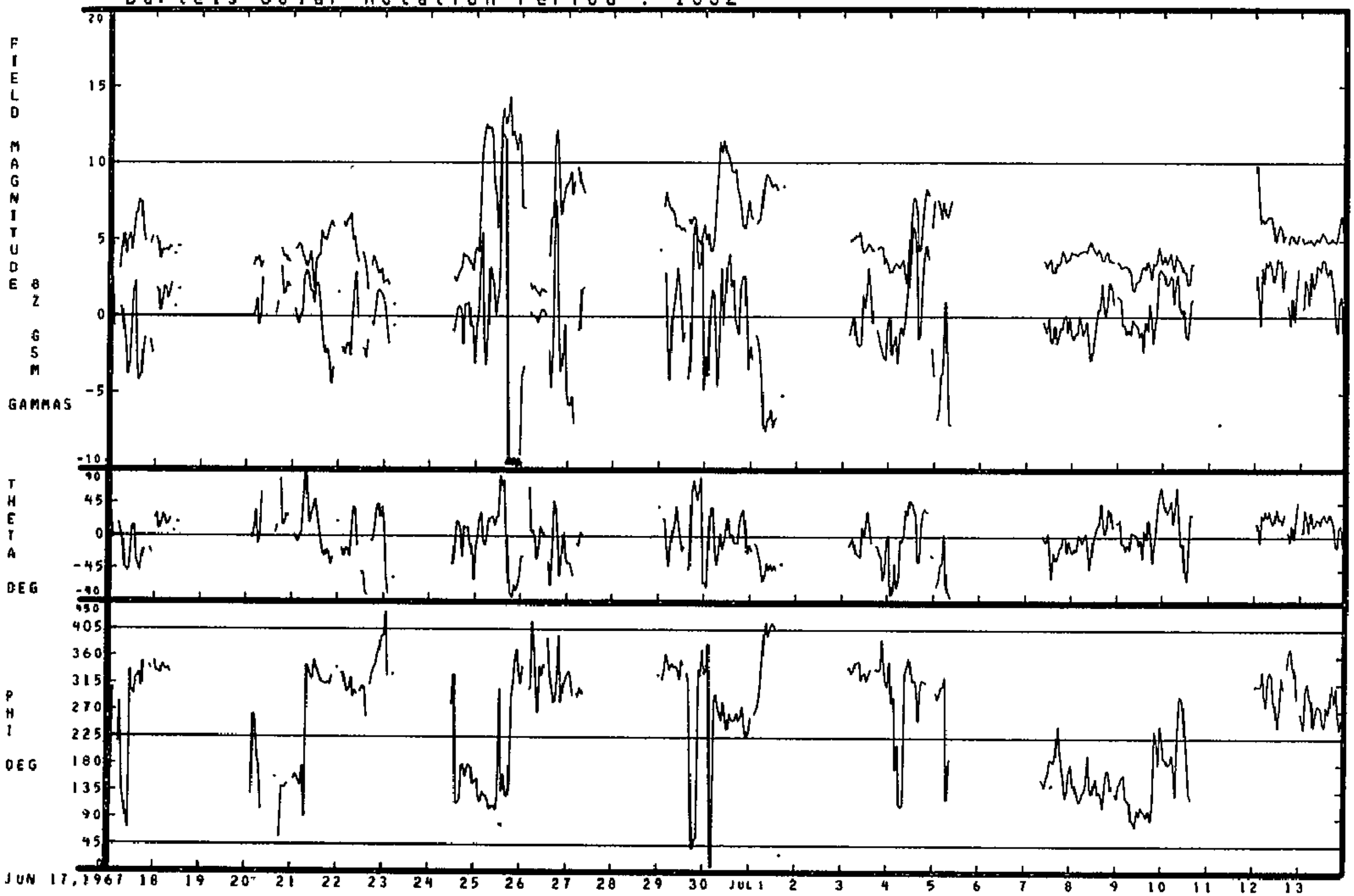
05/21/67 - 06/16/67

06/17/67 - 07/13/67

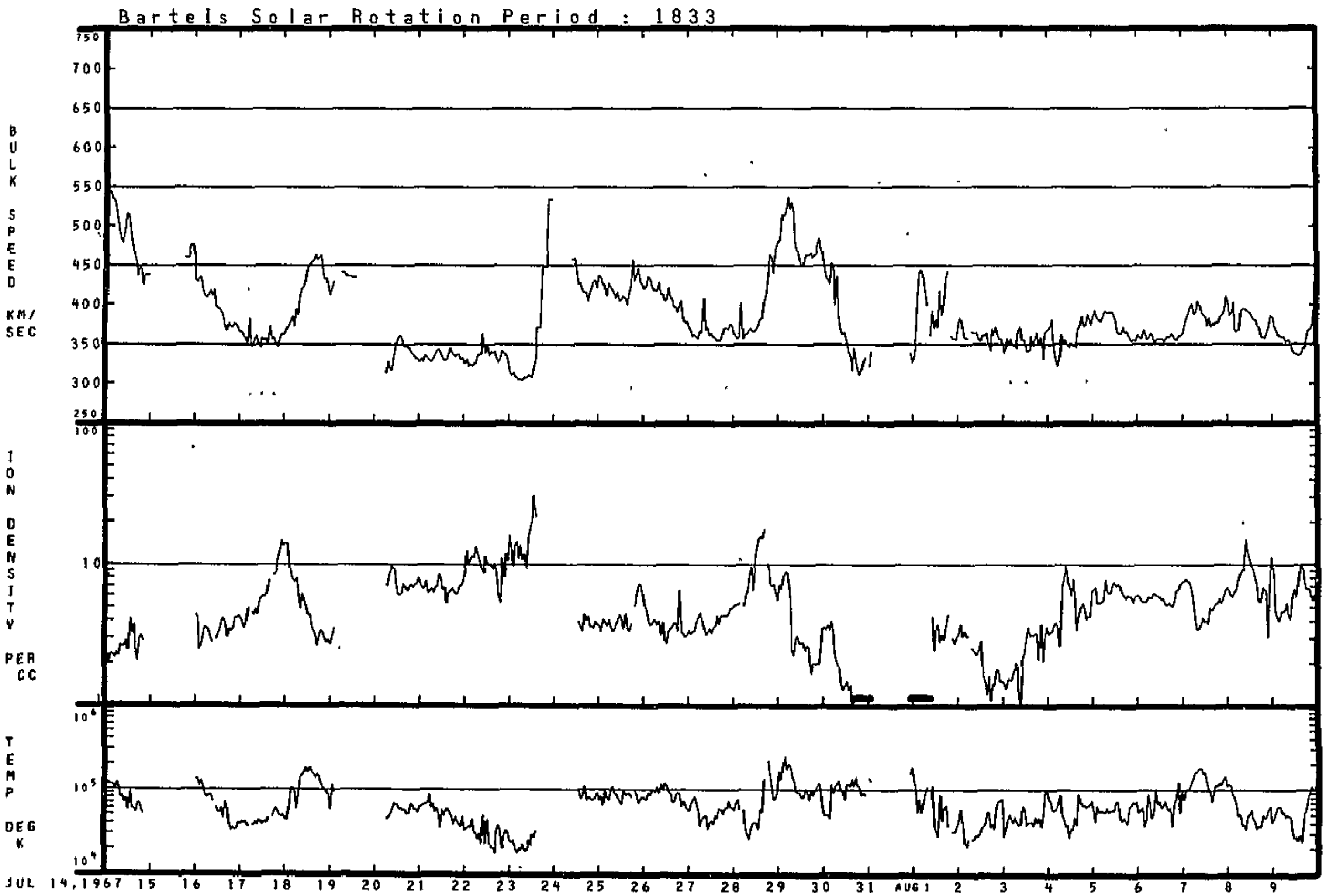
Bartels Solar Rotation Period : 1832



Bartels Solar Rotation Period : 1832

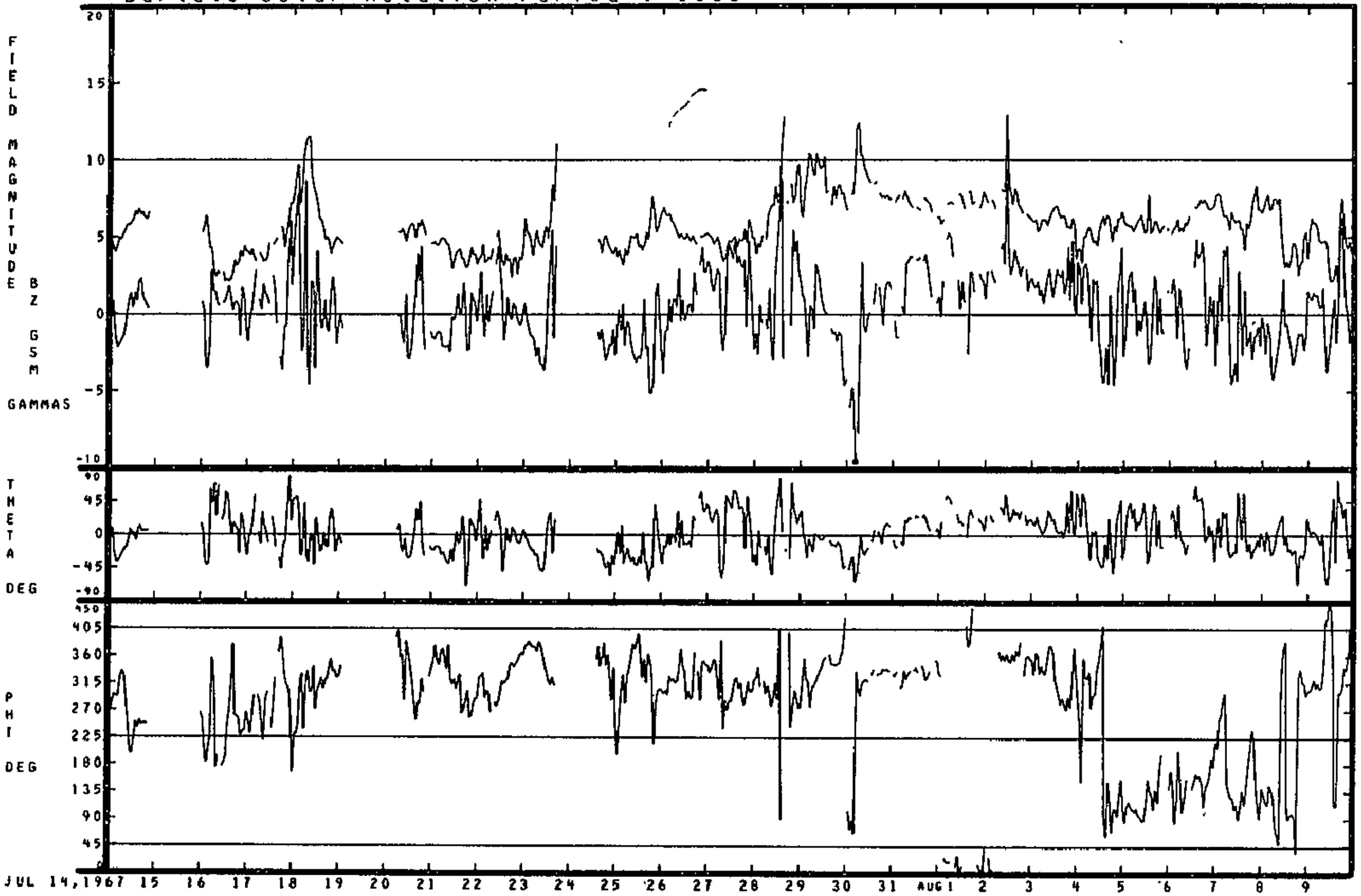


06/17/67 - 07/13/67



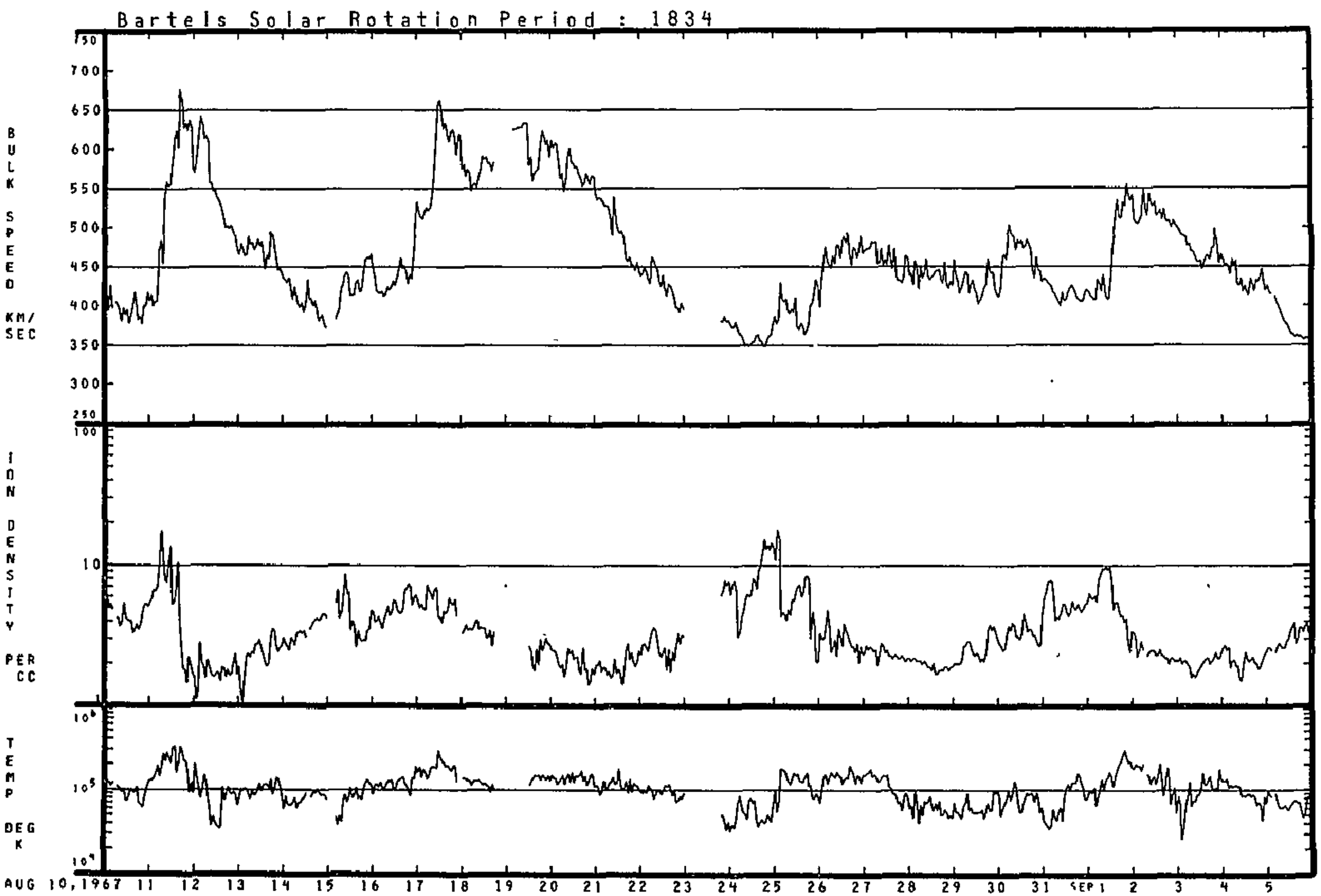
07/14/67 - 08/09/67

Bartels Solar Rotation Period : 1833

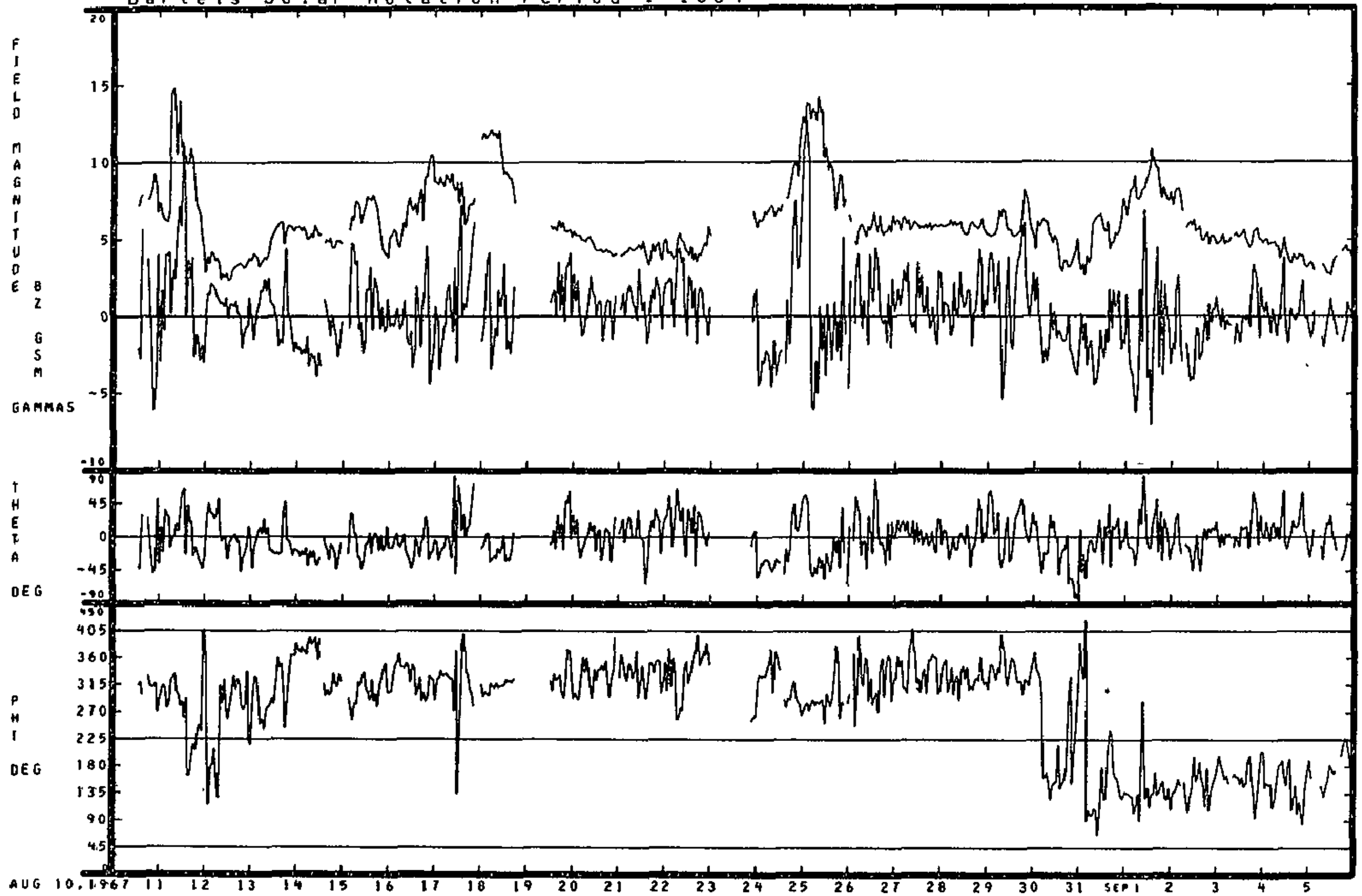


07/14/67 - 08/09/67

08/10/67 - 09/05/67

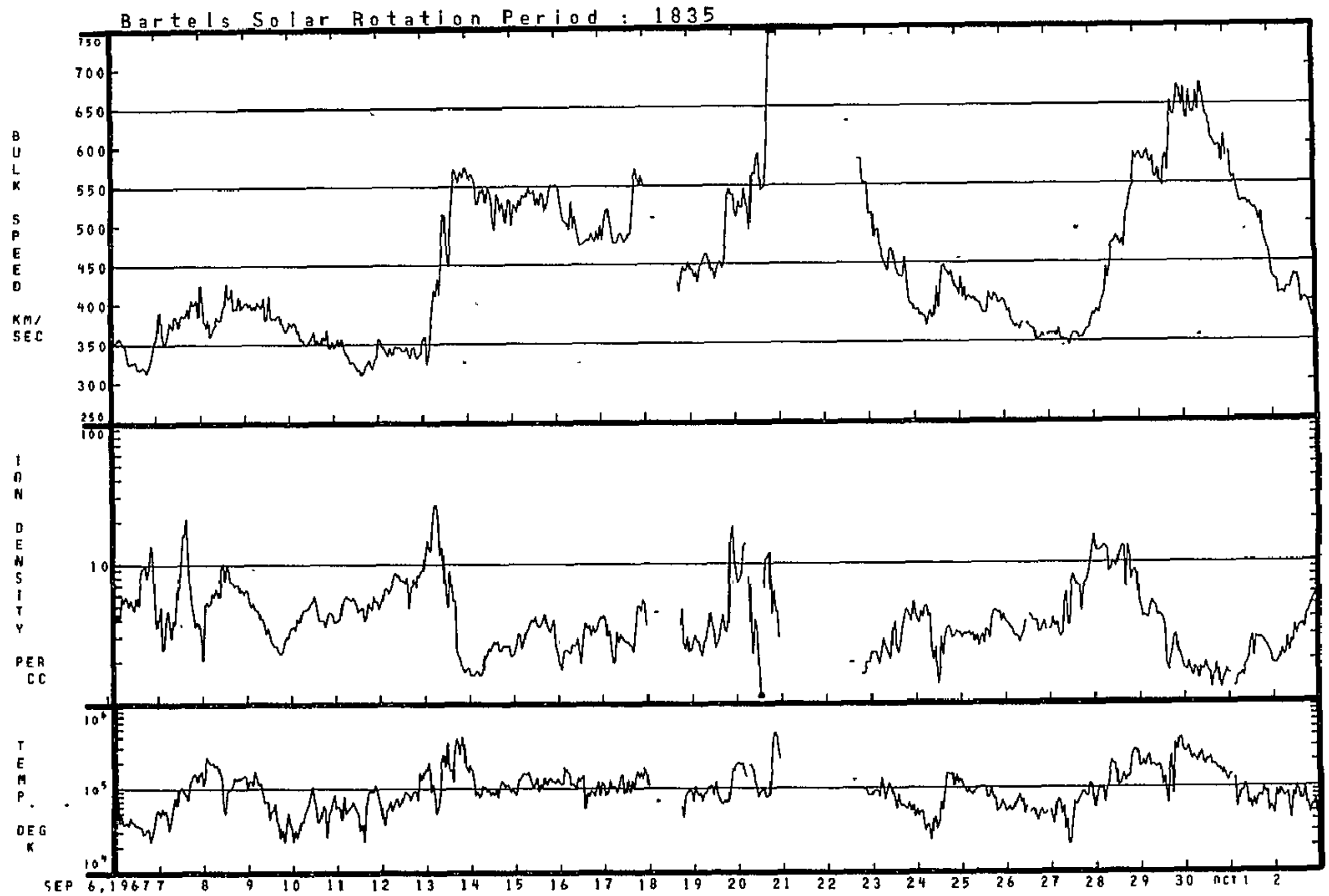


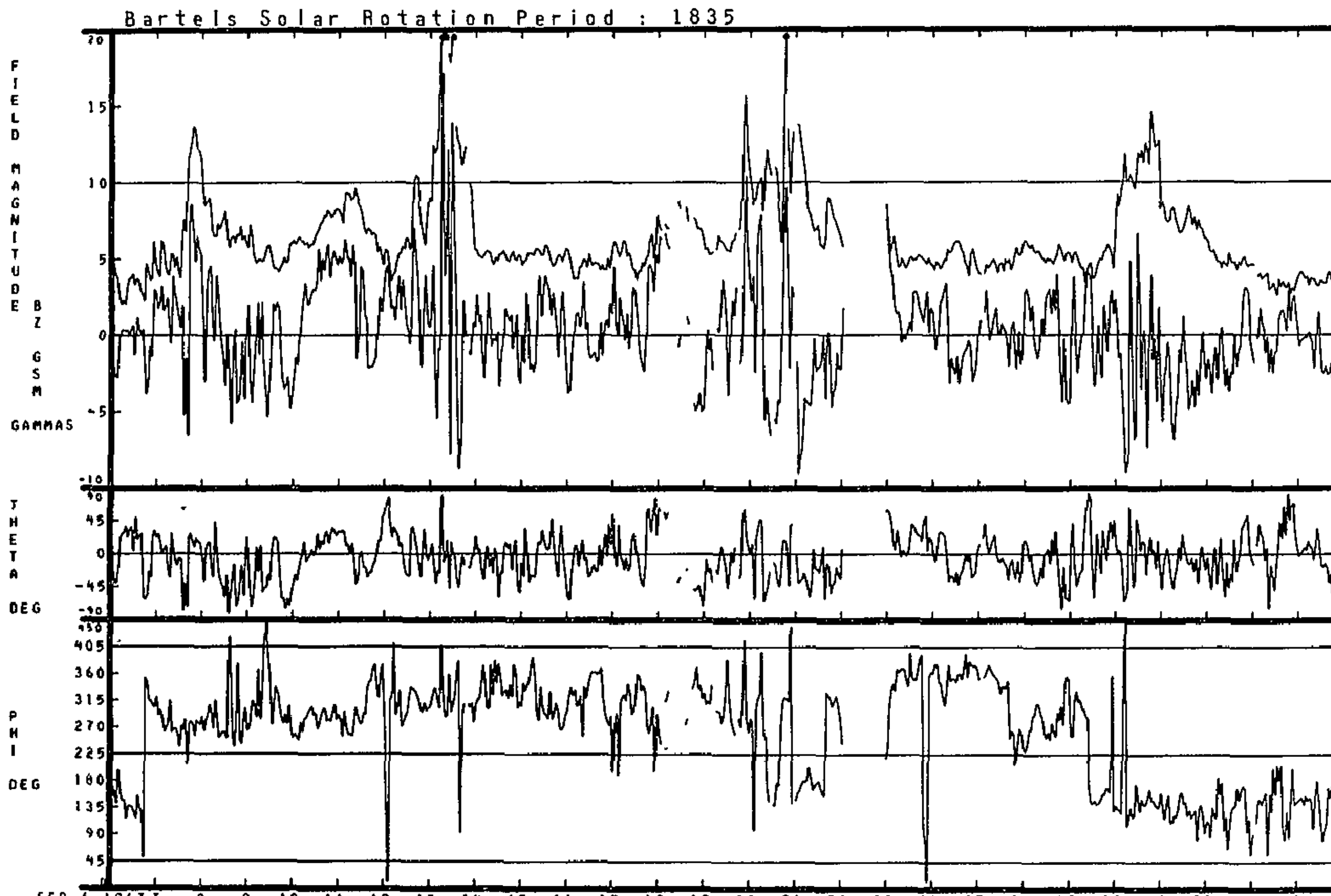
Barteis Solar Rotation Period : 1834



08/10/67 - 09/05/67

09/06/67 - 10/02/67

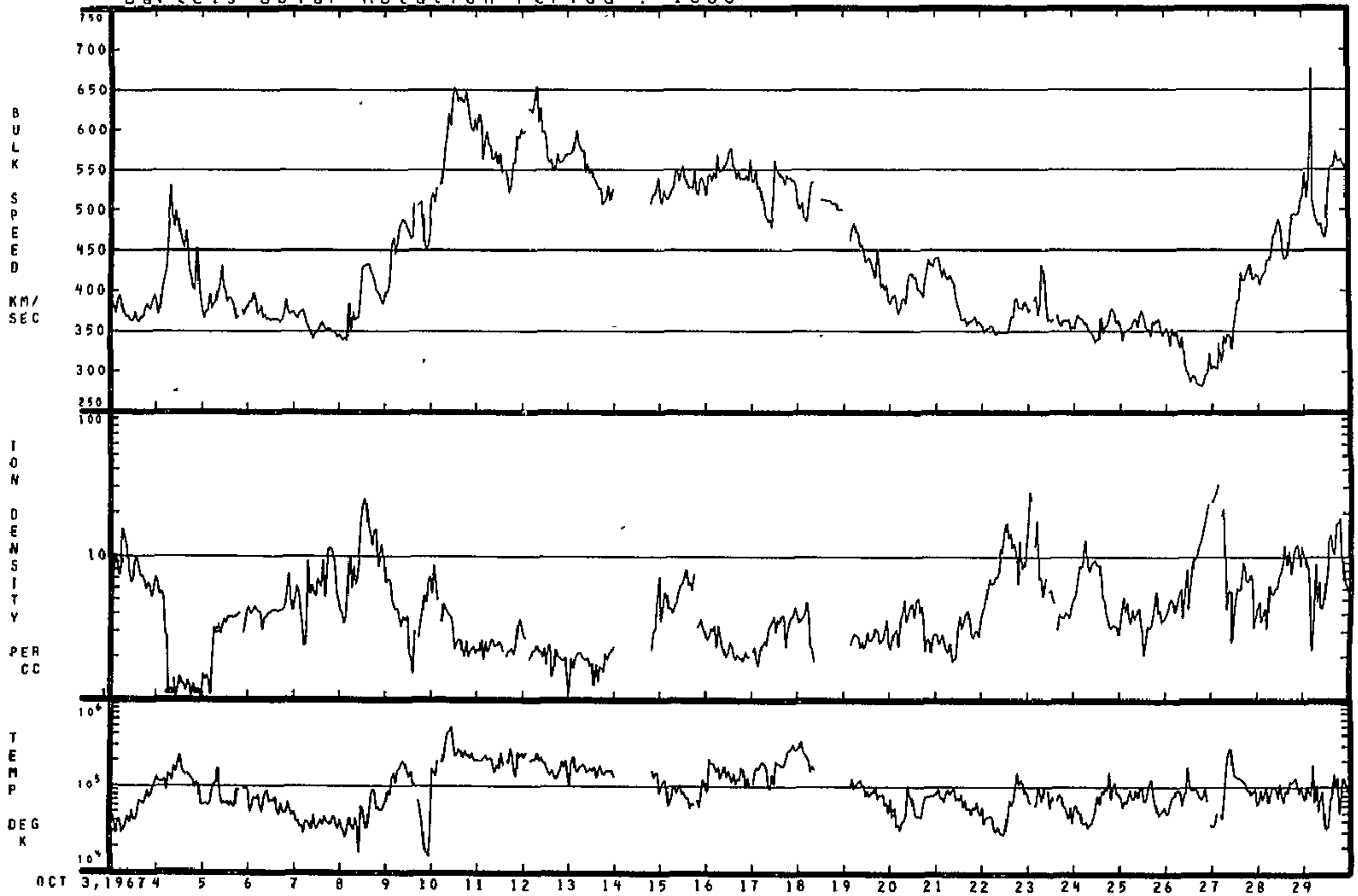




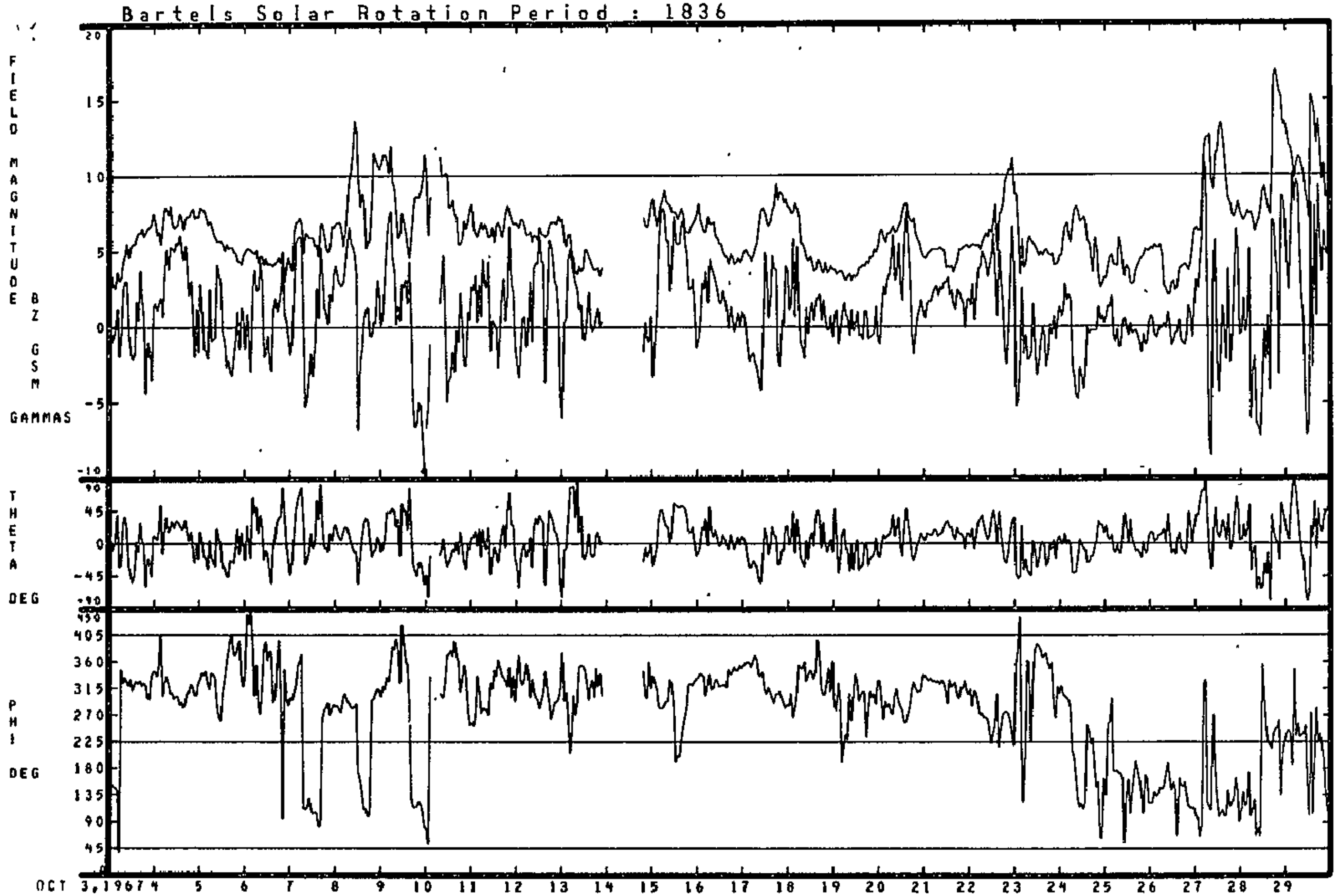
09/06/67 - 10/02/67

10/03/67 - 10/29/67

Bartels Solar Rotation Period : 1836



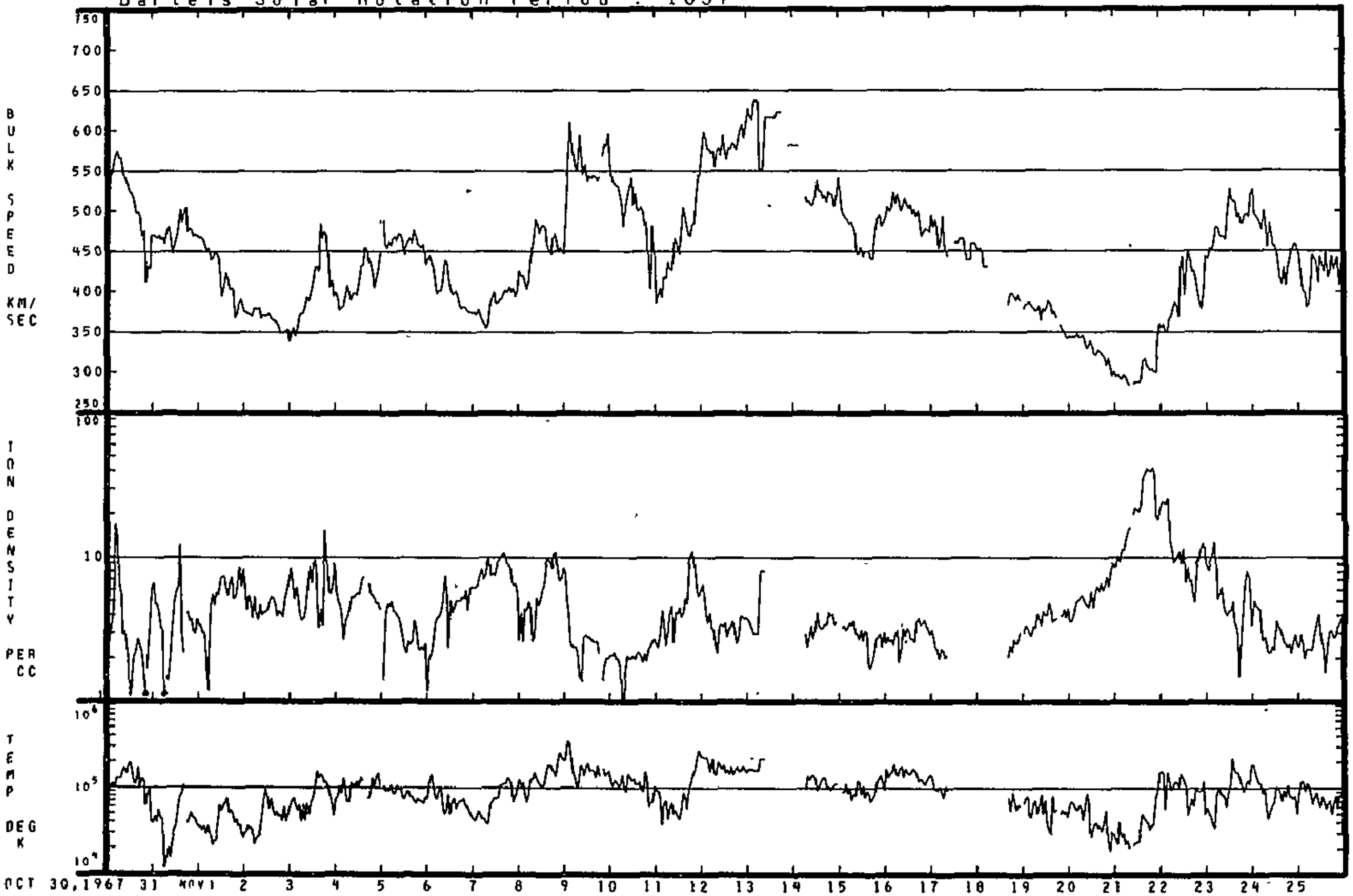
Bartels Solar Rotation Period : 1836



10/03/67 - 10/29/67

10/30/67 - 11/25/67

Bartels Solar Rotation Period : 1837



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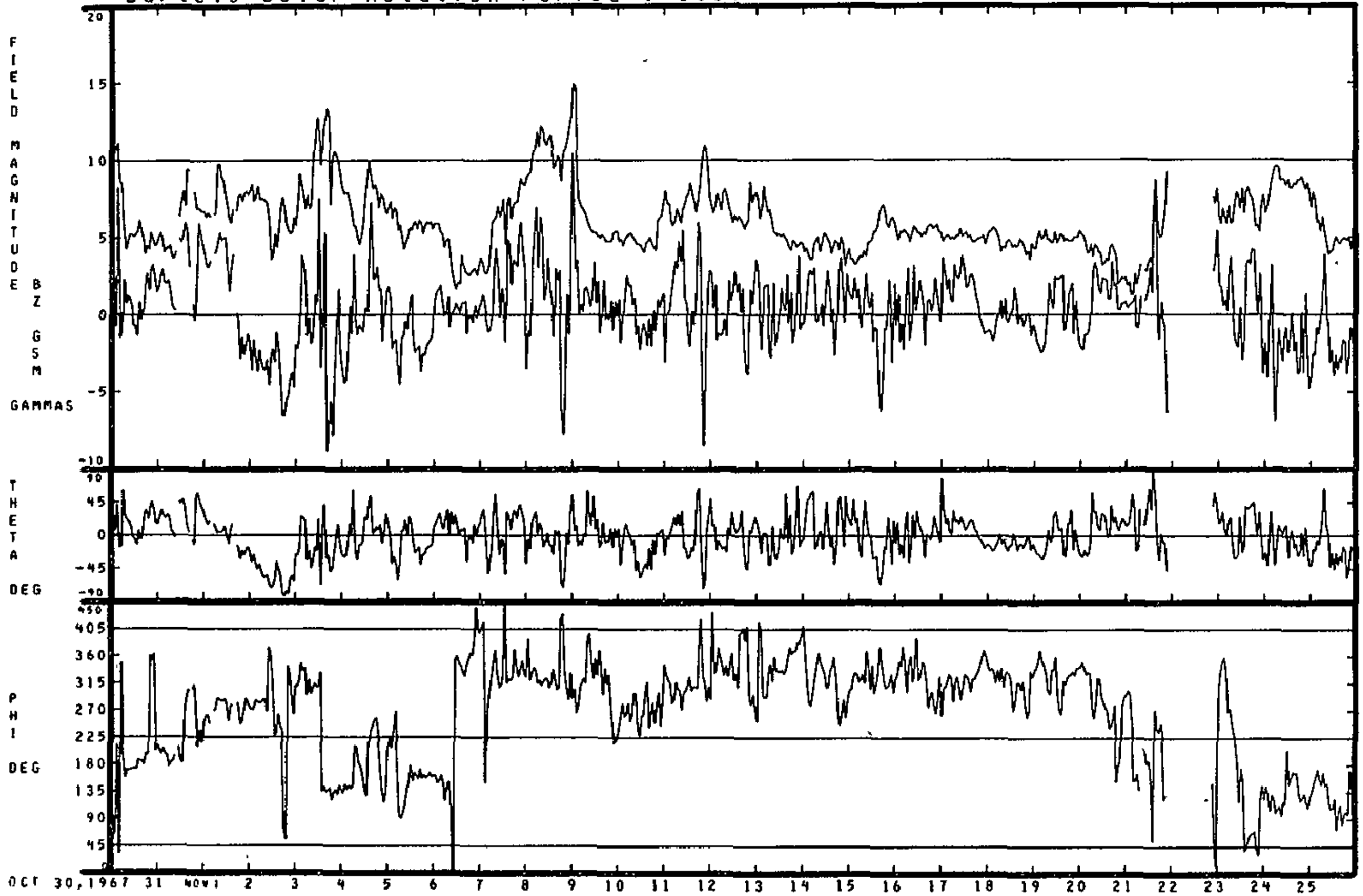
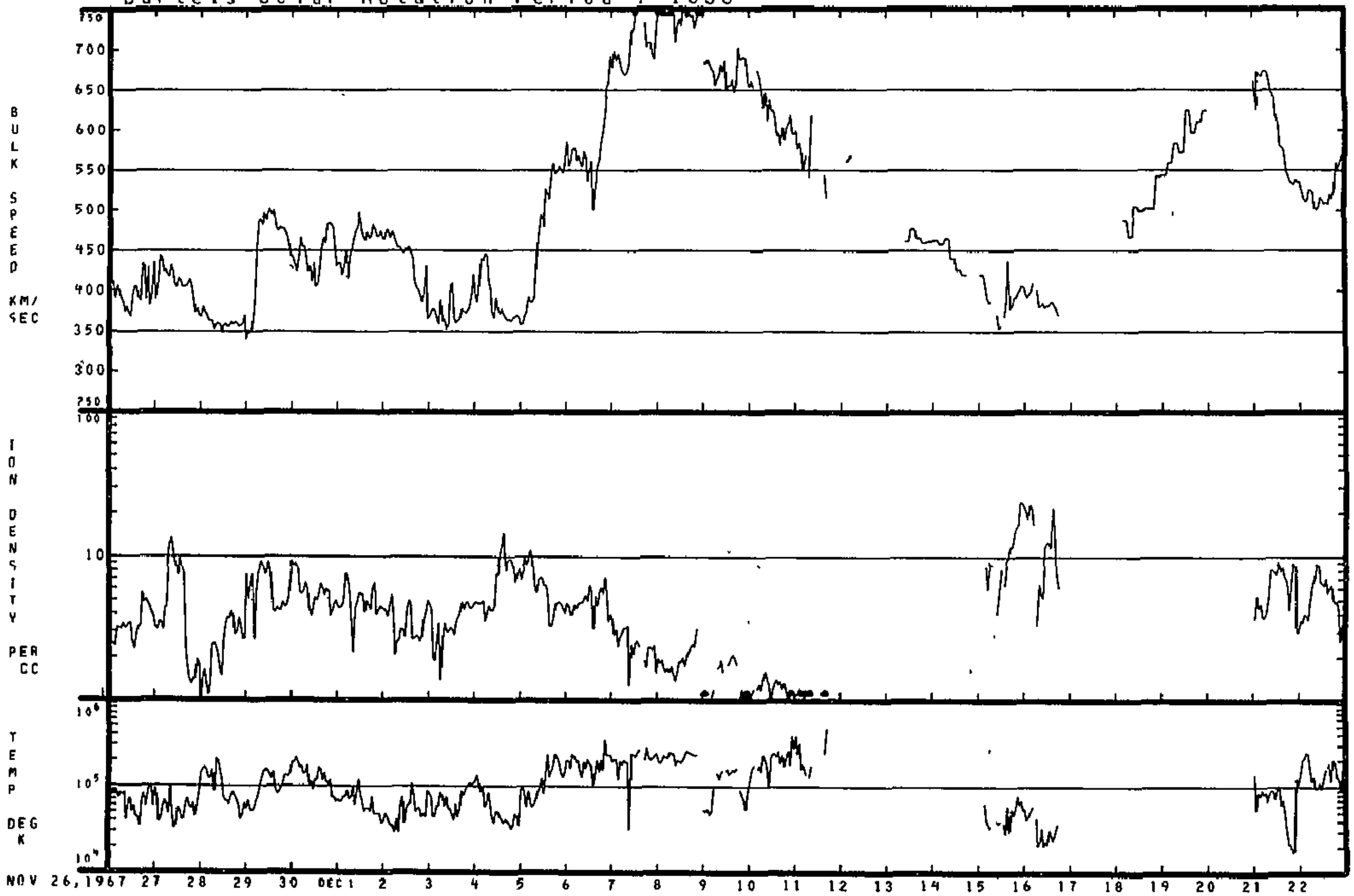


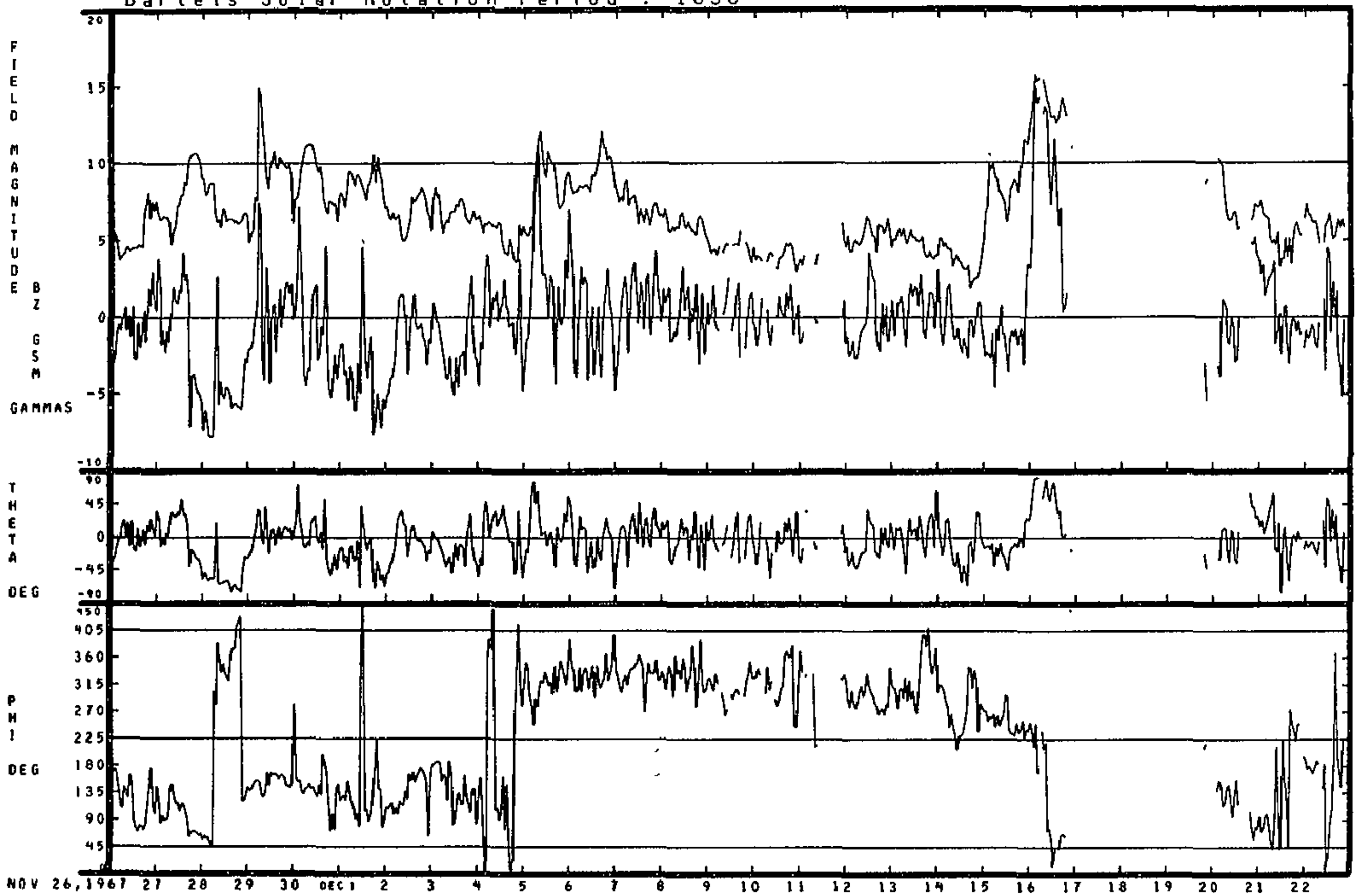
10/30/67 - 11/25/67

Bartels Solar Rotation Period : 1838



11/26/67 - 12/22/67

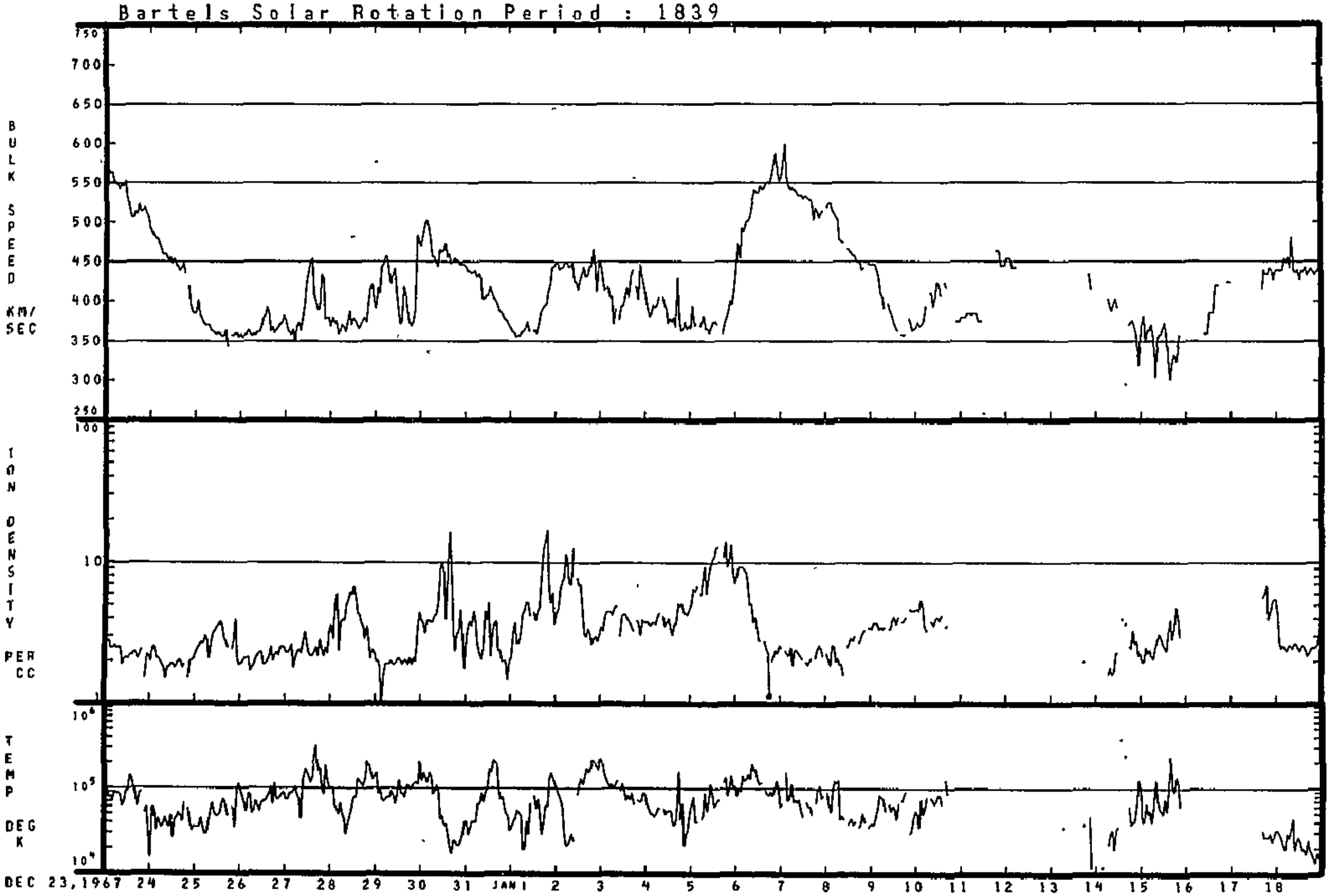
Bartels Solar Rotation Period : 1838



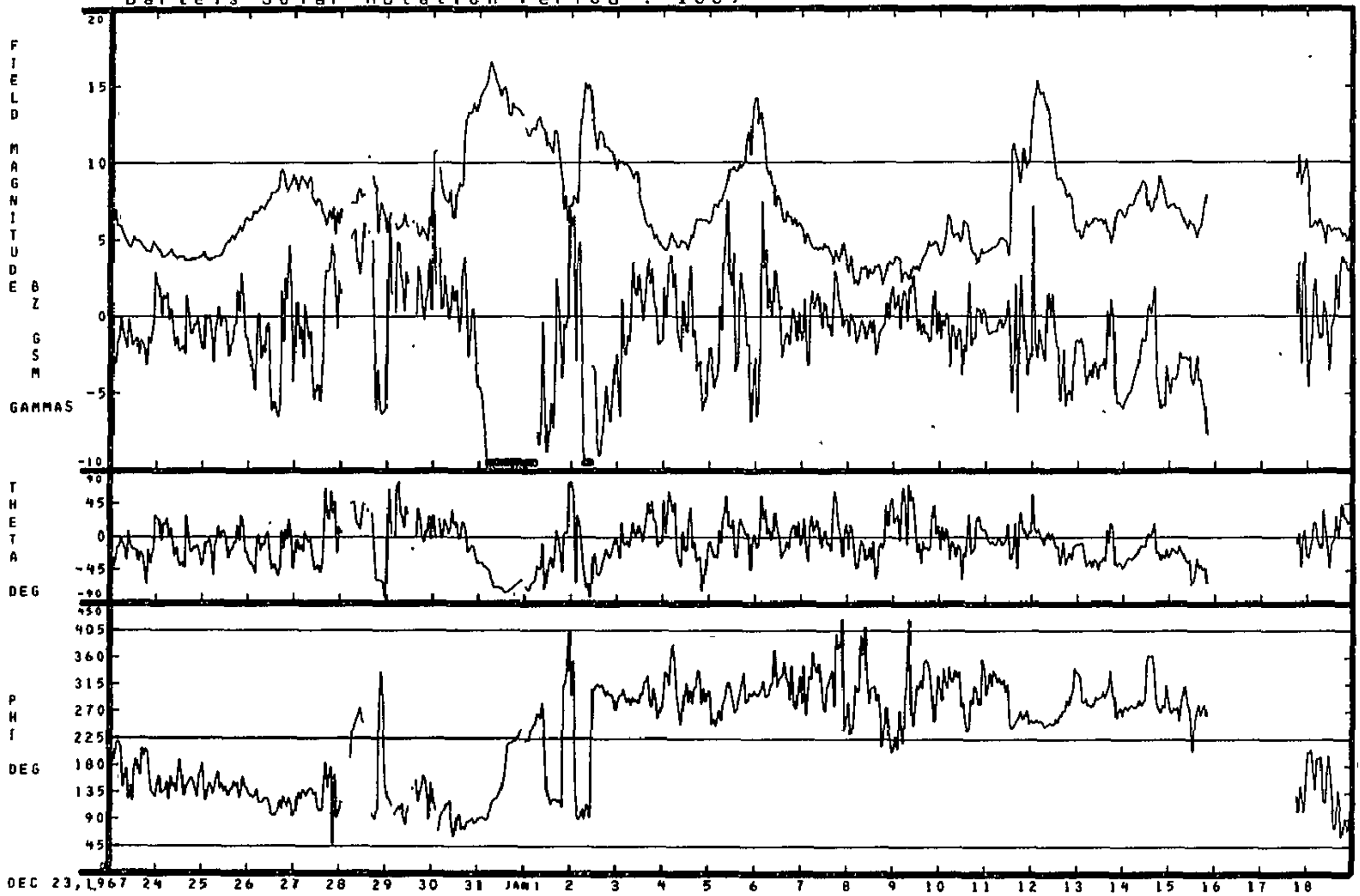
11/26/67 - 12/22/67

12/23/67 - 01/18/68

Bartels Solar Rotation Period : 1839



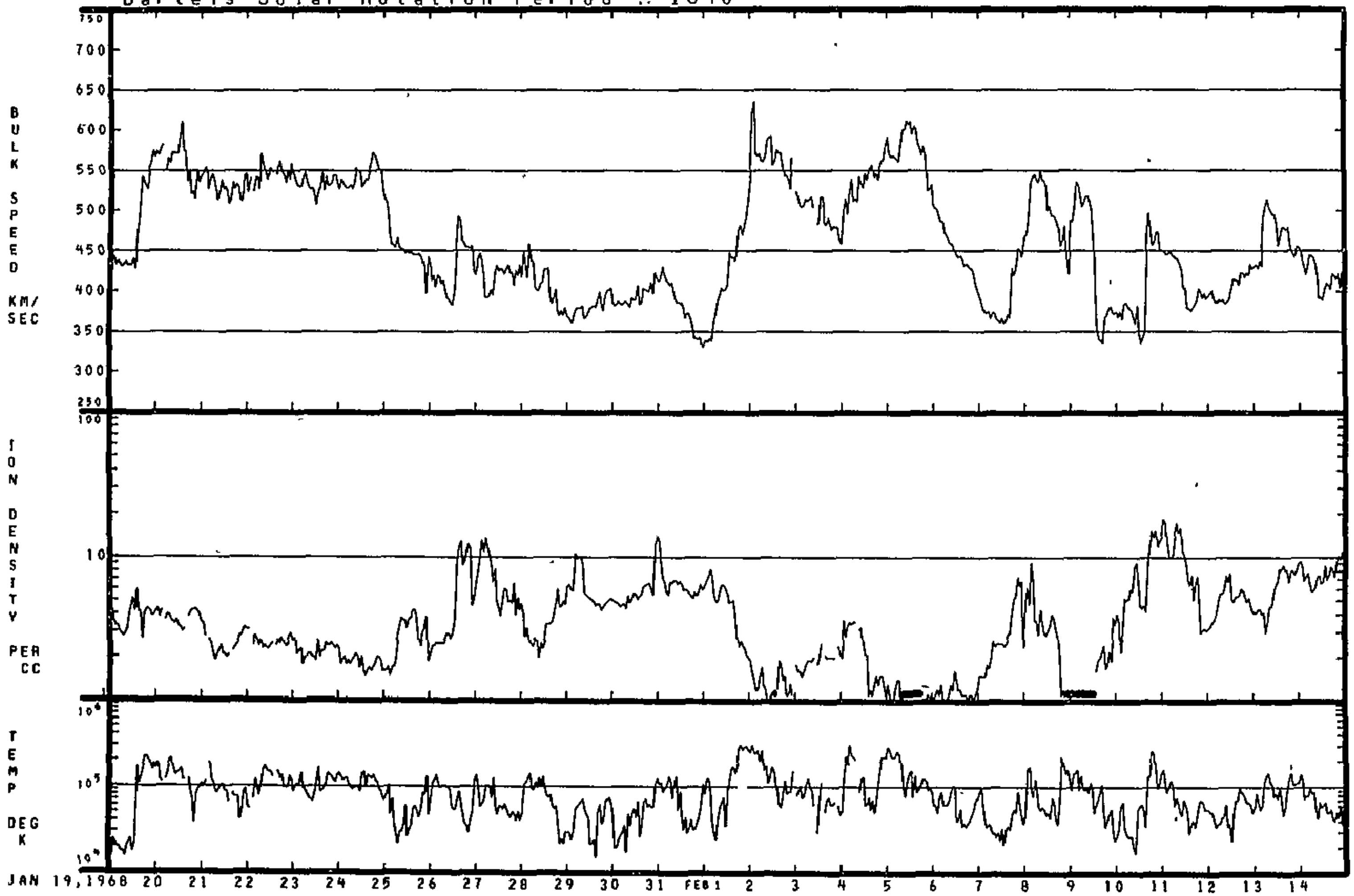
Barteis Solar Rotation Period : 1839

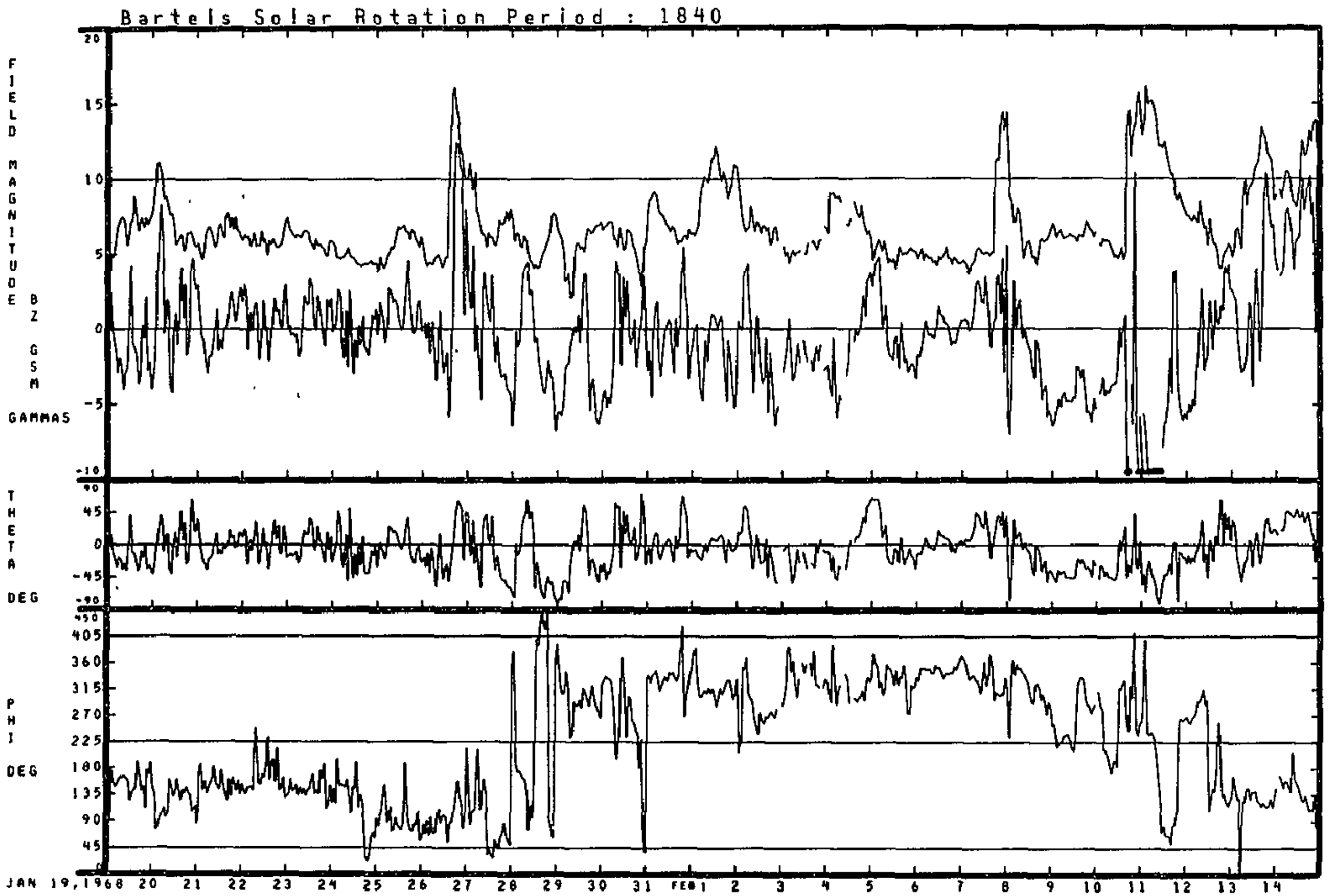


12/23/67 - 01/18/68

01/19/68 - 02/14/68

Bartels Solar Rotation Period : 1840

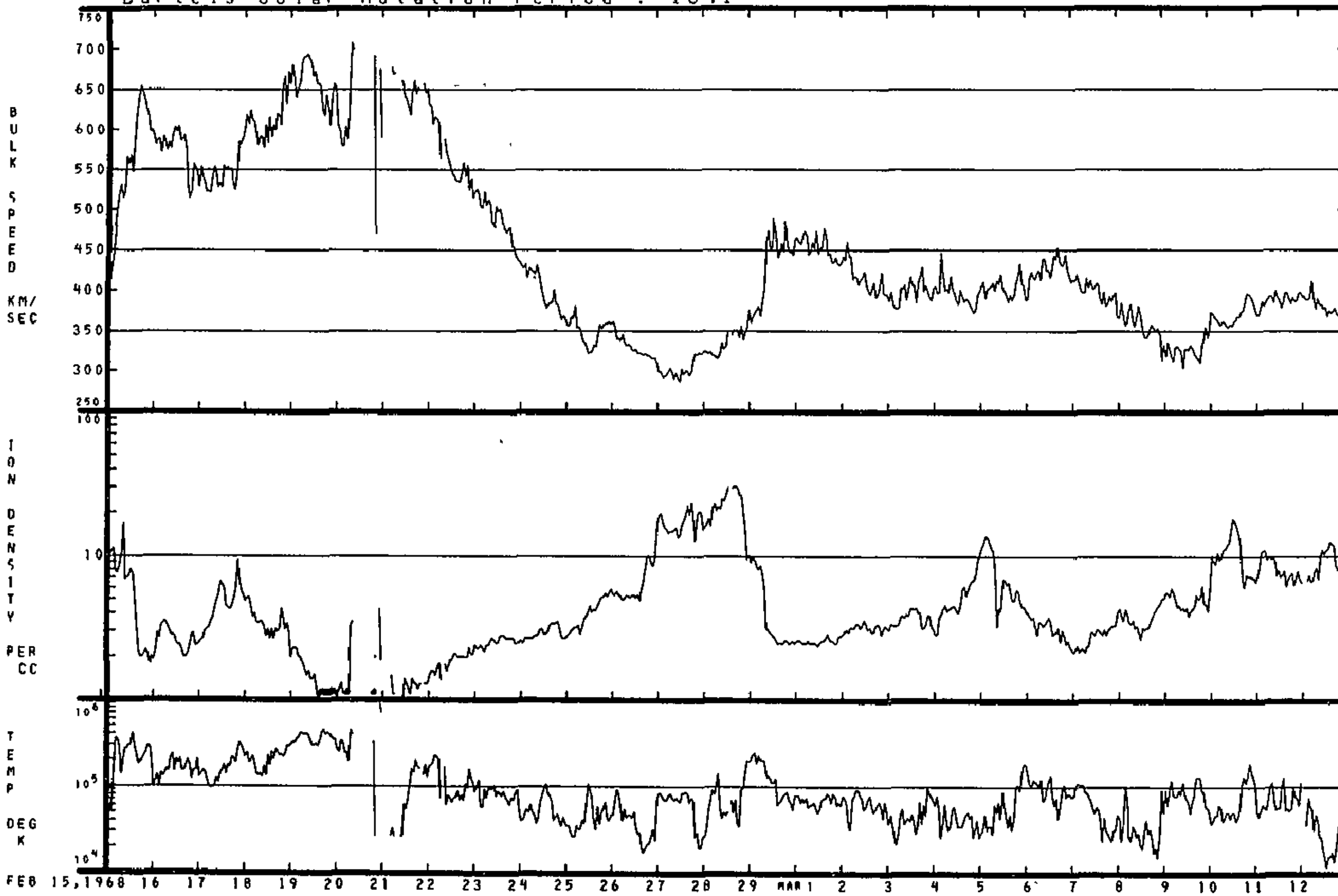




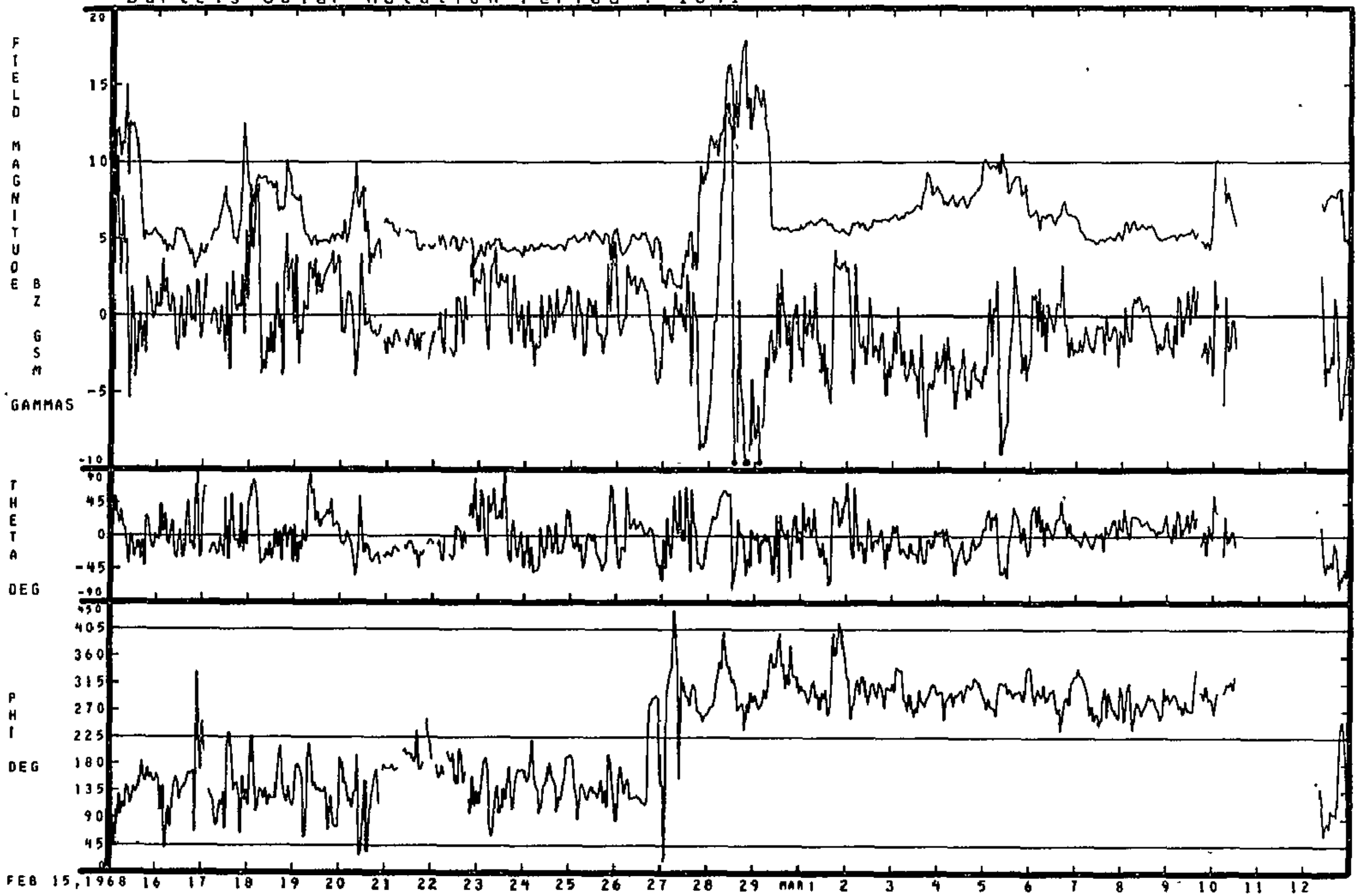
01/19/68 - 02/14/68

02/15/68 - 03/12/68

Bartels Solar Rotation Period : 1841



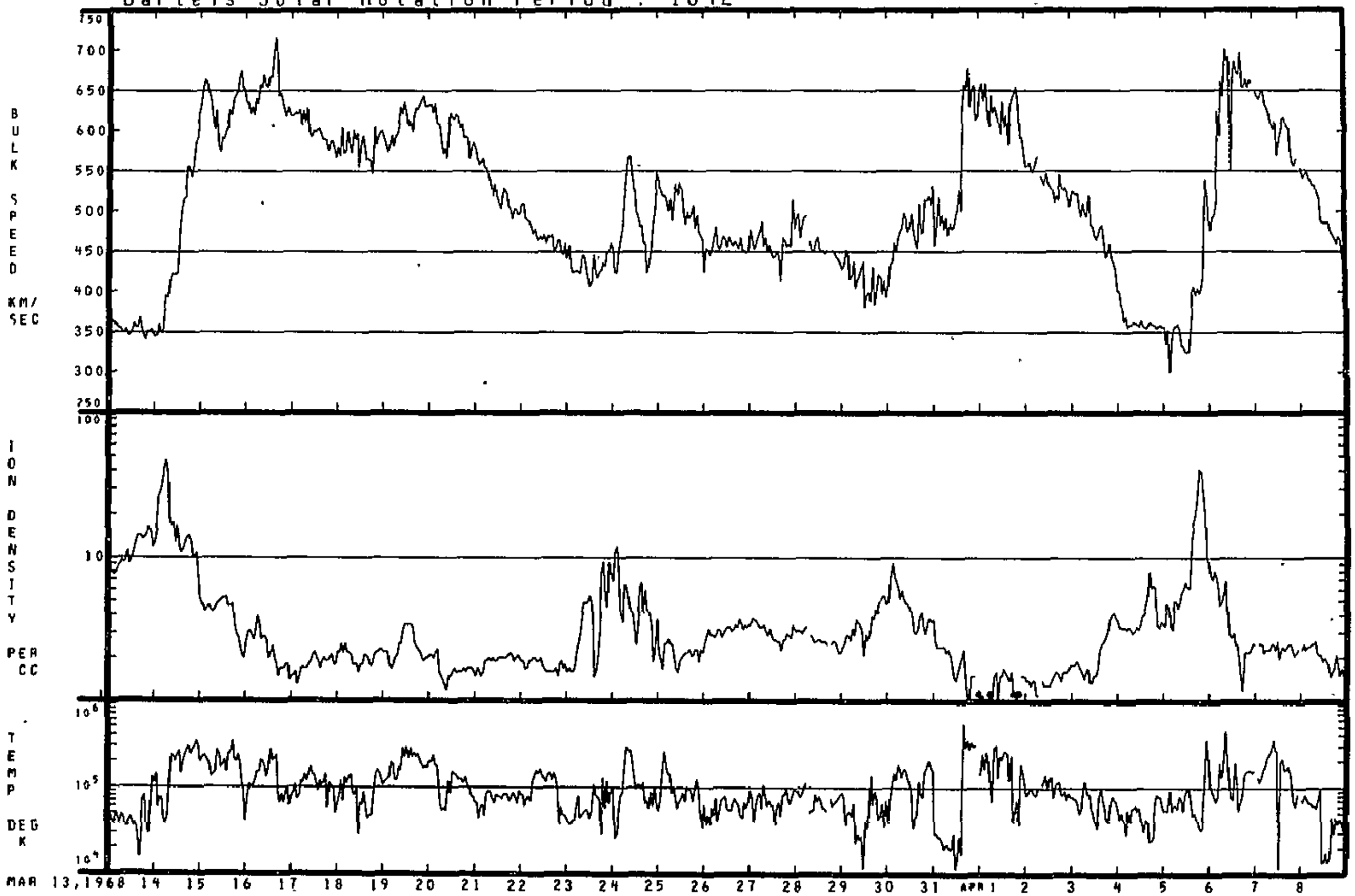
Bartels Solar Rotation Period : 1841

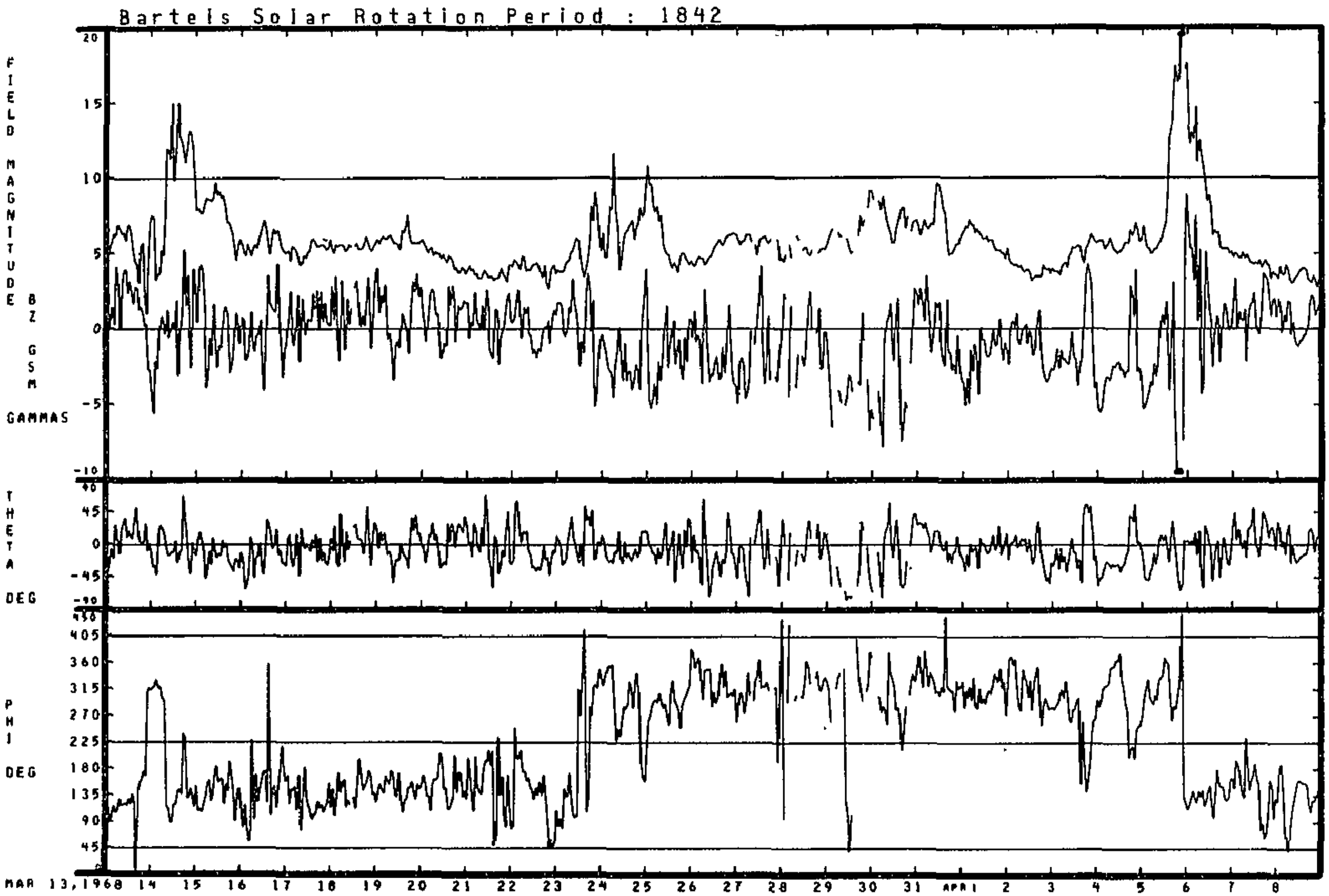


02/15/68 - 03/12/68

03/13/68 - 04/08/68

Bartels Solar Rotation Period : 1842

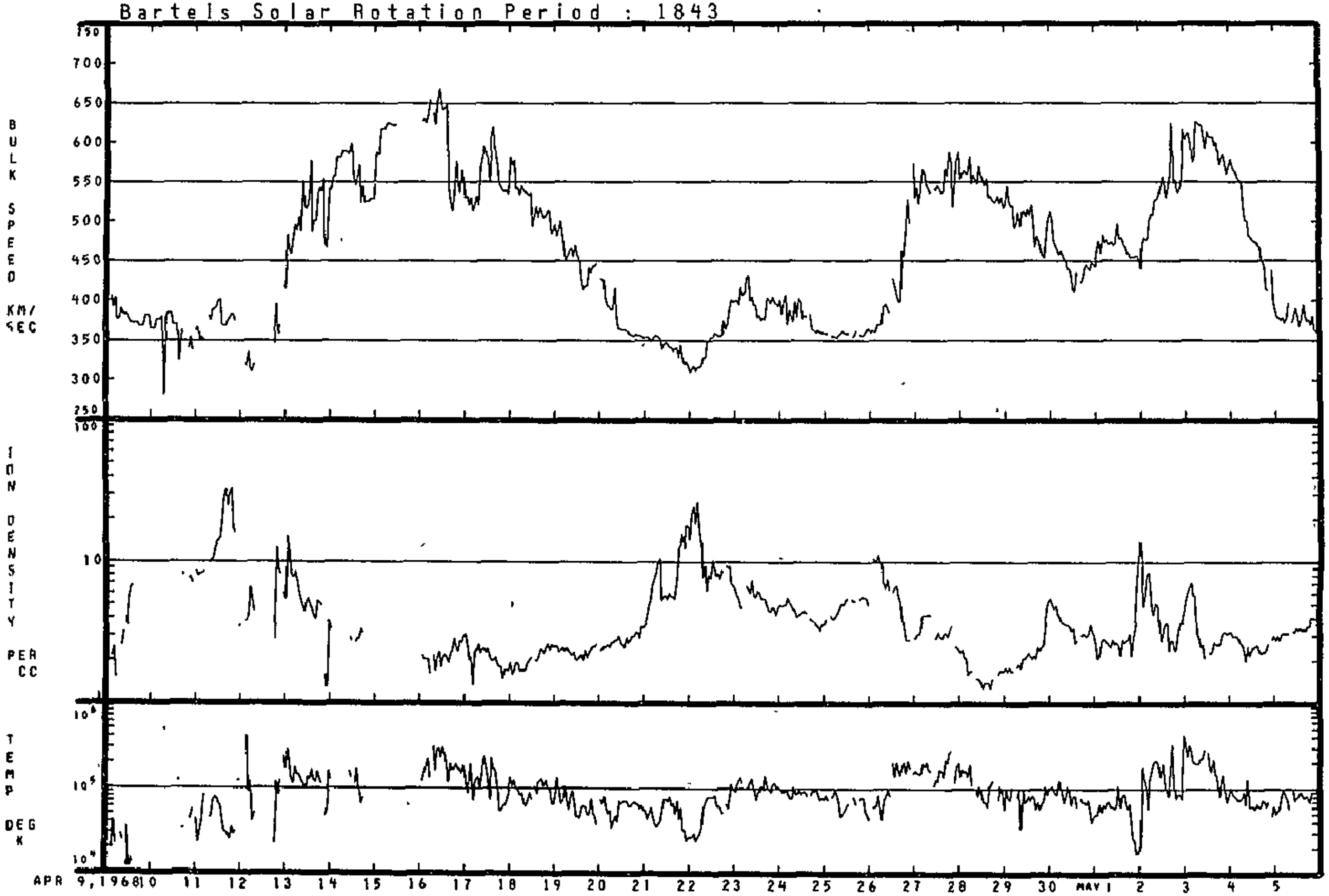




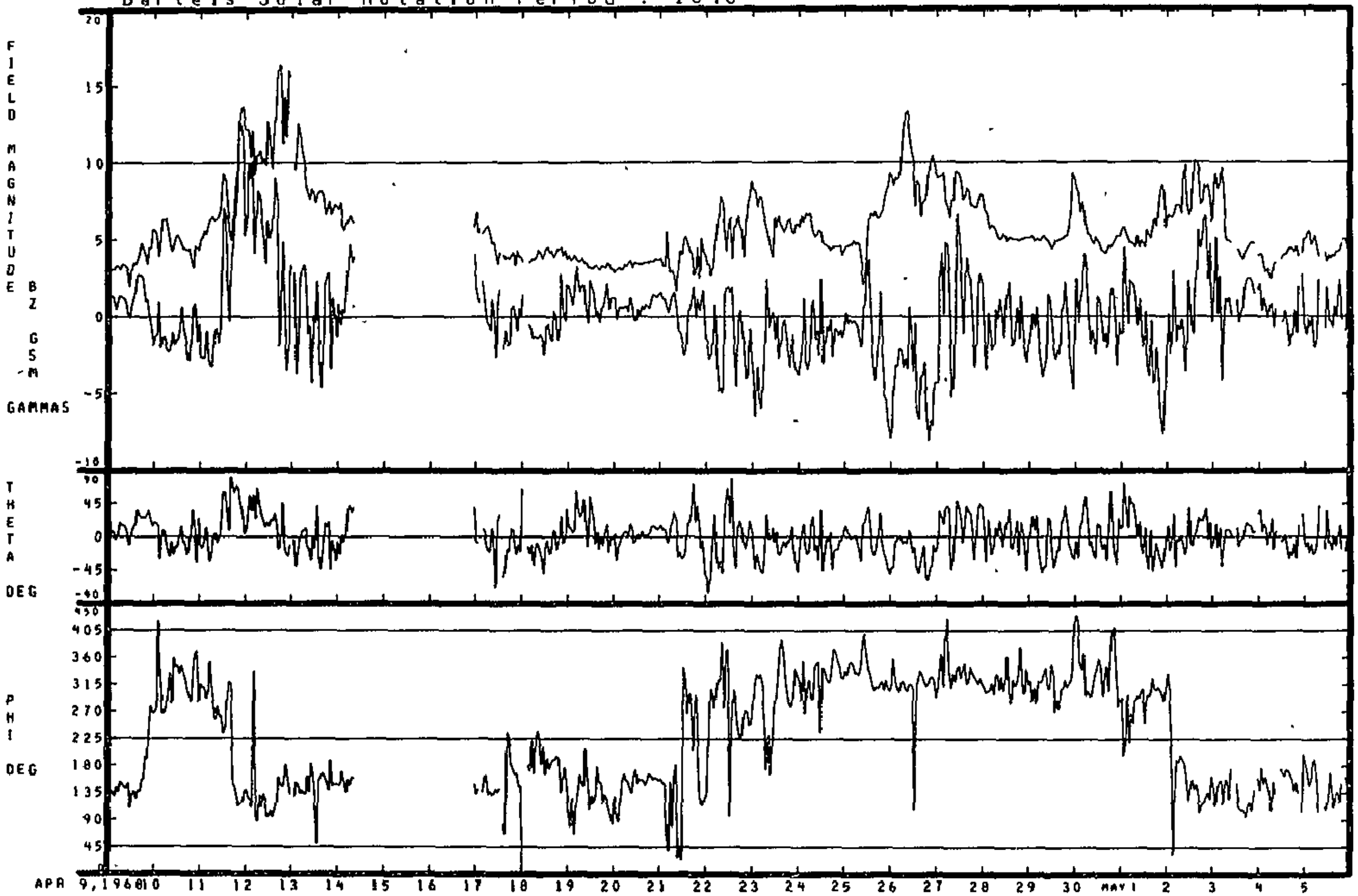
03/13/68 - 04/08/68

04/09/68 - 05/05/68

Bartels Solar Rotation Period : 1843



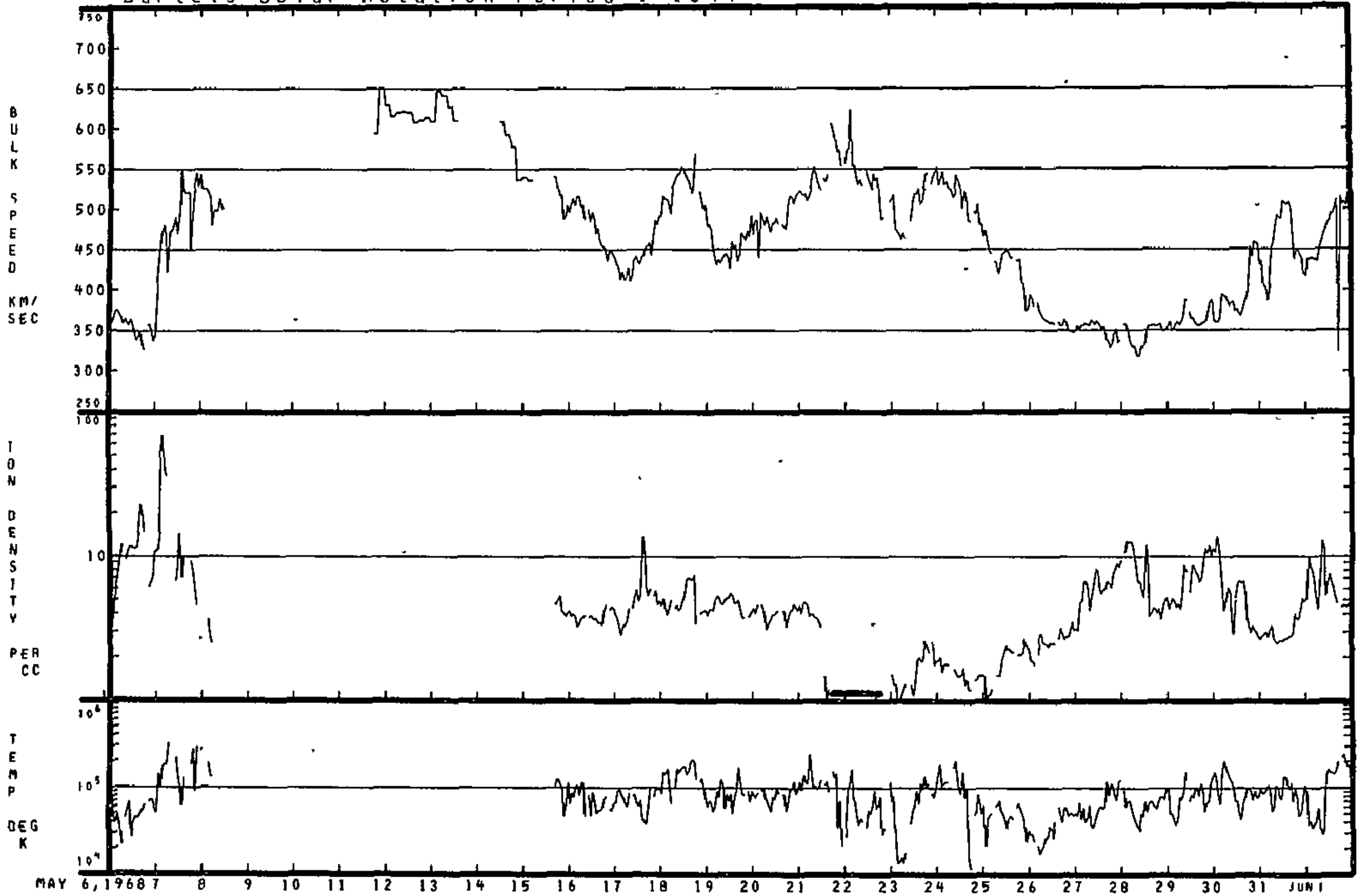
Bartels Solar Rotation Period : 1843

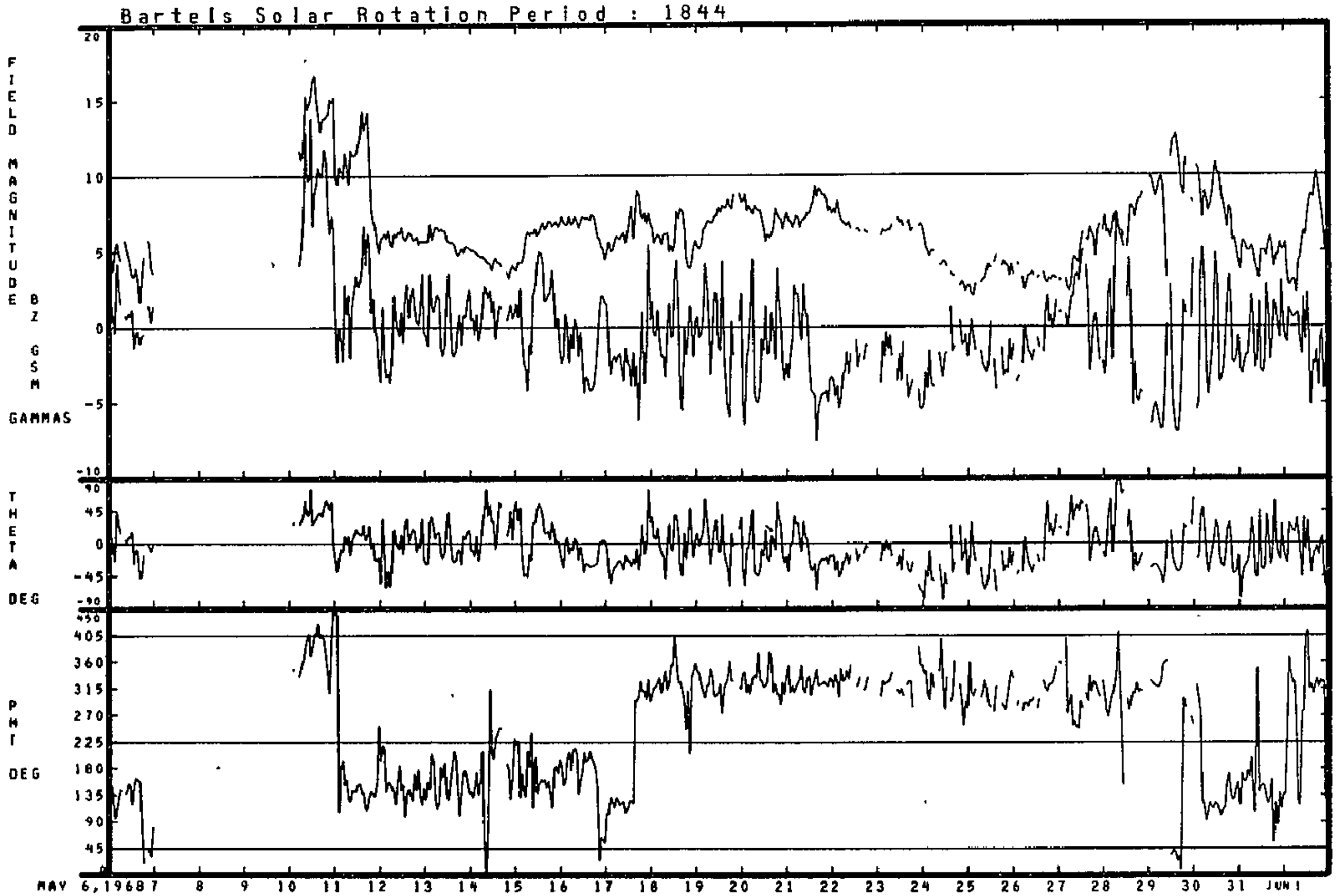


04/09/68 - 05/05/68

05/06/68 - 06/01/68

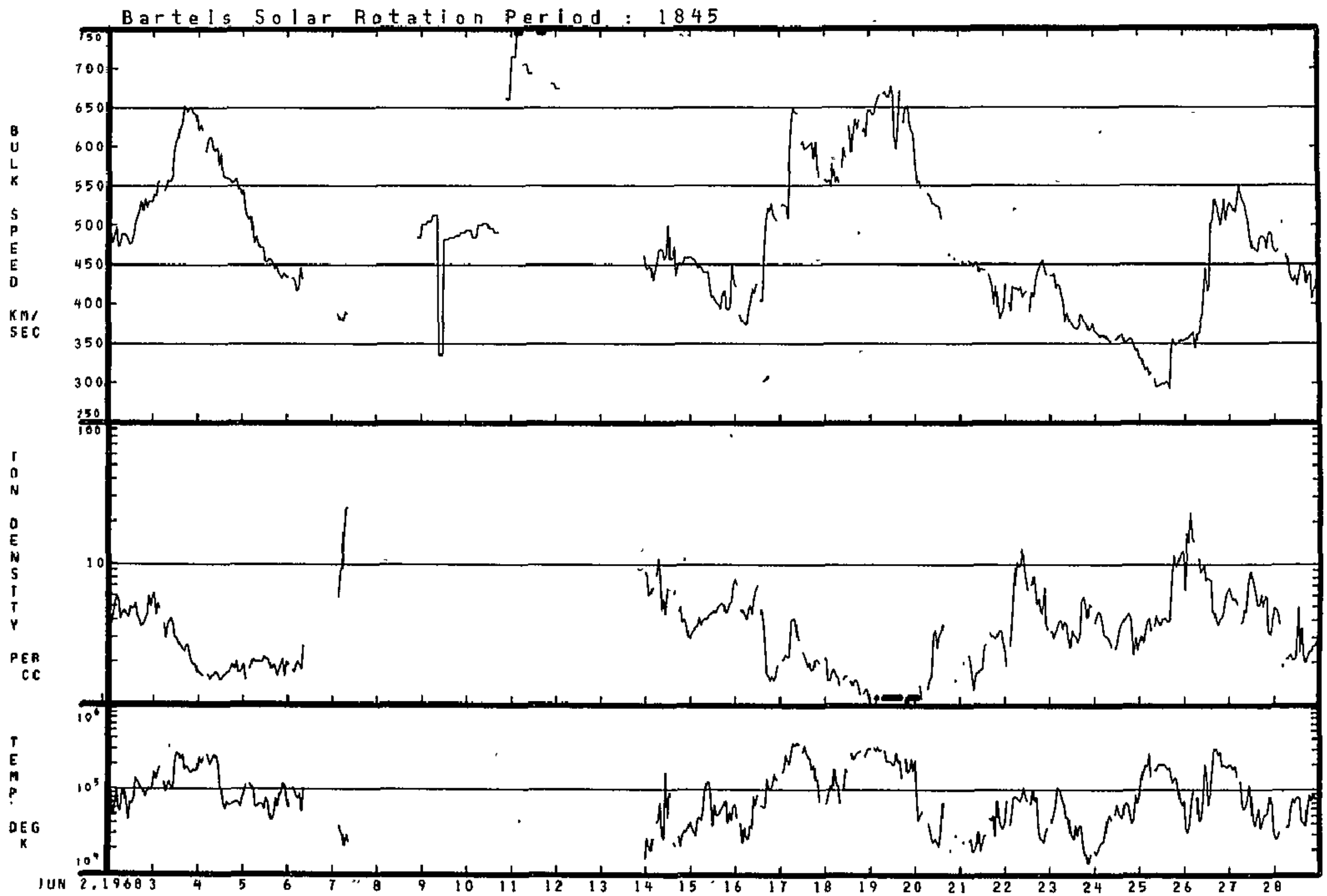
Bartels Solar Rotation Period : 1844

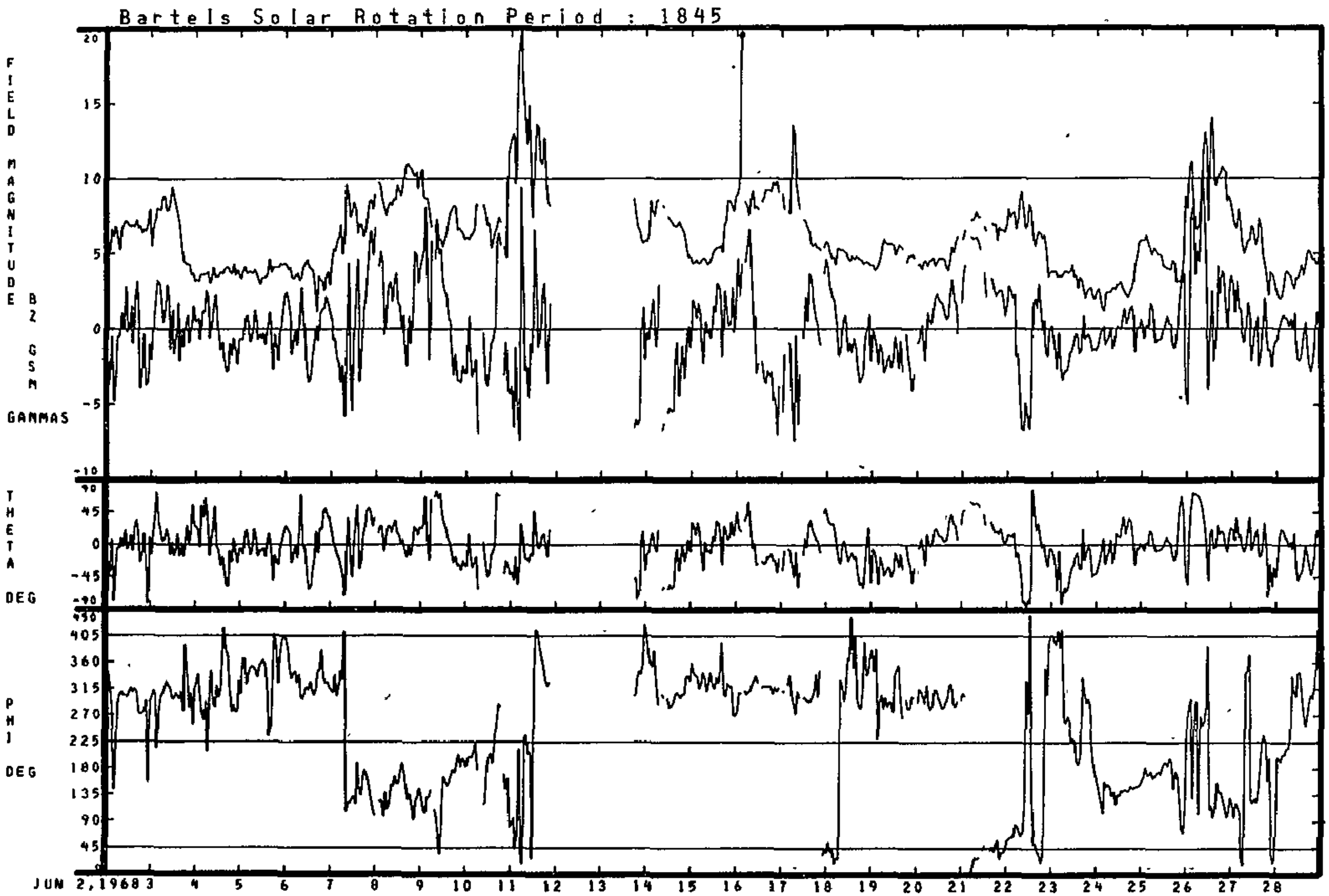




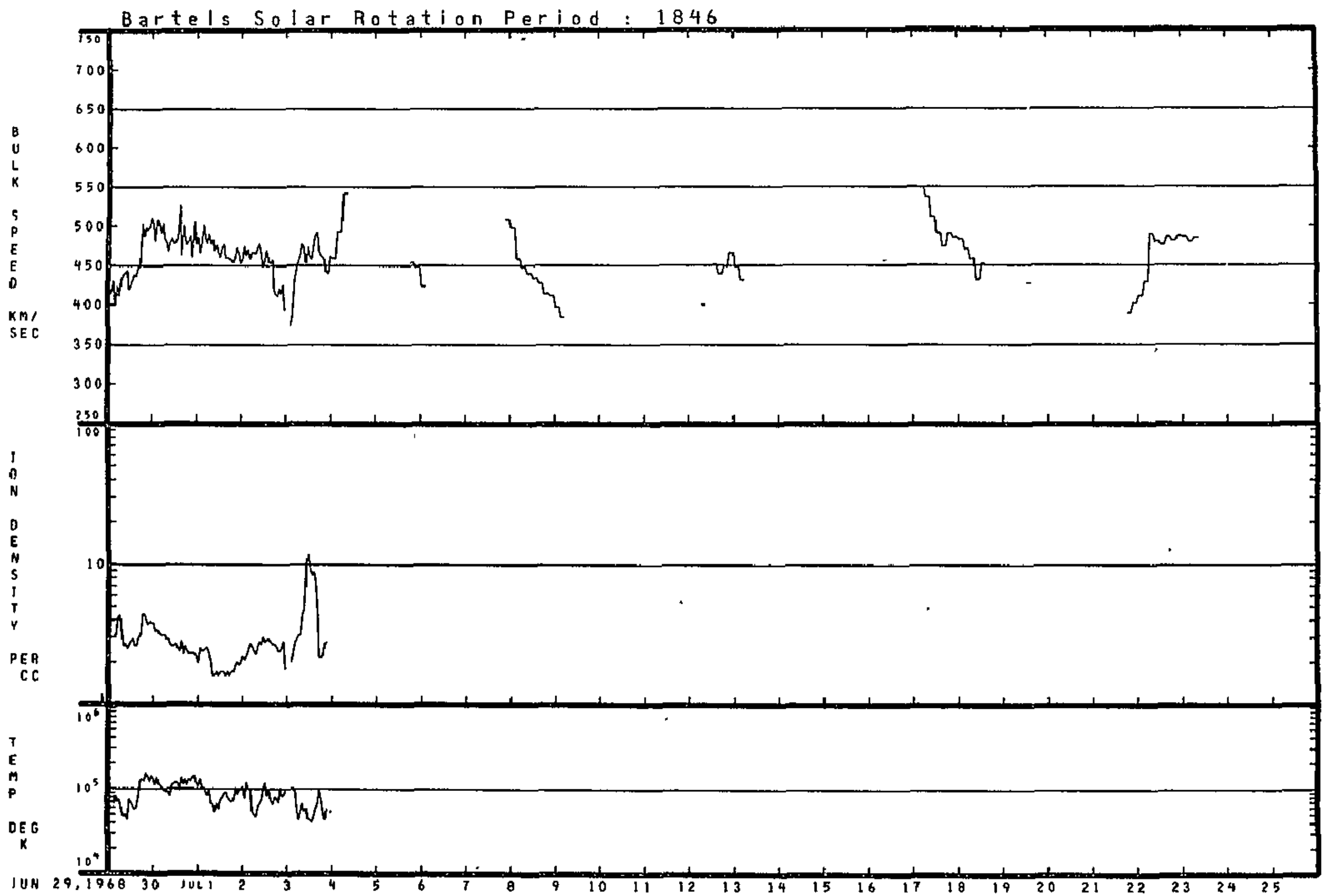
05/06/68 - 06/01/68

06/02/68 - 06/28/68



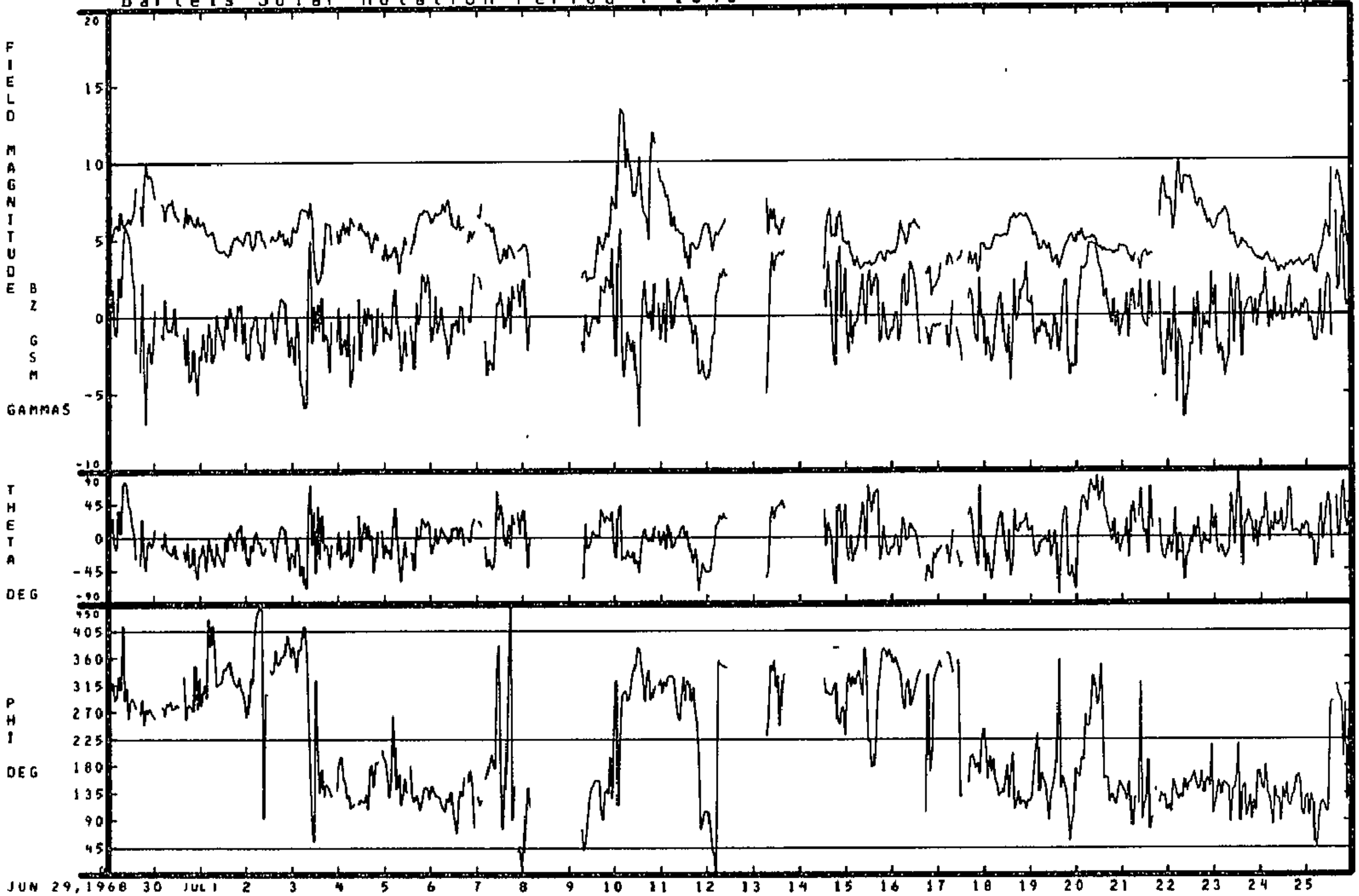


06/02/68 - 06/28/68



06/29/68 - 07/25/68

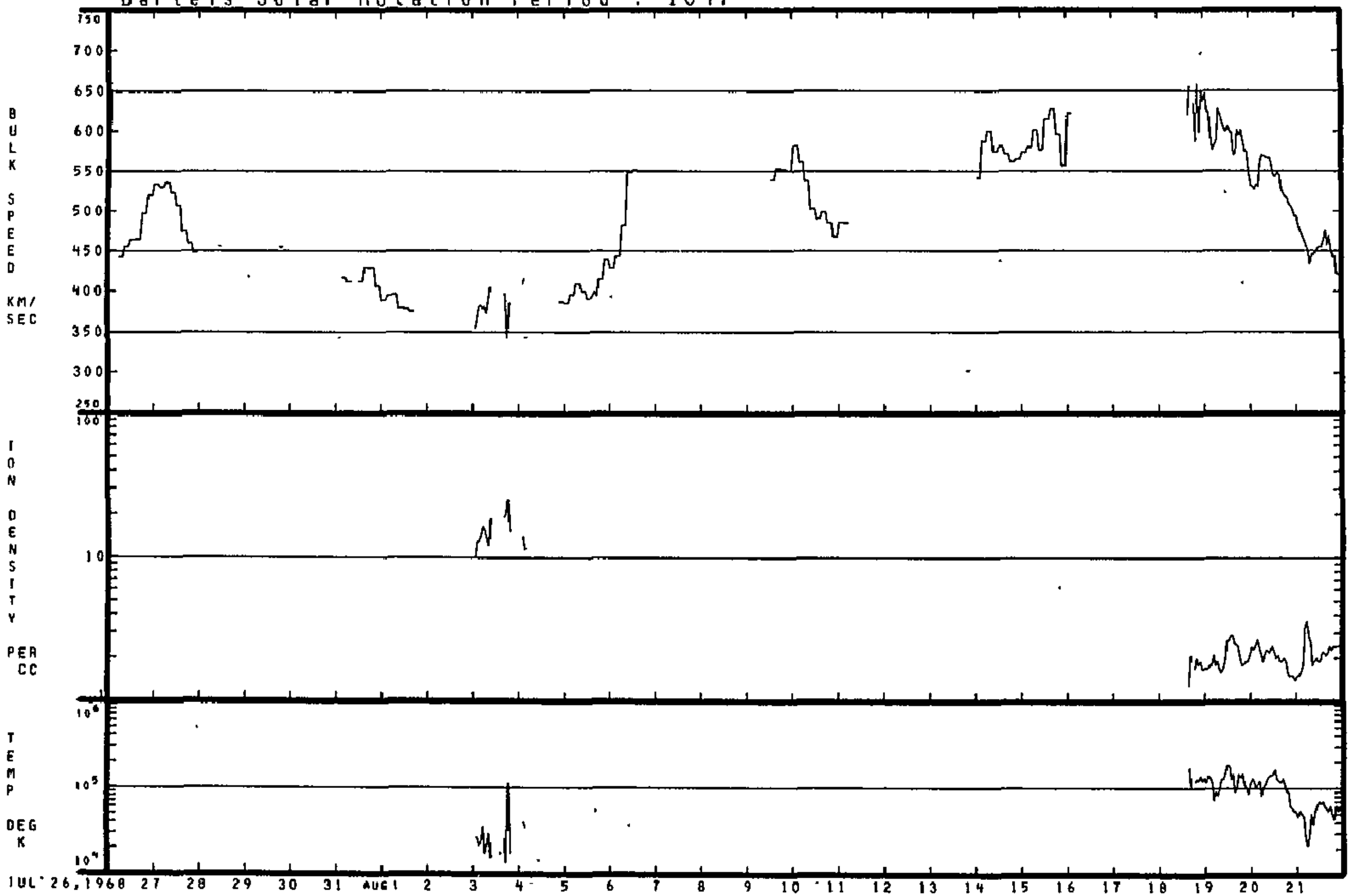
Bartels Solar Rotation Period : 1846



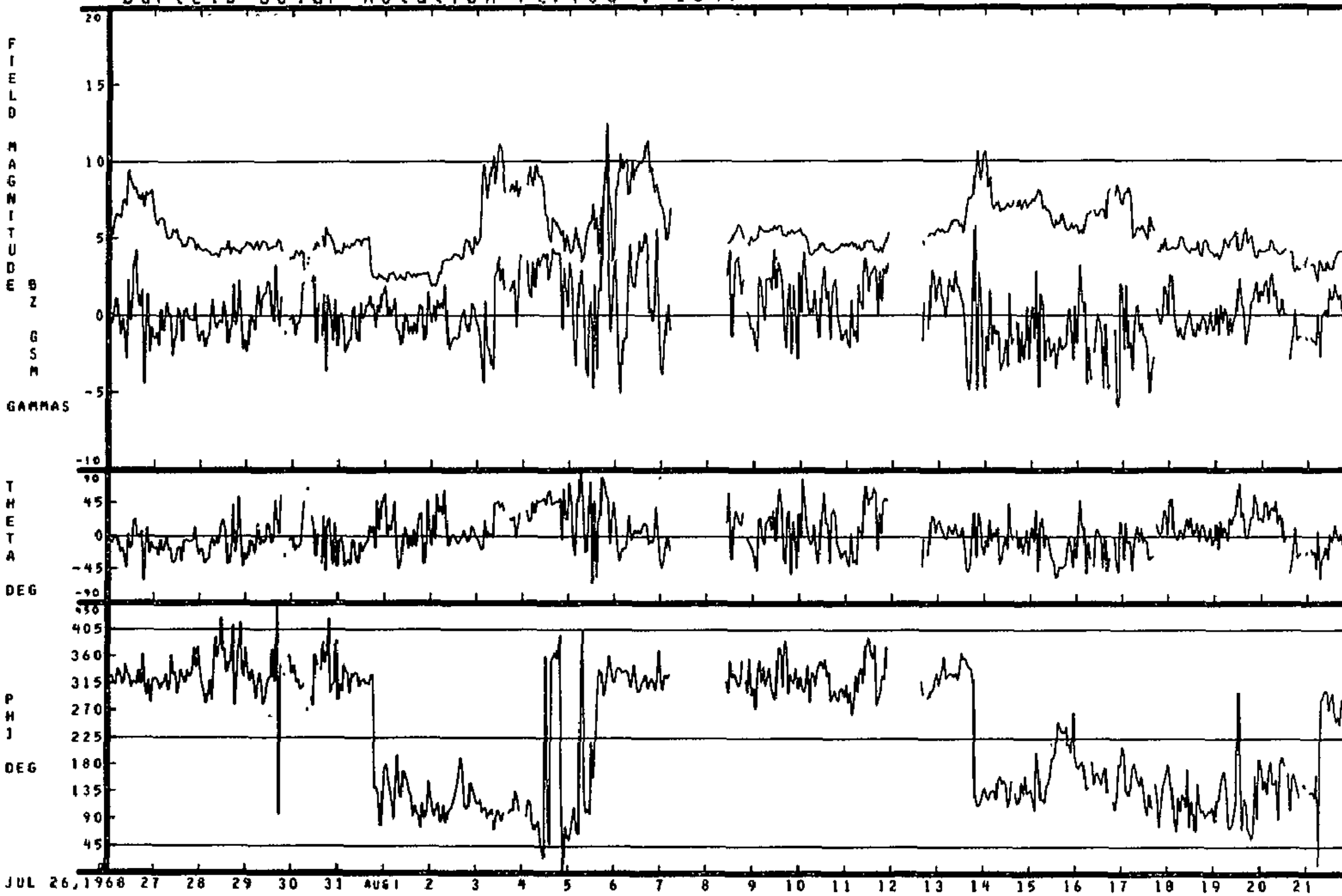
06/29/68 - 07/25/68

07/26/68 - 08/21/68

Bartels Solar Rotation Period : 1847



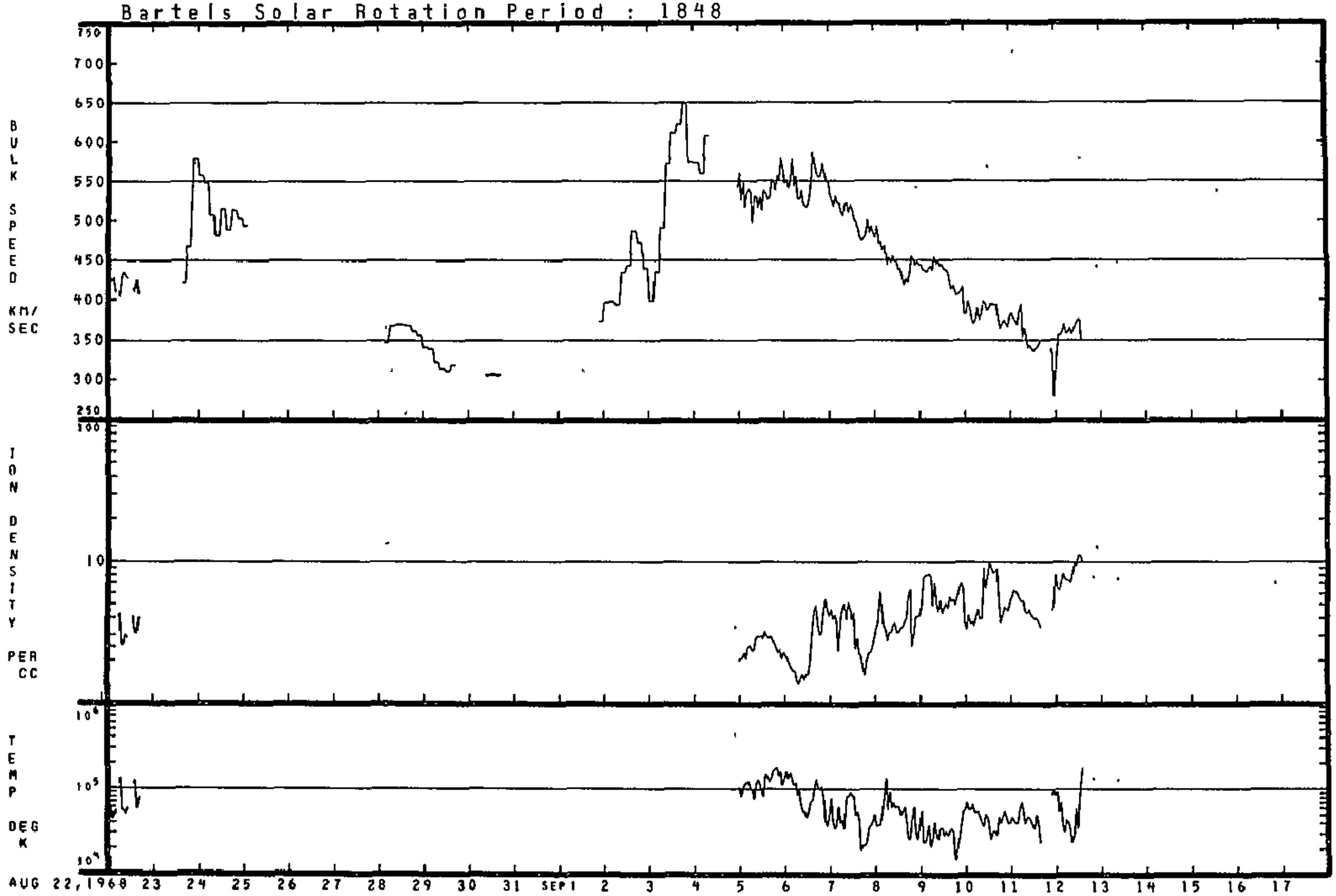
Bartels Solar Rotation Period : 1847



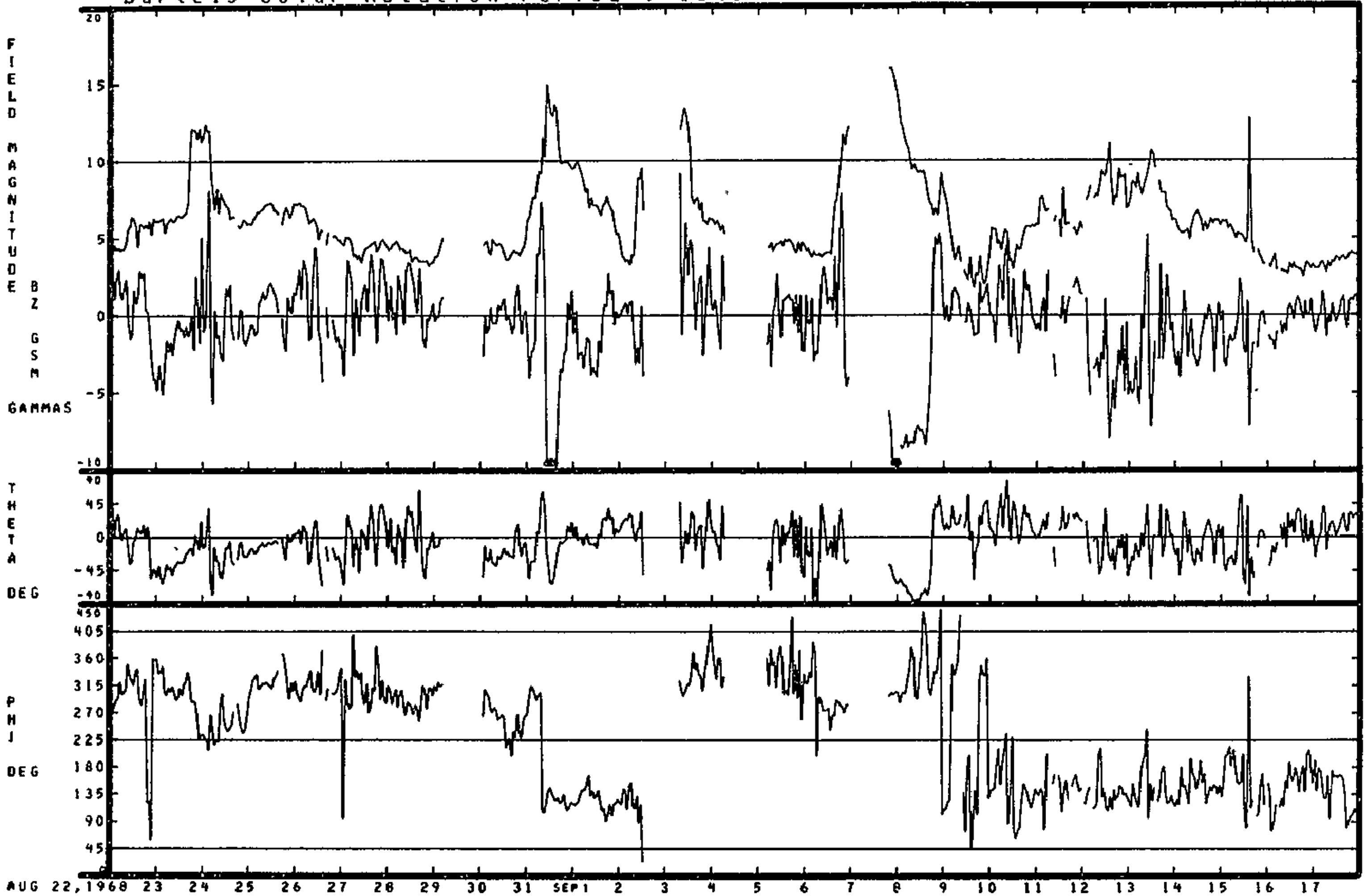
07/26/68 - 08/21/68

08/22/68 - 09/17/68

Bartels Solar Rotation Period : 1848



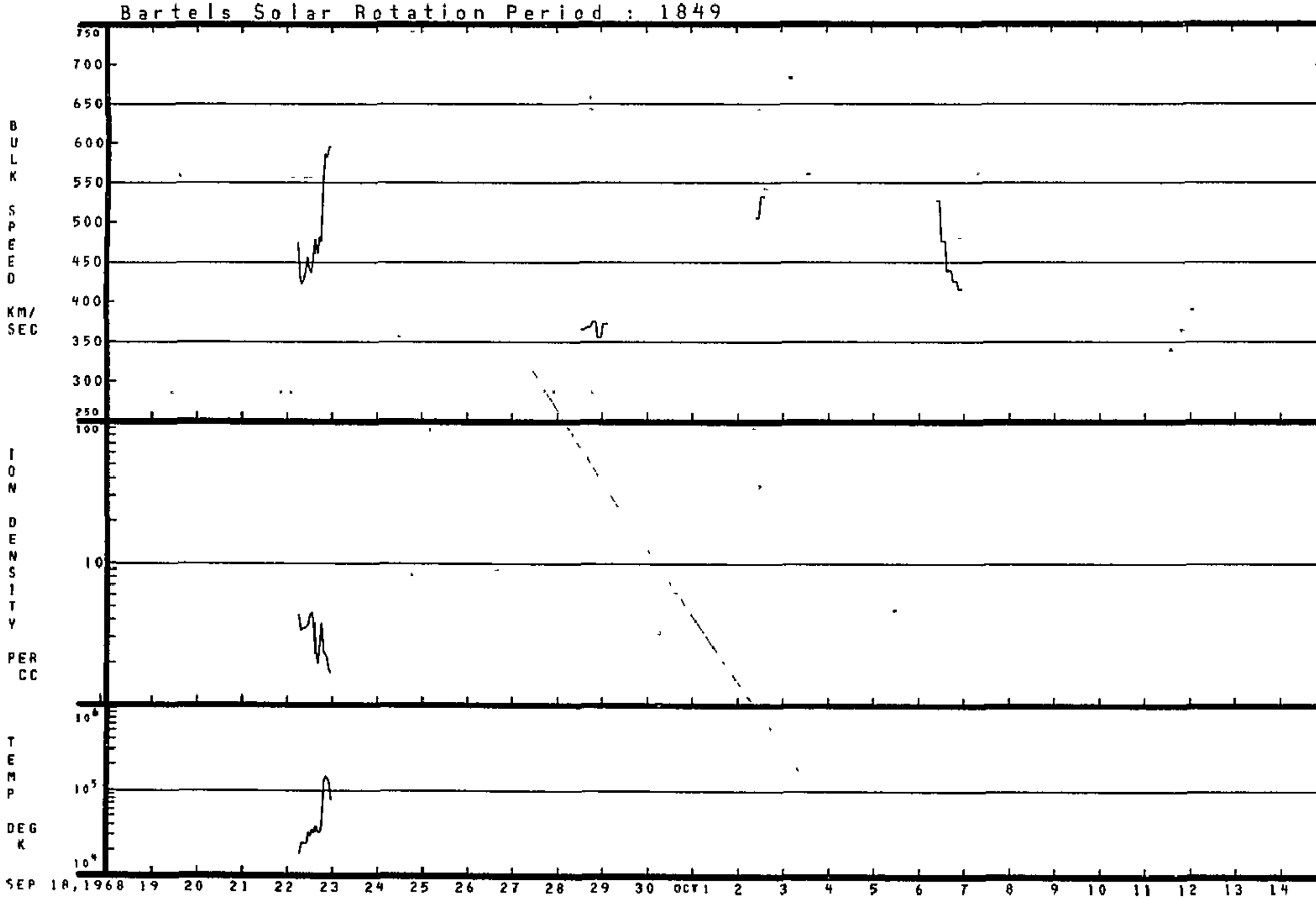
Bartels Solar Rotation Period : 1848

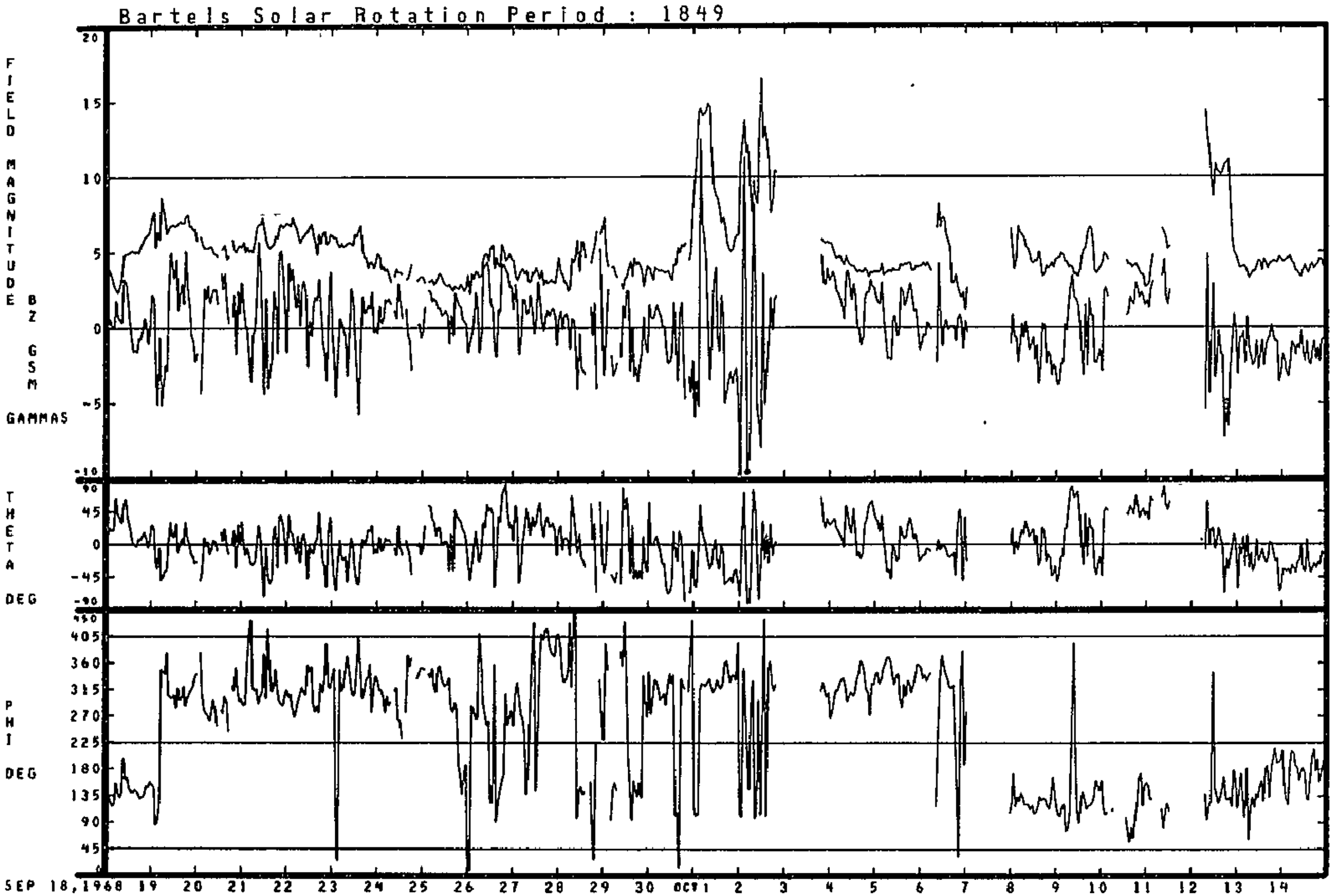


08/22/68 - 09/17/68

09/18/68 - 10/14/68

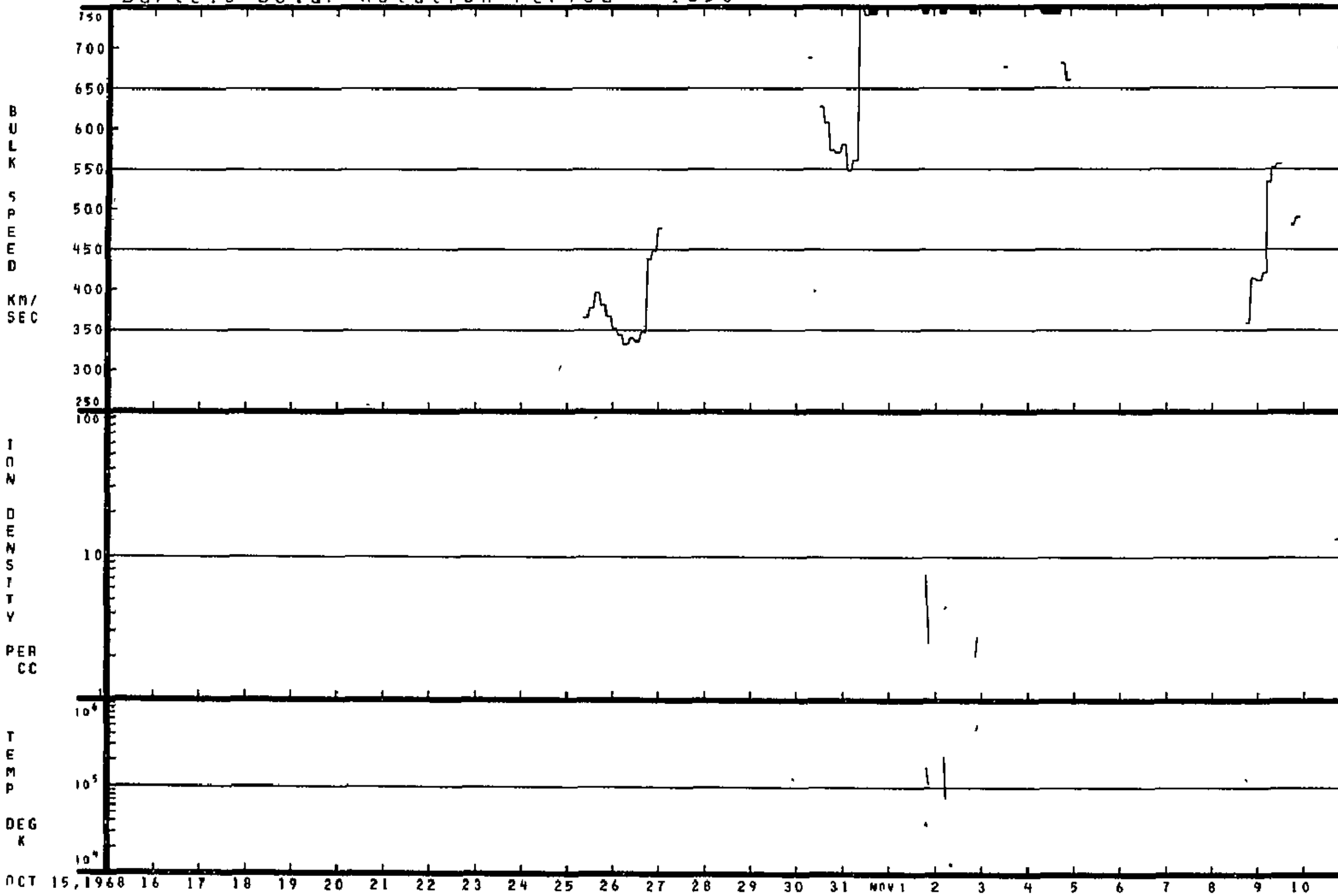
Bartels Solar Rotation Period : 1849



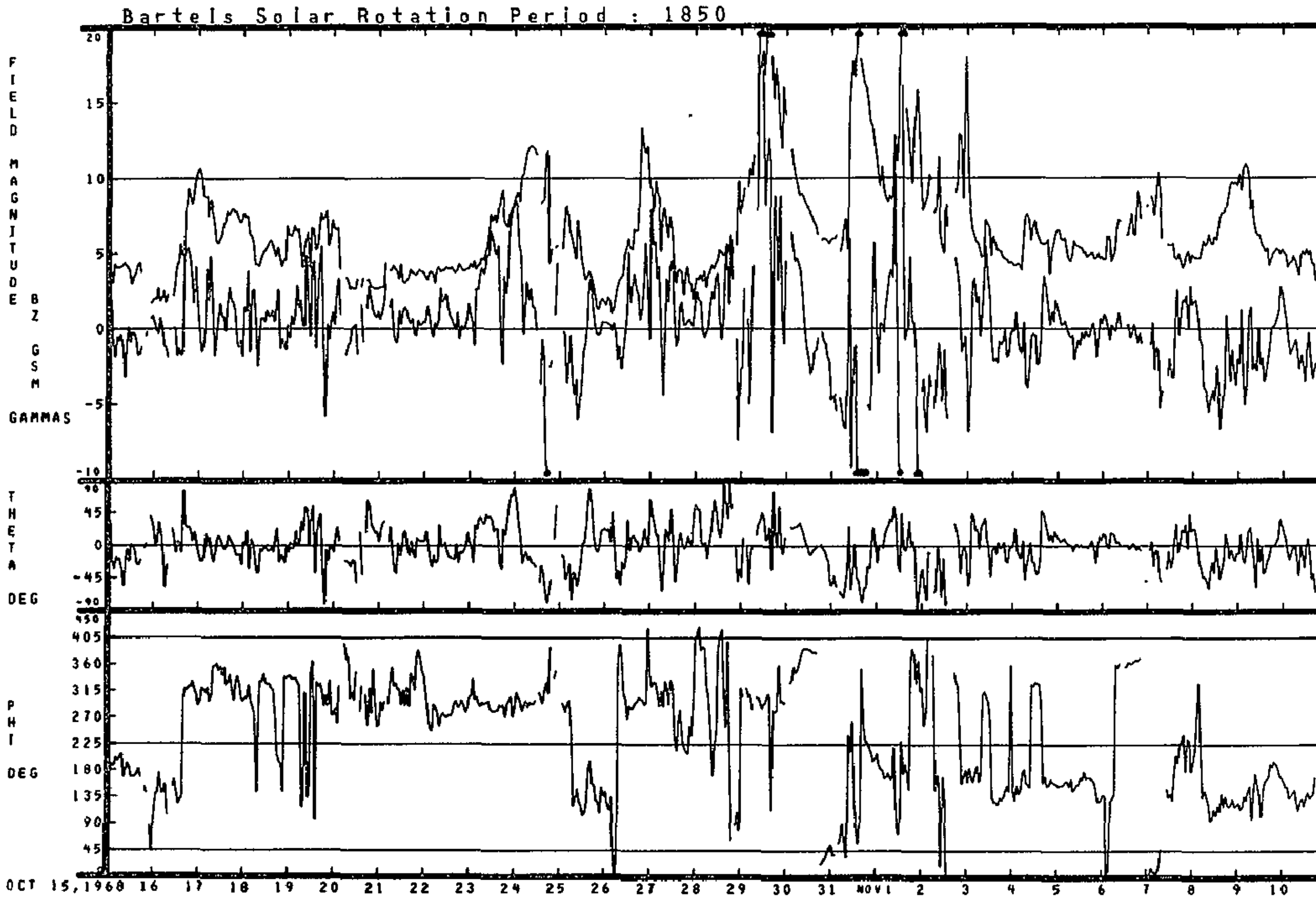


09/18/68 - 10/14/68

Bartels Solar Rotation Period · 1850



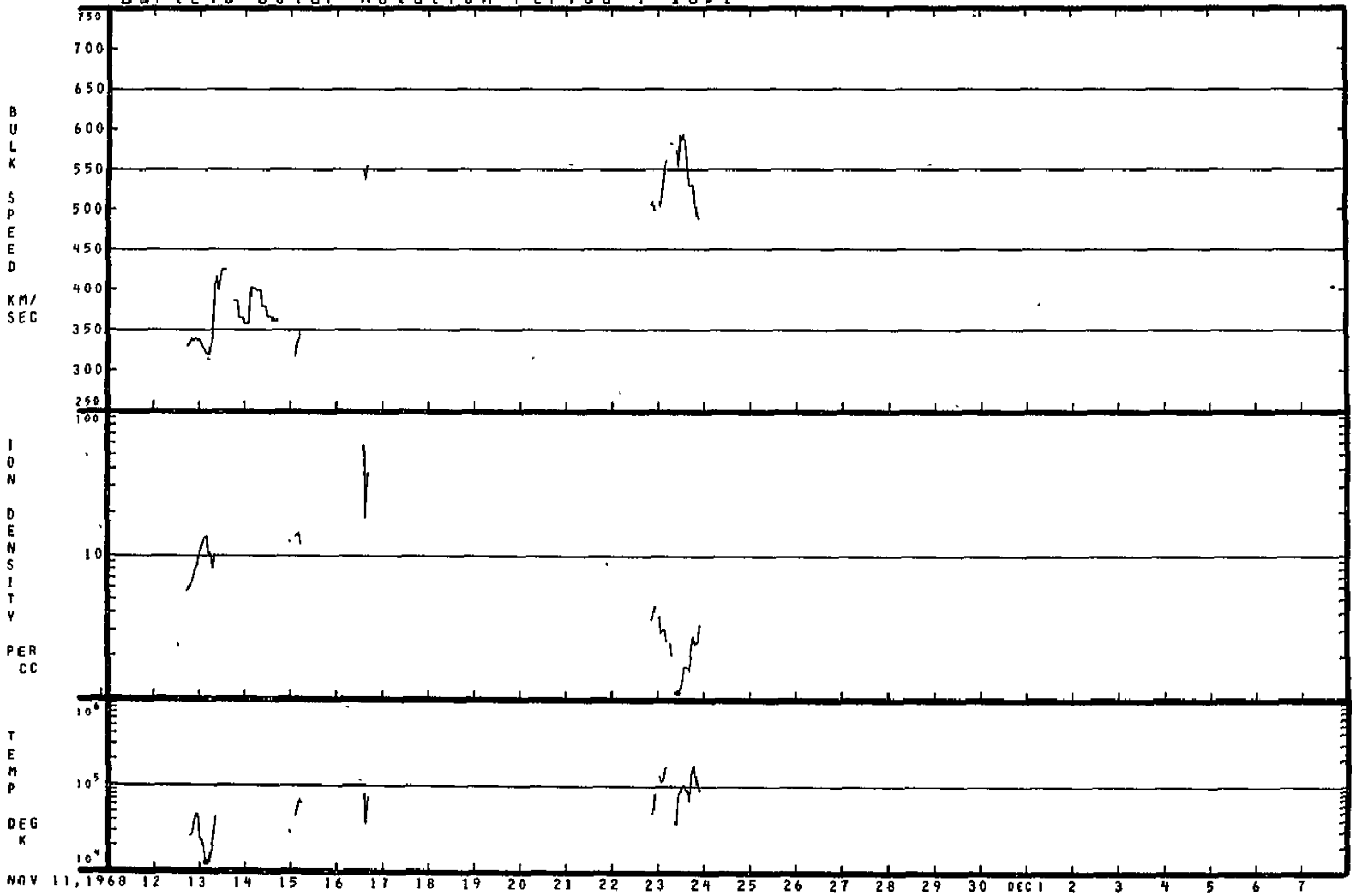
10/15/68 - 11/10/68.



10/15/68 - 11/10/68

11/11/68 - 12/07/68

Bartels Solar Rotation Period : 1851



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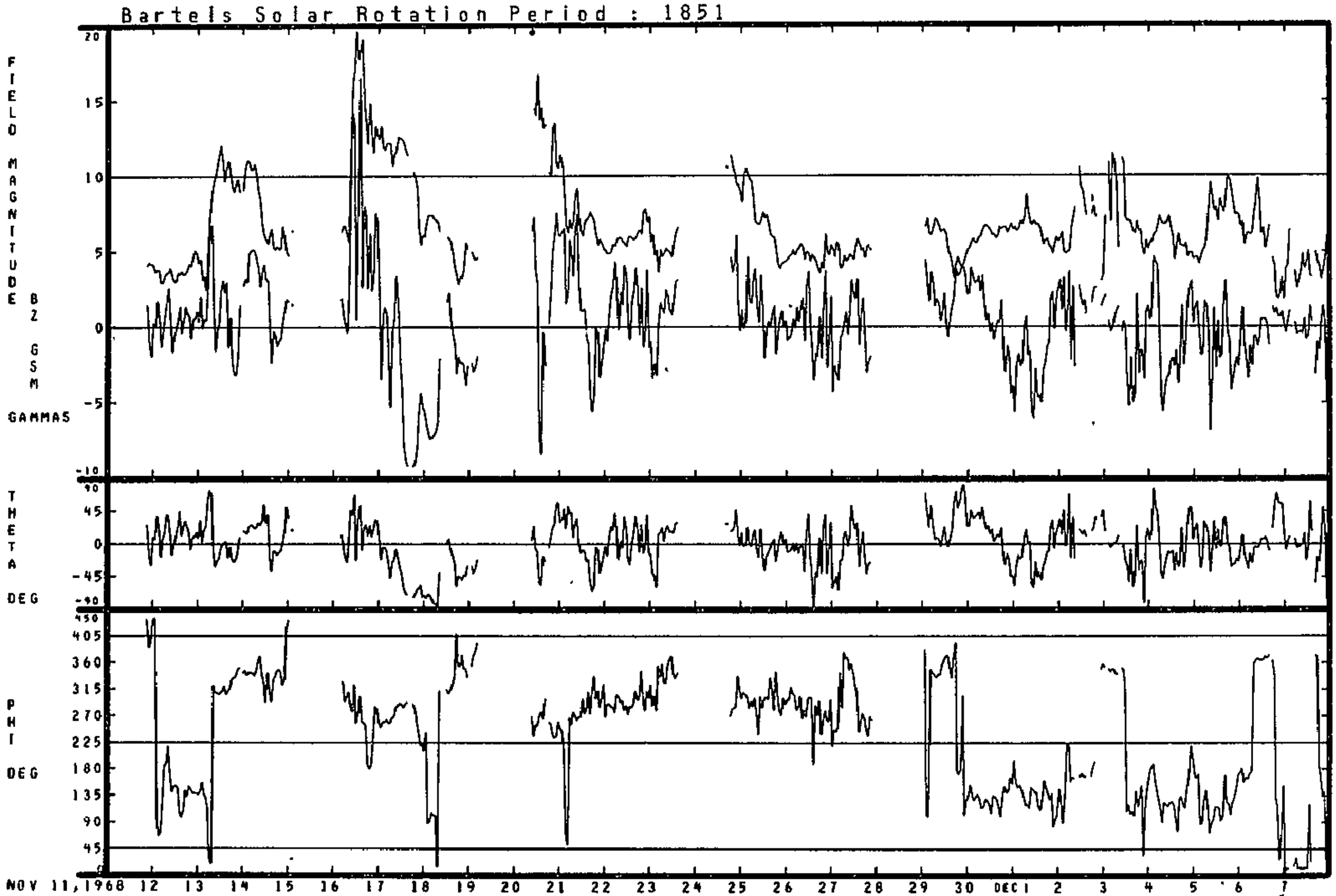
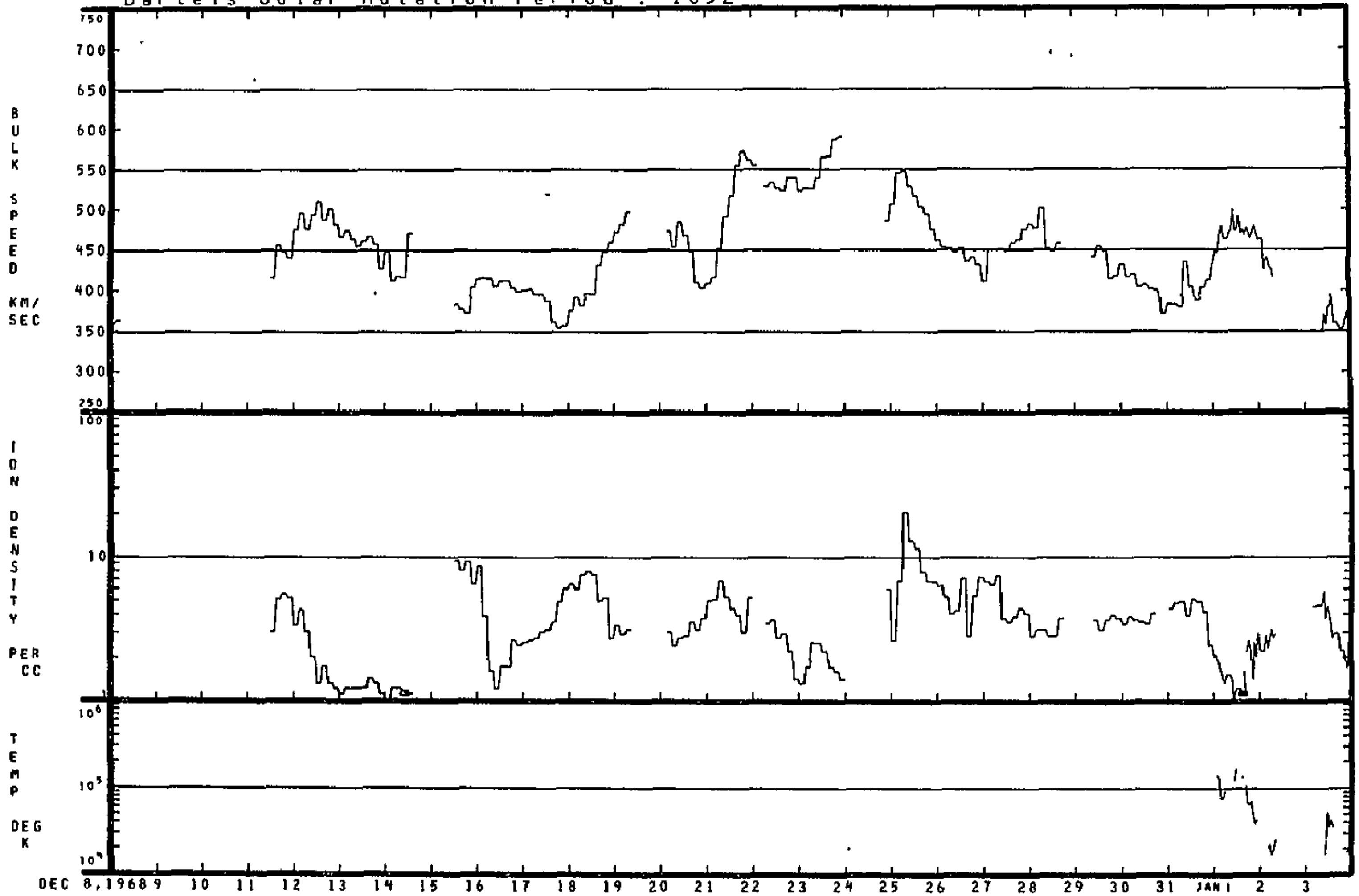
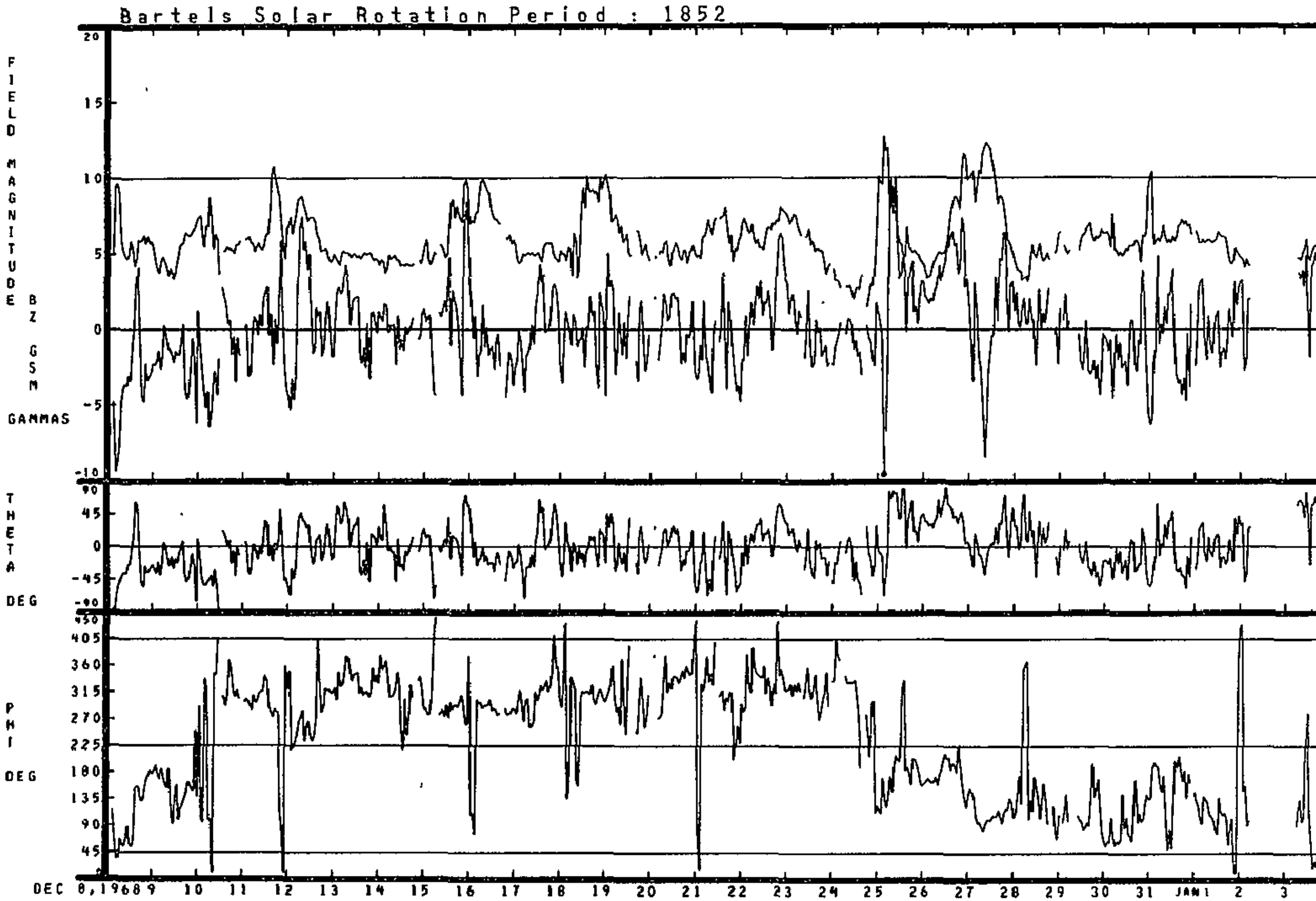


11/11/68 - 12/07/68

12/08/68 - 01/03/69

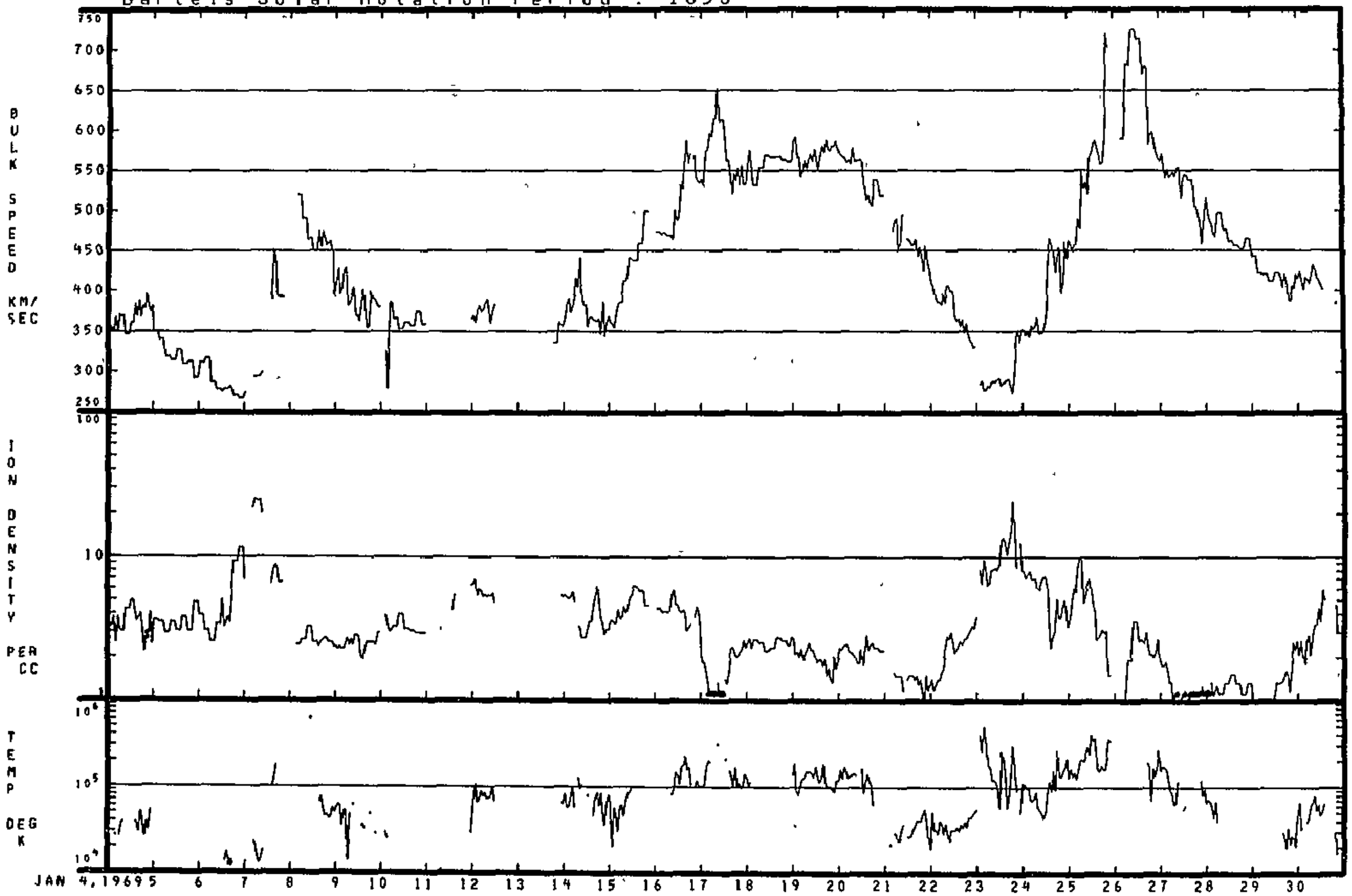
Bartels Solar Rotation Period : 1852





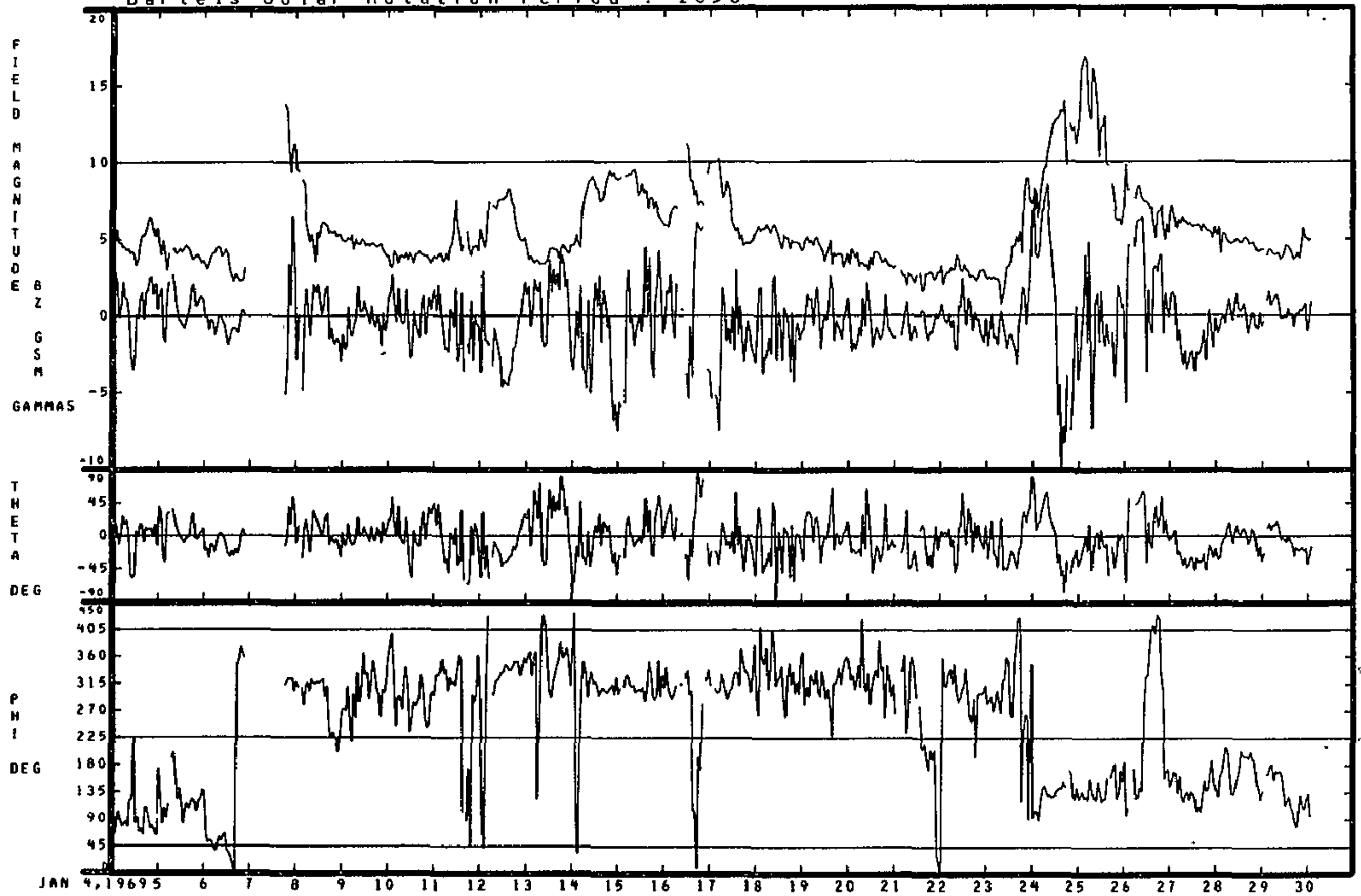
12/08/68 - 01/03/69

Bartels Solar Rotation Period : 1853



01/04/69 - 01/30/69

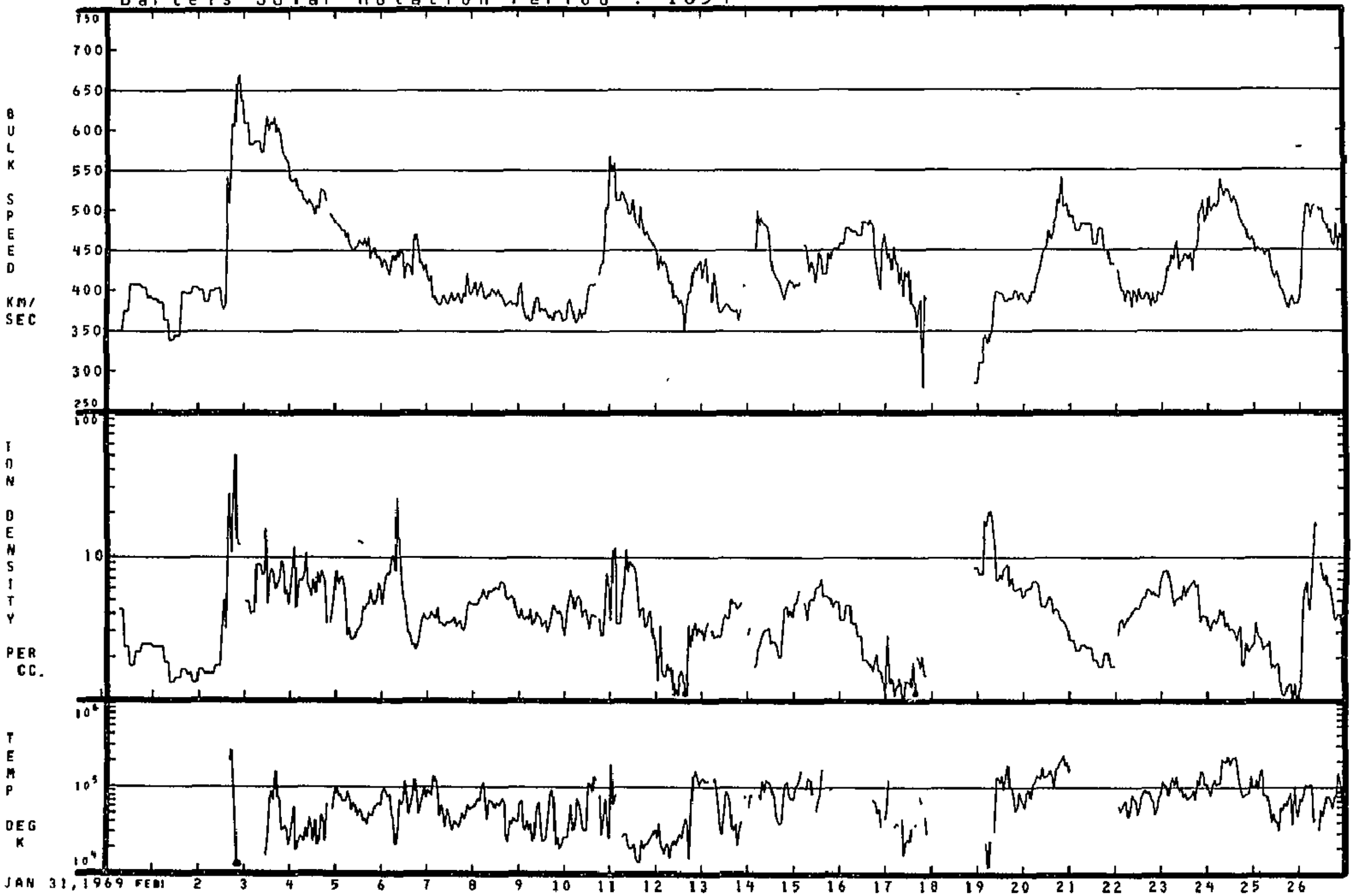
Bartels Solar Rotation Period : 1853

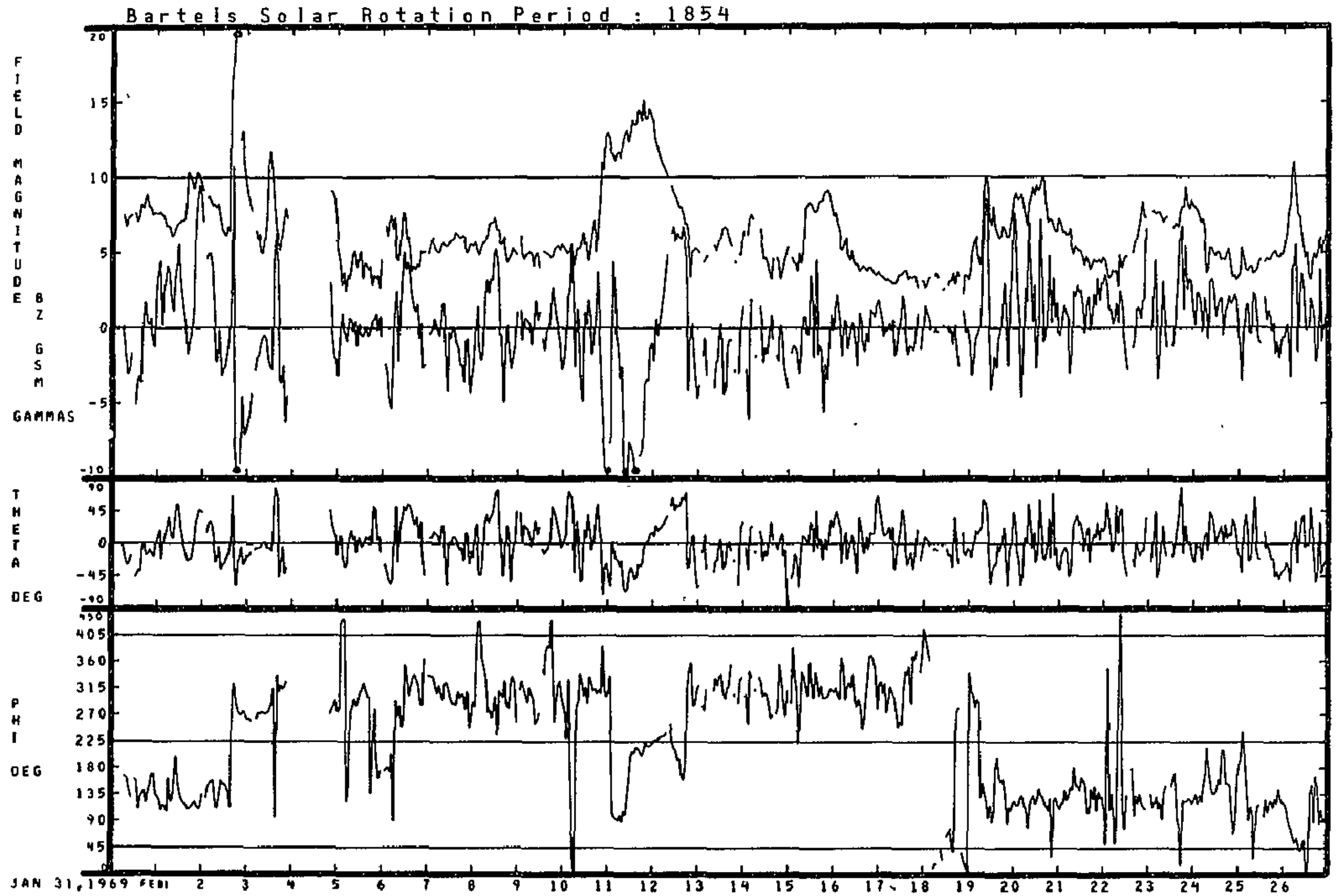


01/04/69 - 01/30/69

01/31/69 - 02/26/69

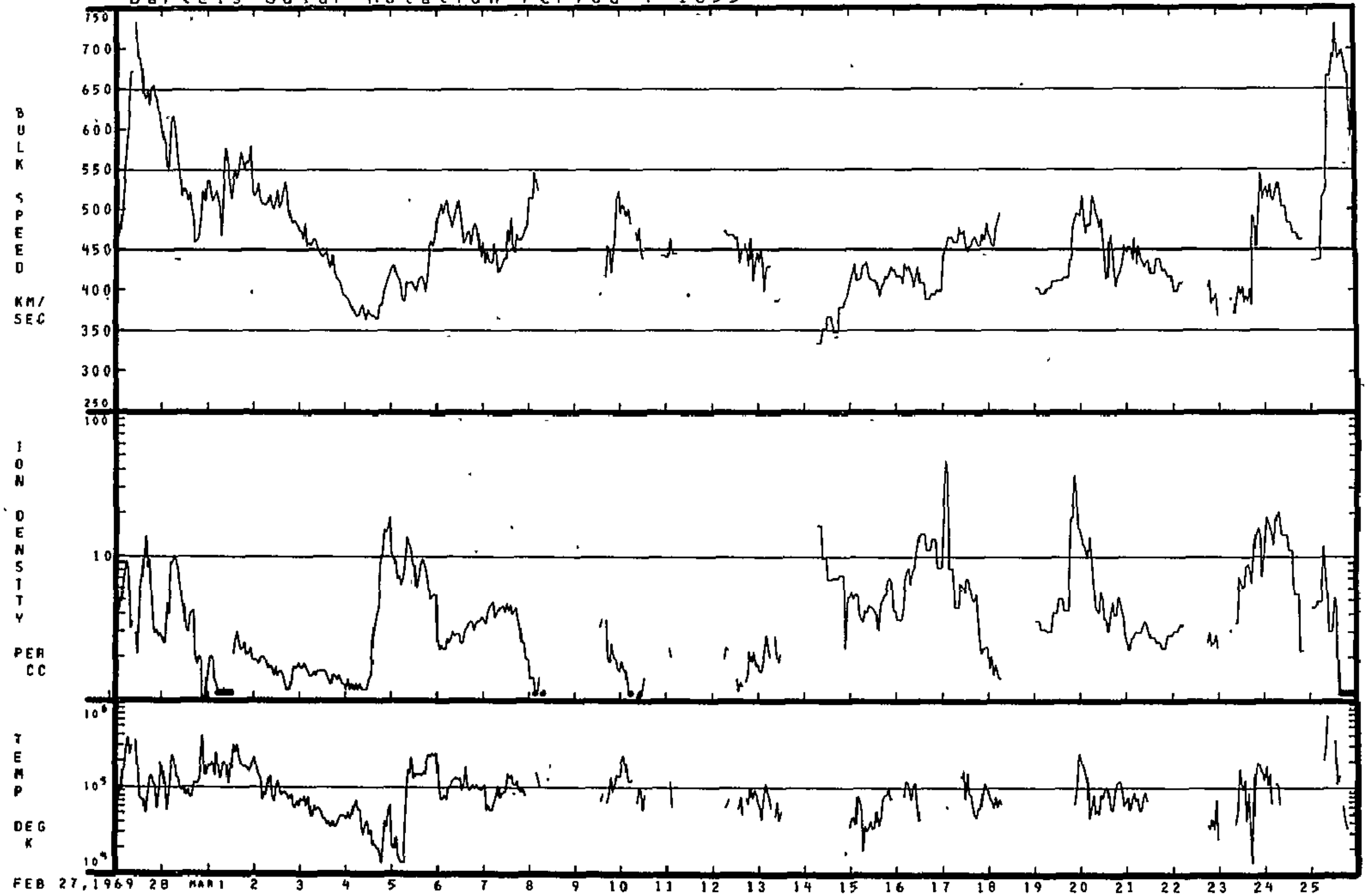
Bartels Solar Rotation Period : 1854



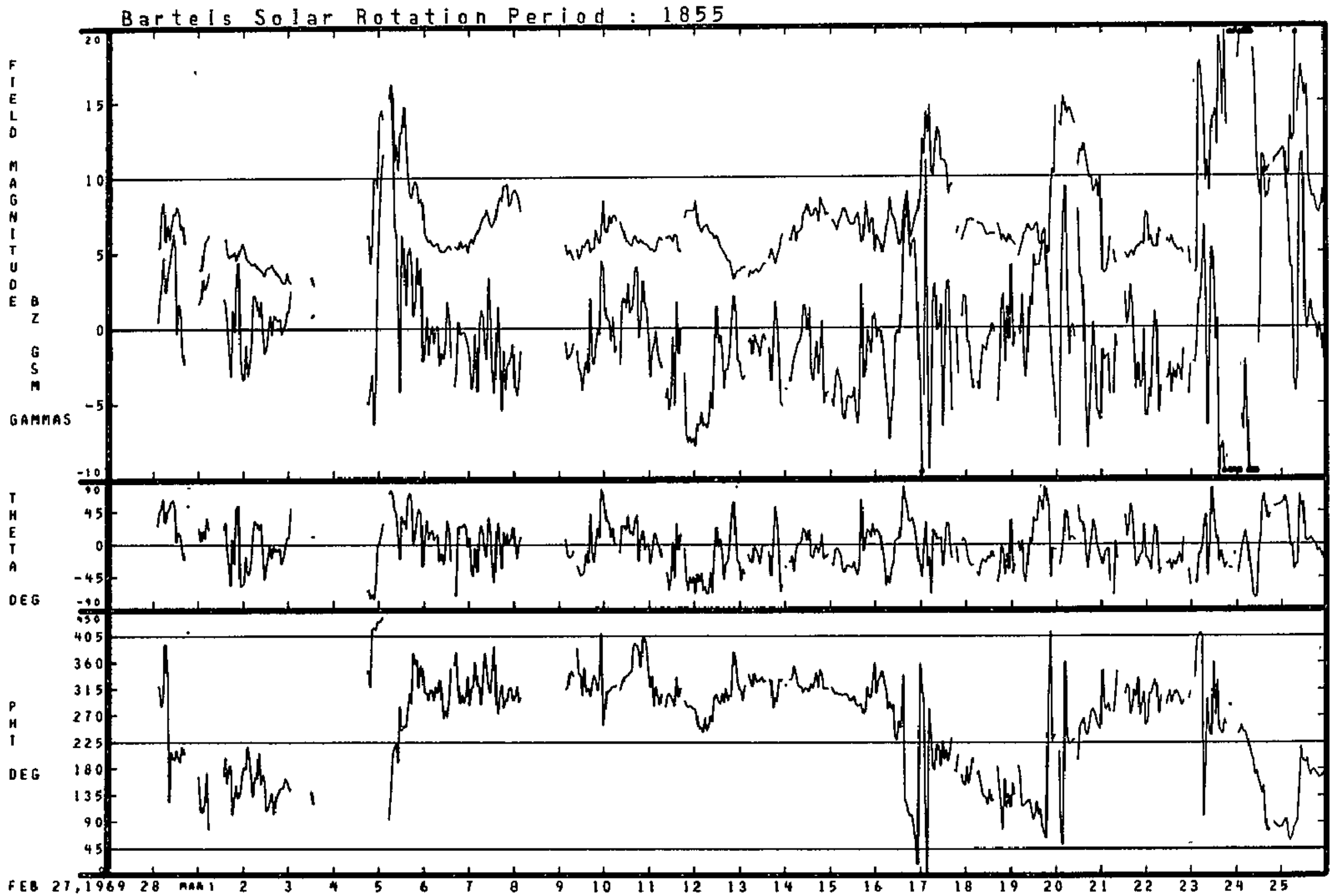


01/31/69 - 02/26/69

Bartels Solar Rotation Period : 1855

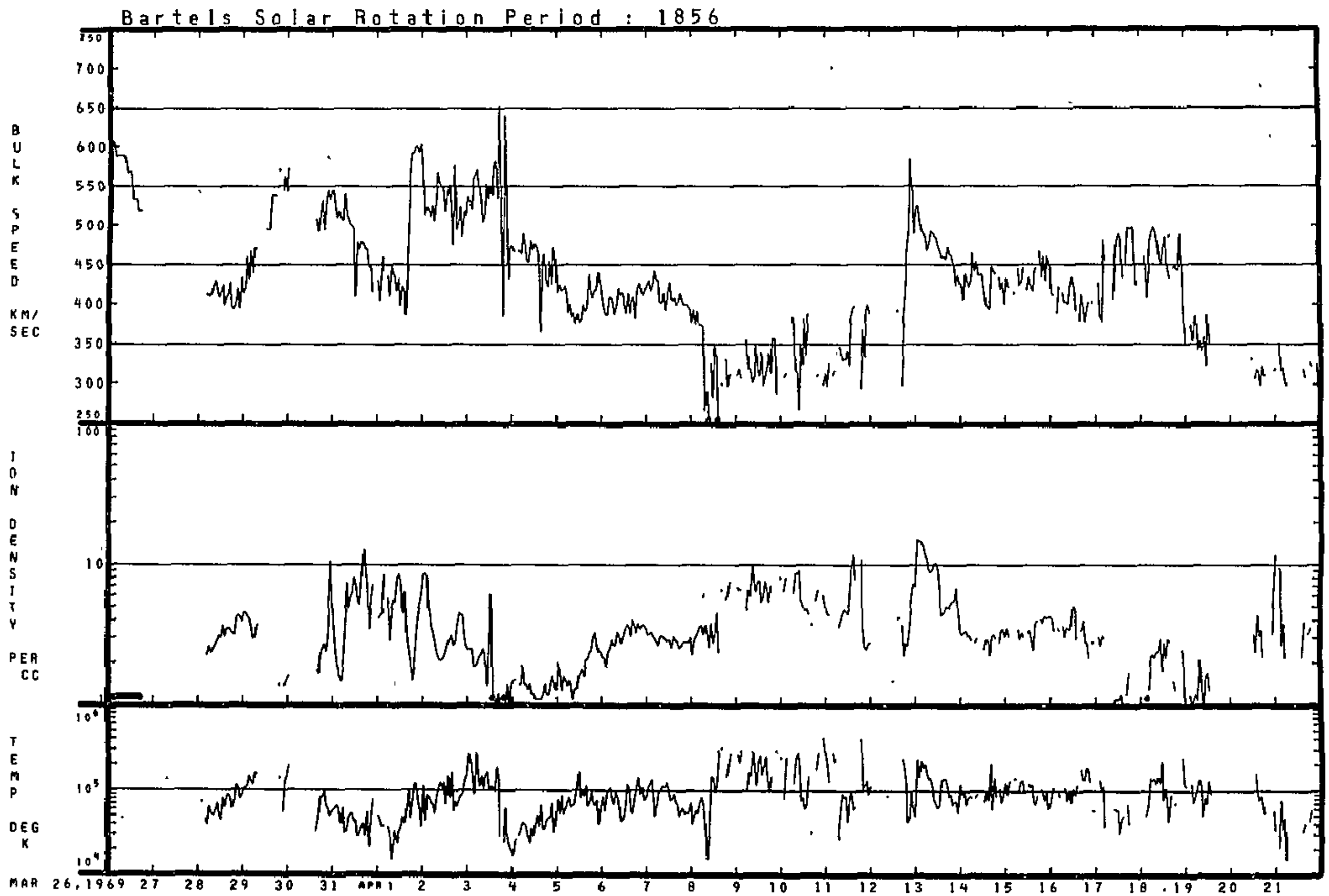


02/27/69 - 03/25/69



02/27/69 - 03/25/69

03/26/69 - 04/21/69



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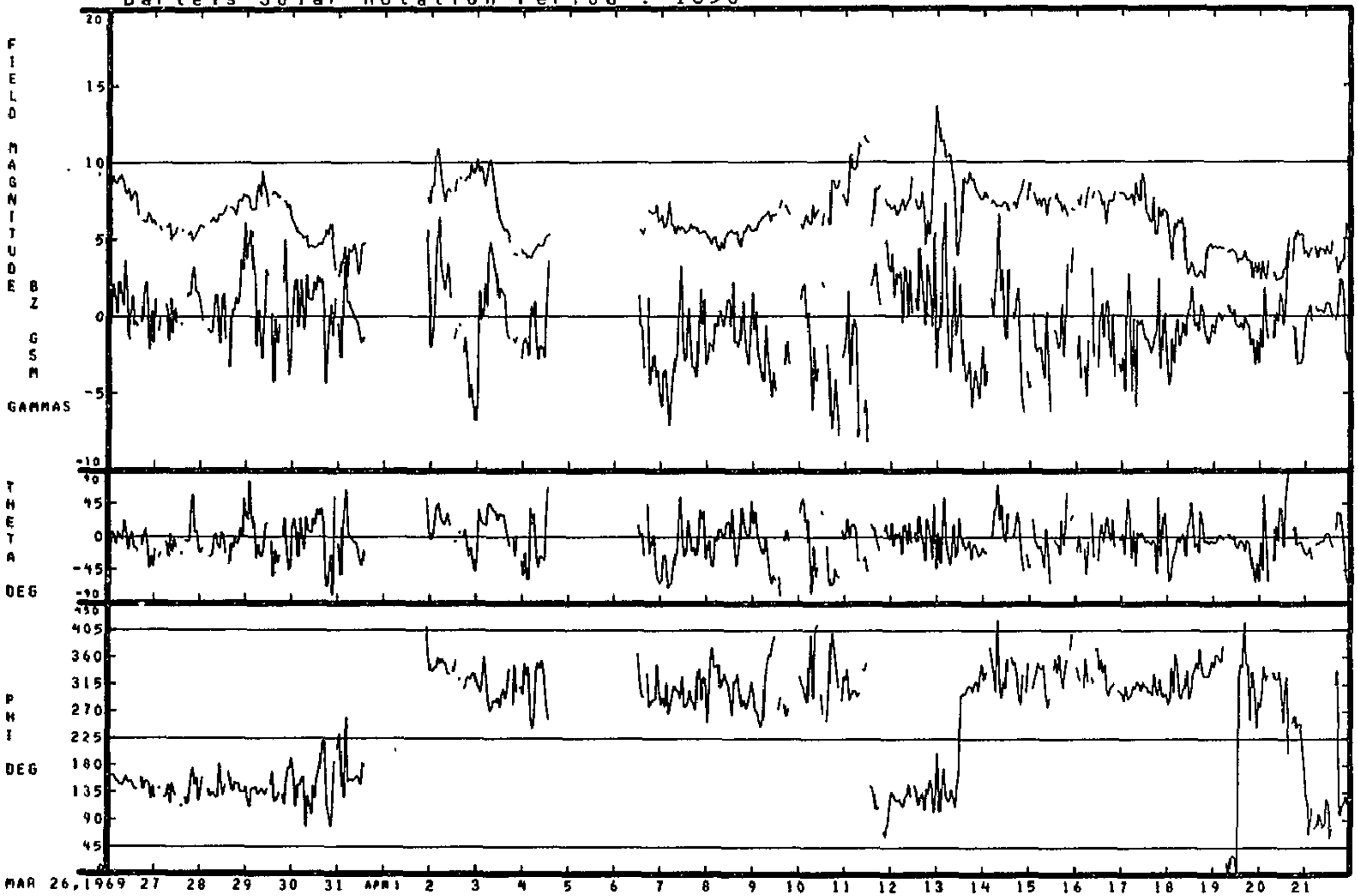
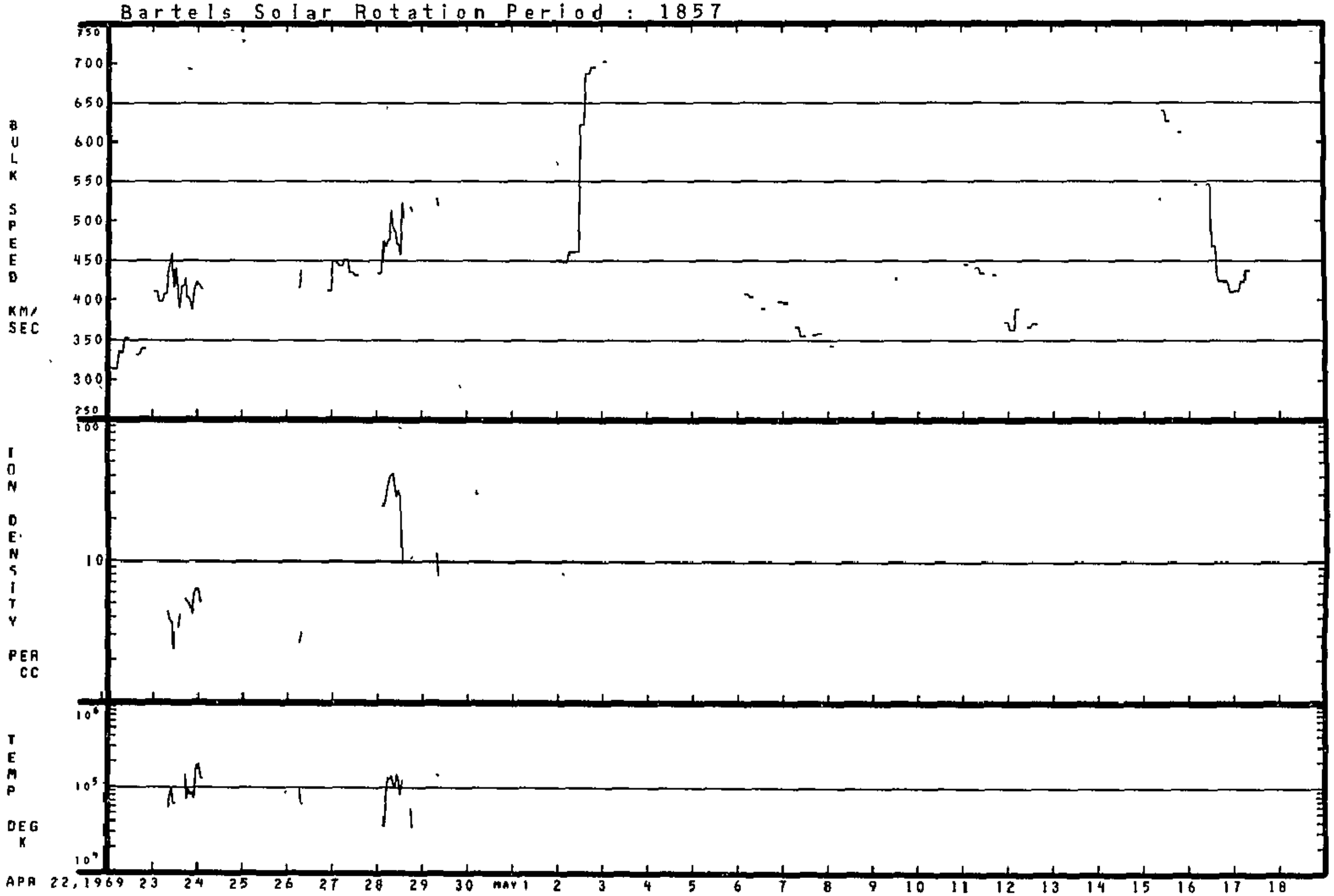
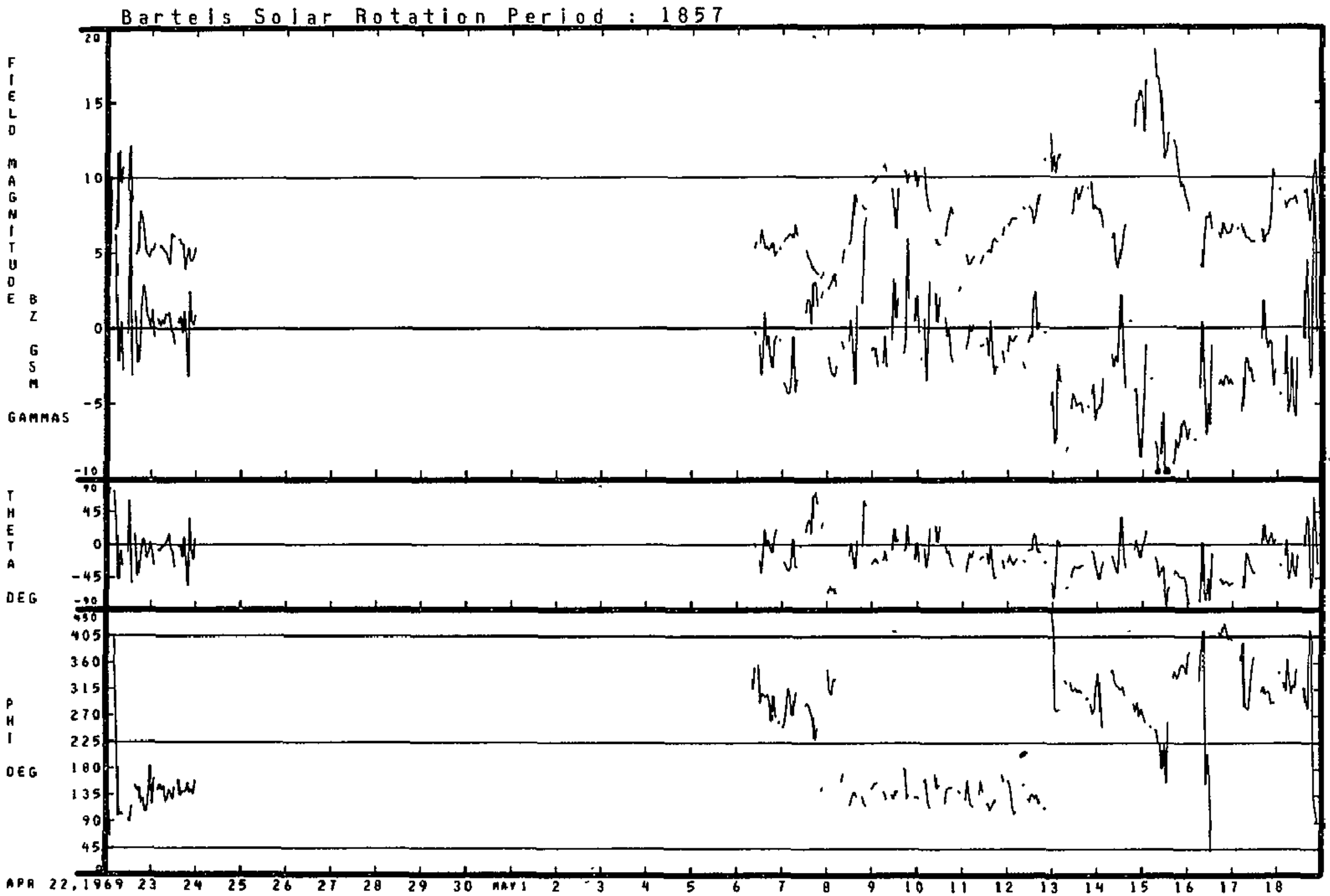


03/26/69 - 04/21/69

Bartels Solar Rotation Period : 1857

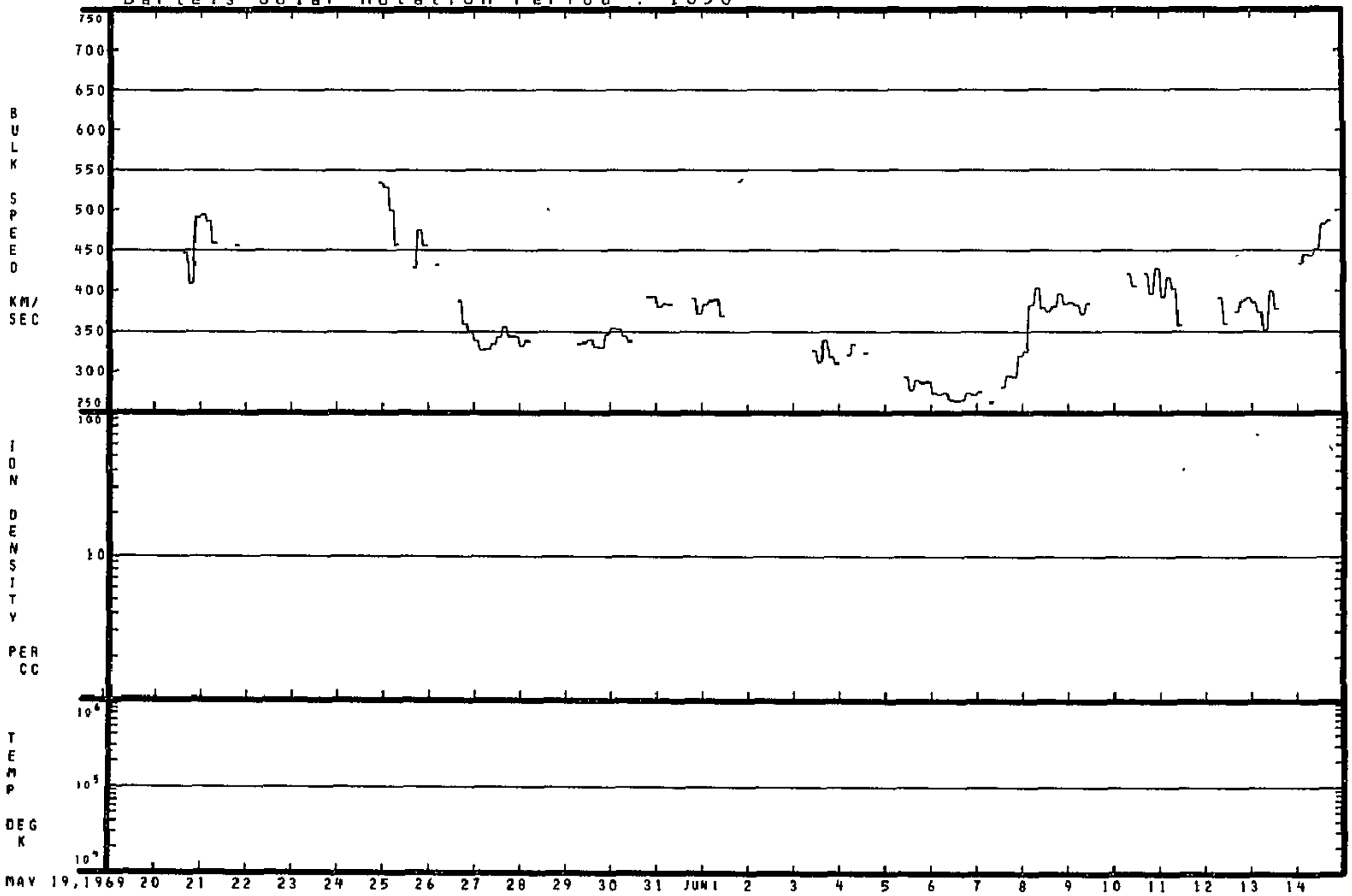


04/22/69 - 05/18/69

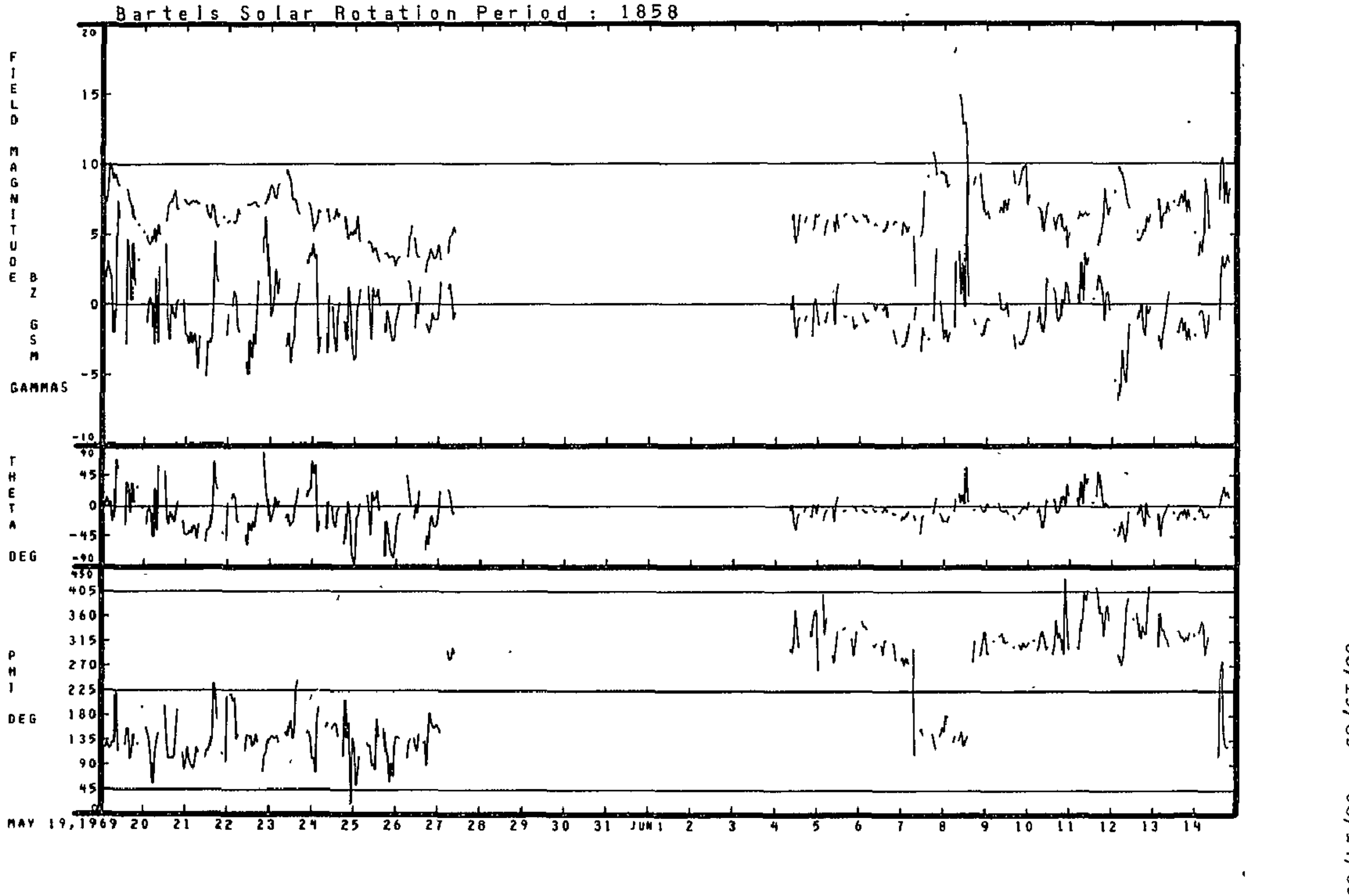


04/22/69 - 05/18/69

Bartels Solar Rotation Period : 1858

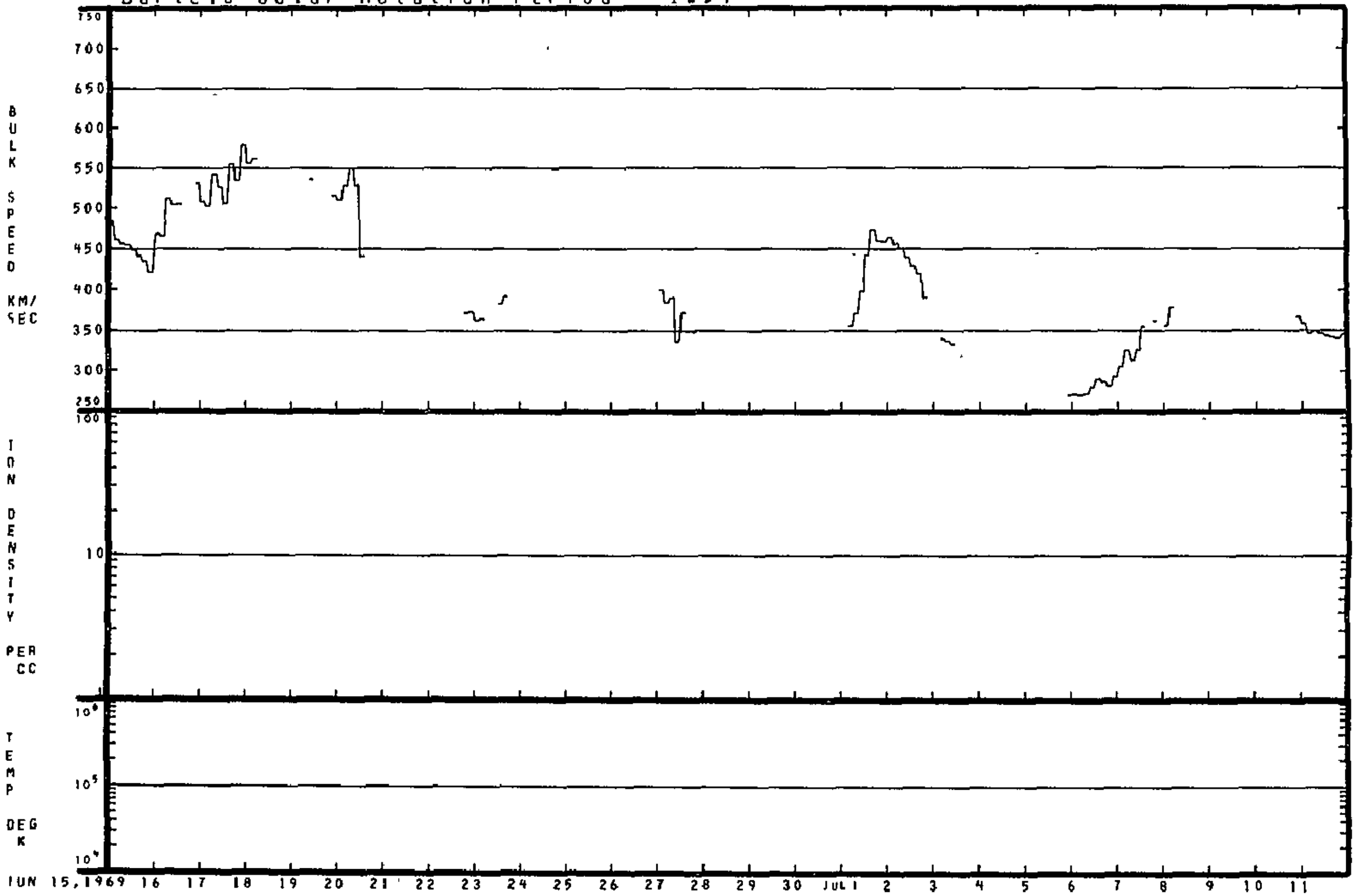


05/19/69 - 06/14/69

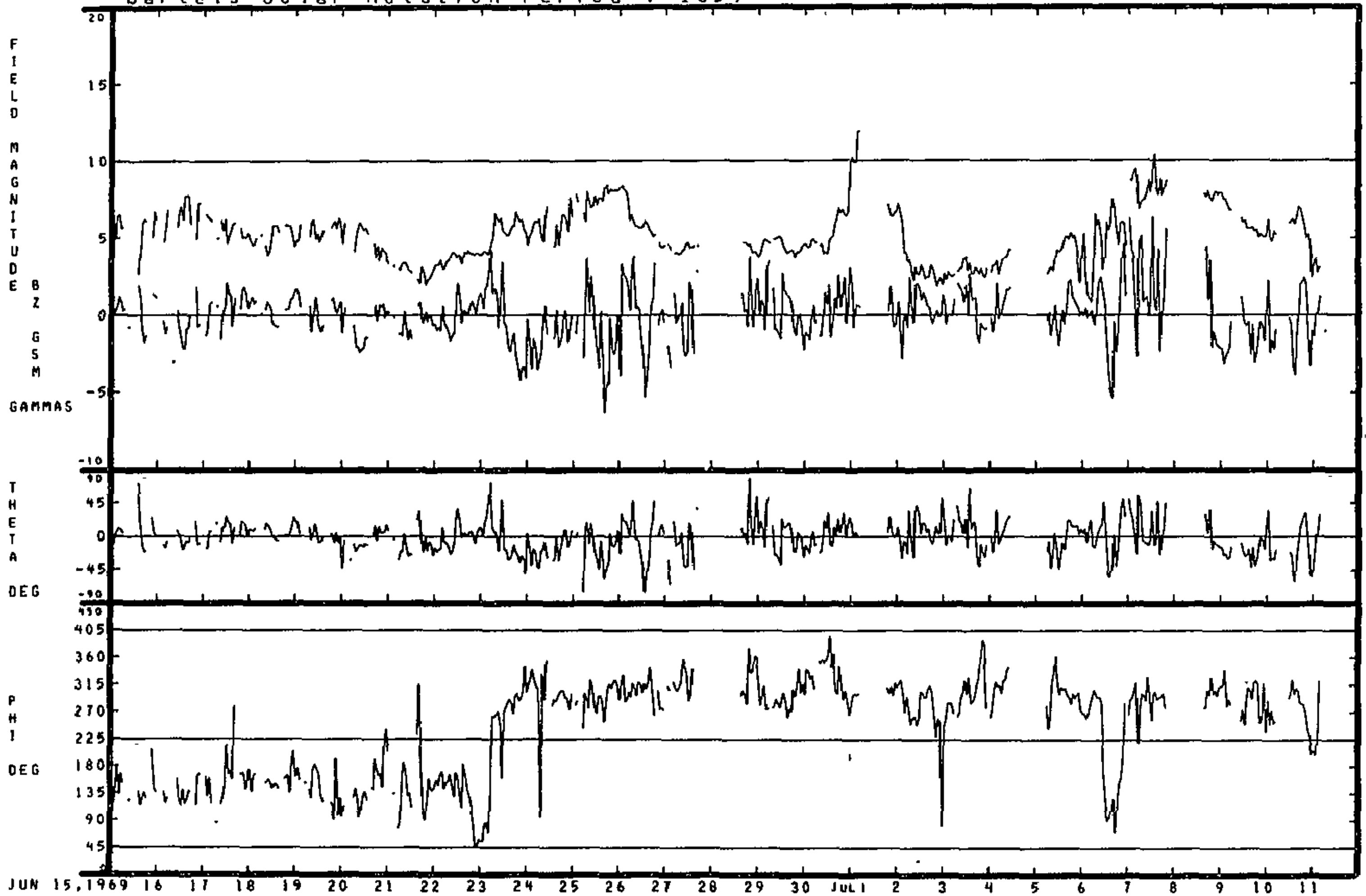

Bartels Solar Rotation Period : 1858
FIELD MAGNITUDE
BZ GSM
GAMMAS
THETA DEG
PHI DEG
MAY 19,1969 20 21 22 23 24 25 26 27 28 29 30 31 JUN 1 2 3 4 5 6 7 8 9 10 11 12 13 14
05/19/69 - 06/14/69

06/15/69 - 07/11/69

Bartels Solar Rotation Period · 1859



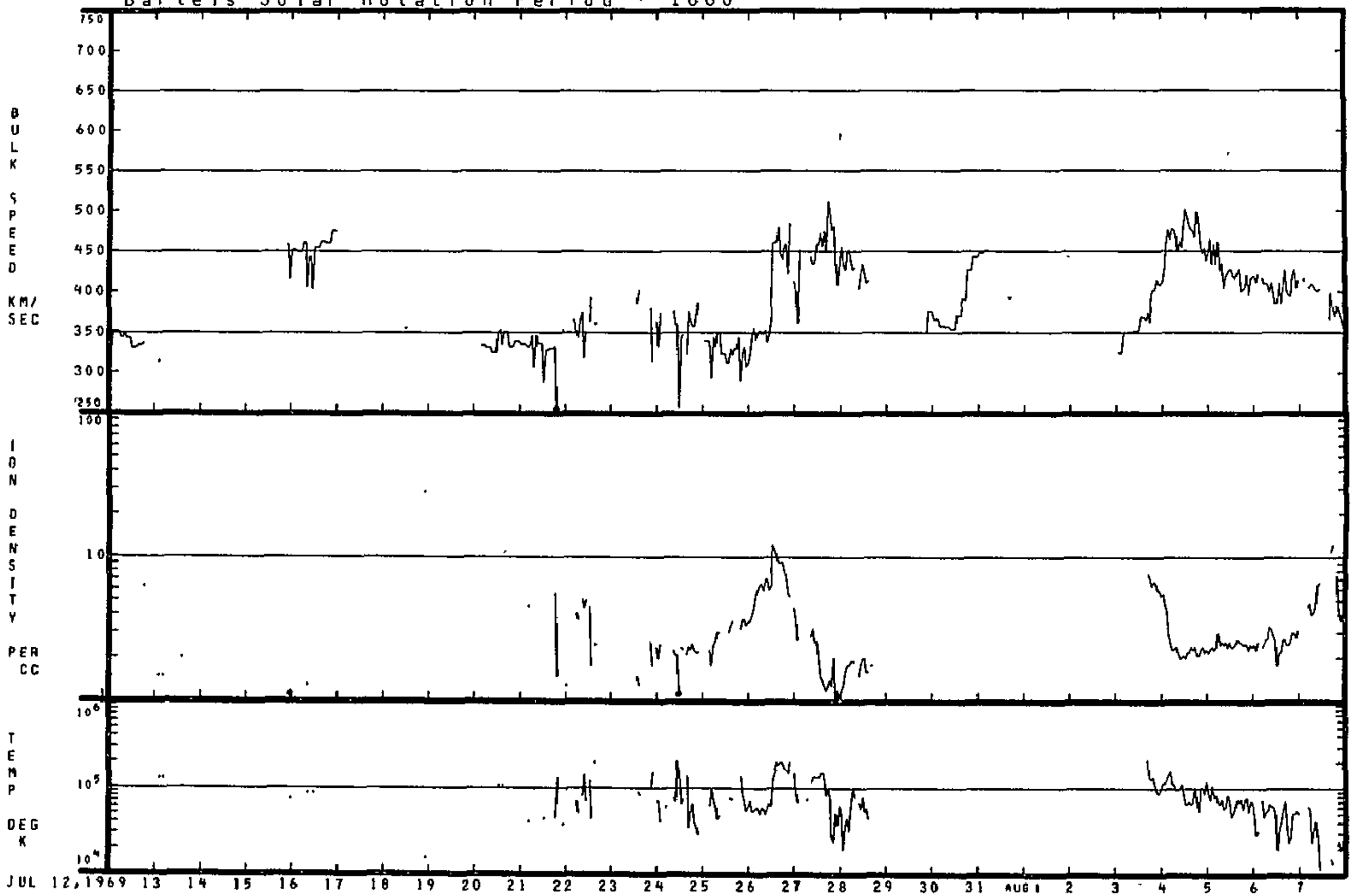
Bartels Solar Rotation Period : 1859



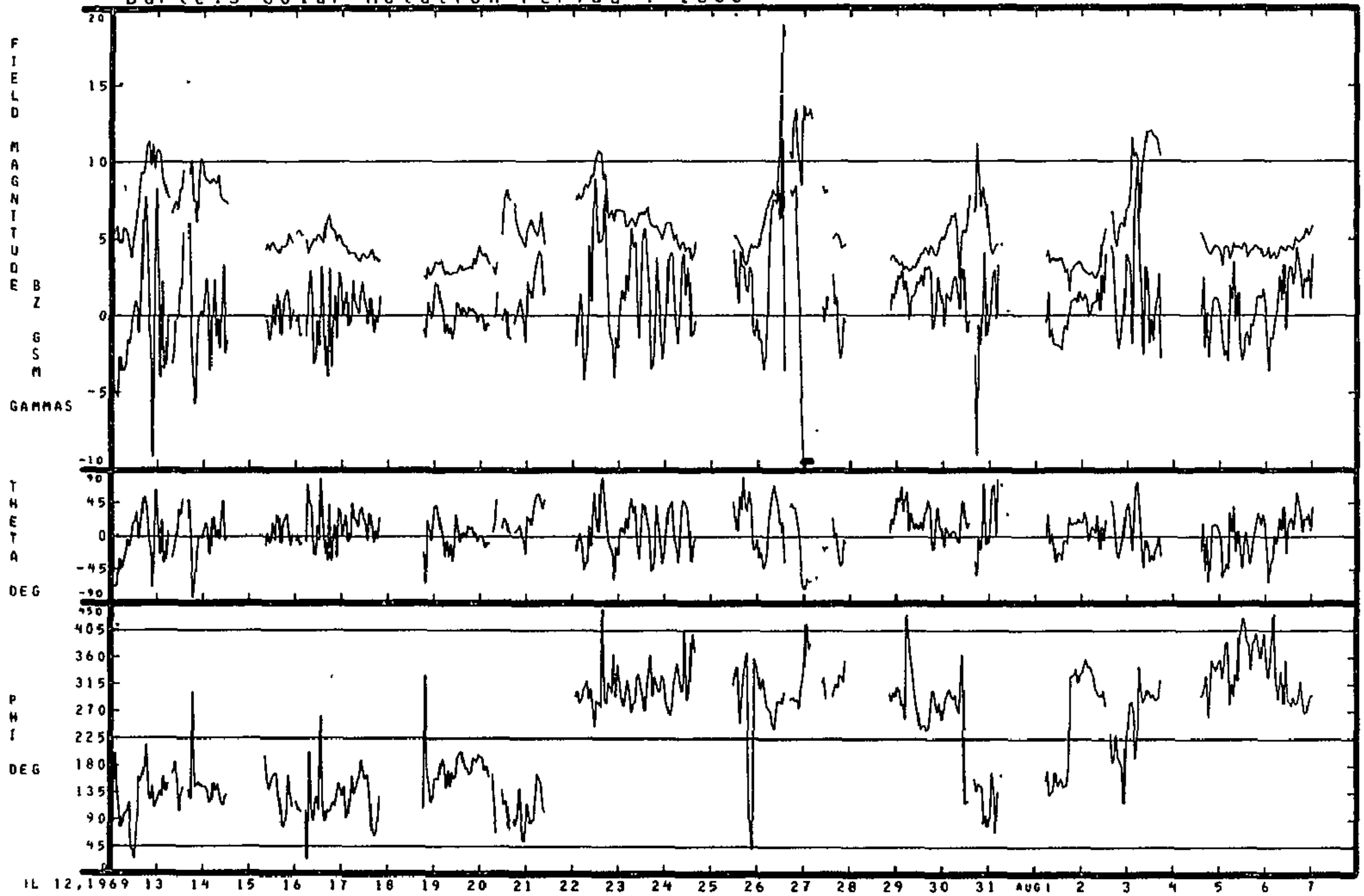
06/15/69 - 07/11/69

07/12/69 - 08/07/69

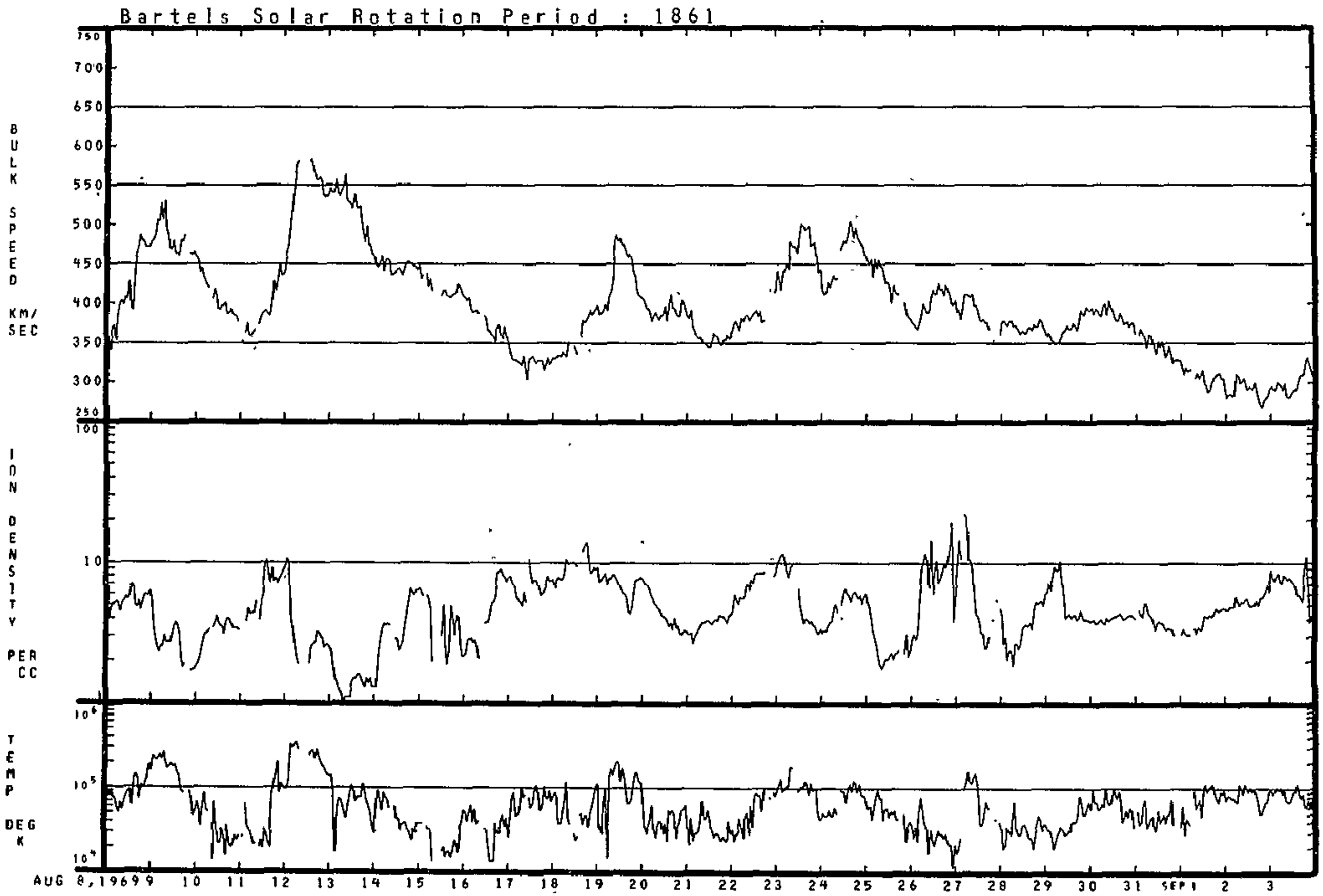
Bartels Solar Rotation Period · 1860



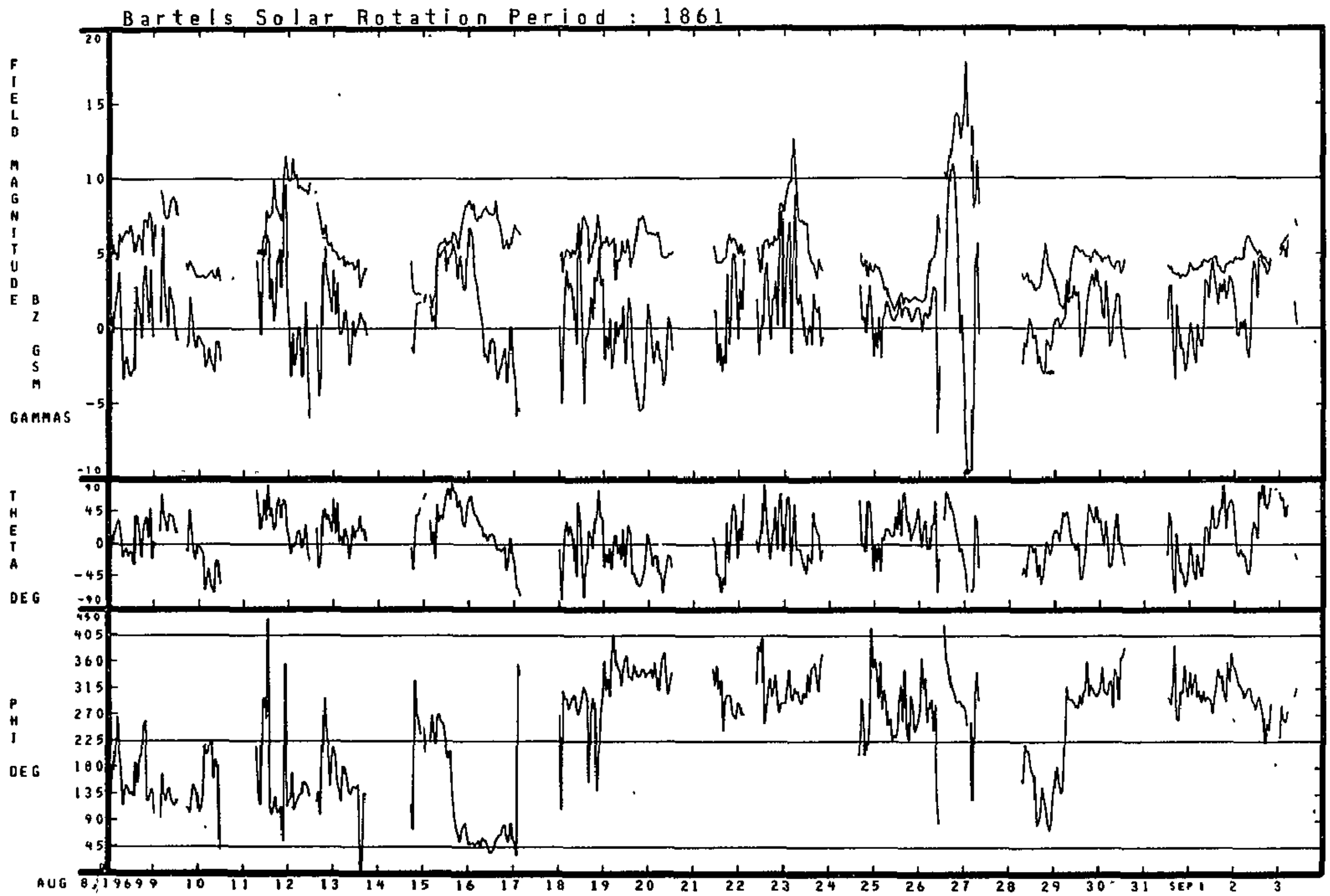
Bartels Solar Rotation Period : 1860



07/12/69 - 08/07/69

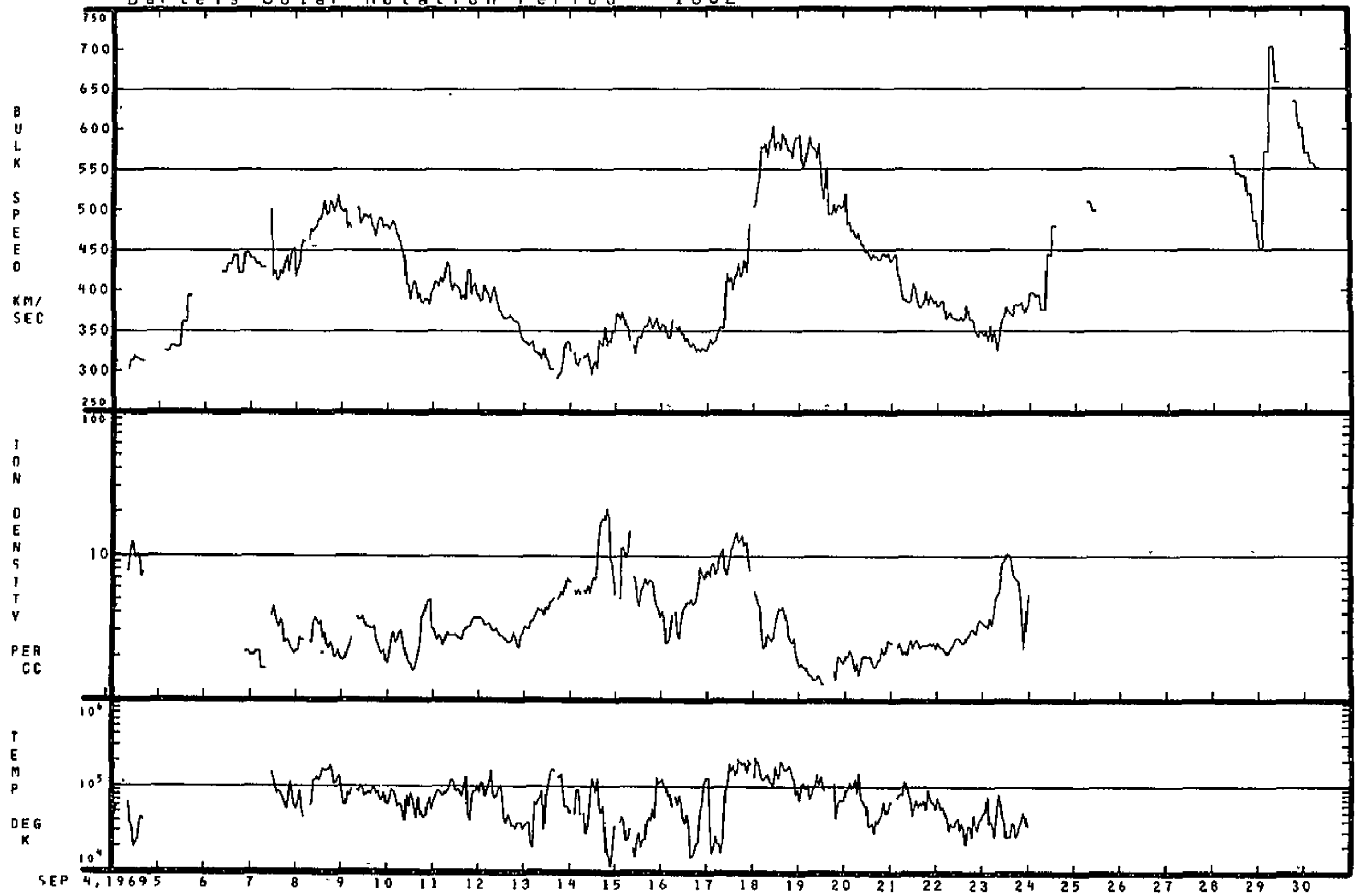


08/08/69 - 09/03/69

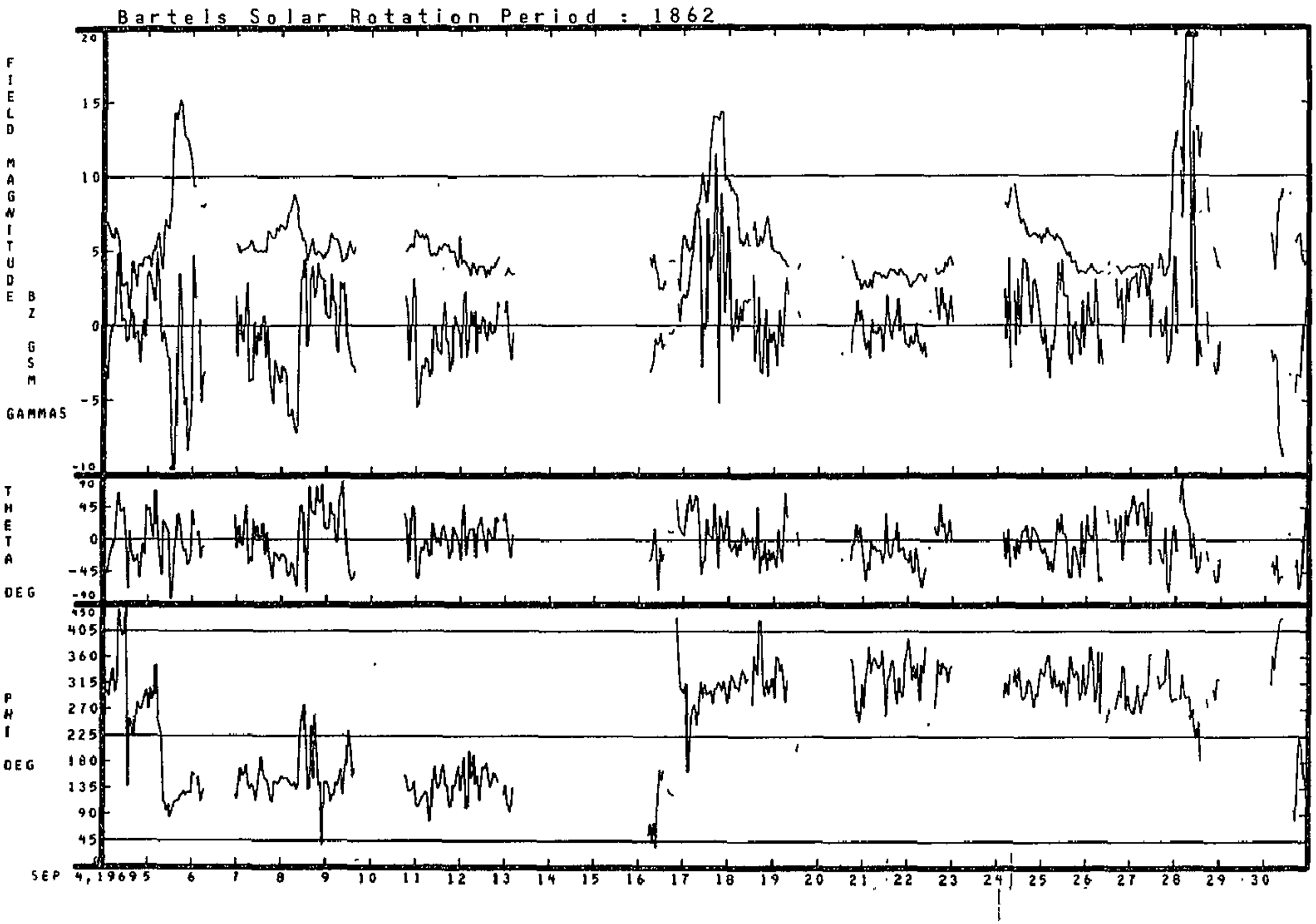


08/08/69 - 09/03/69

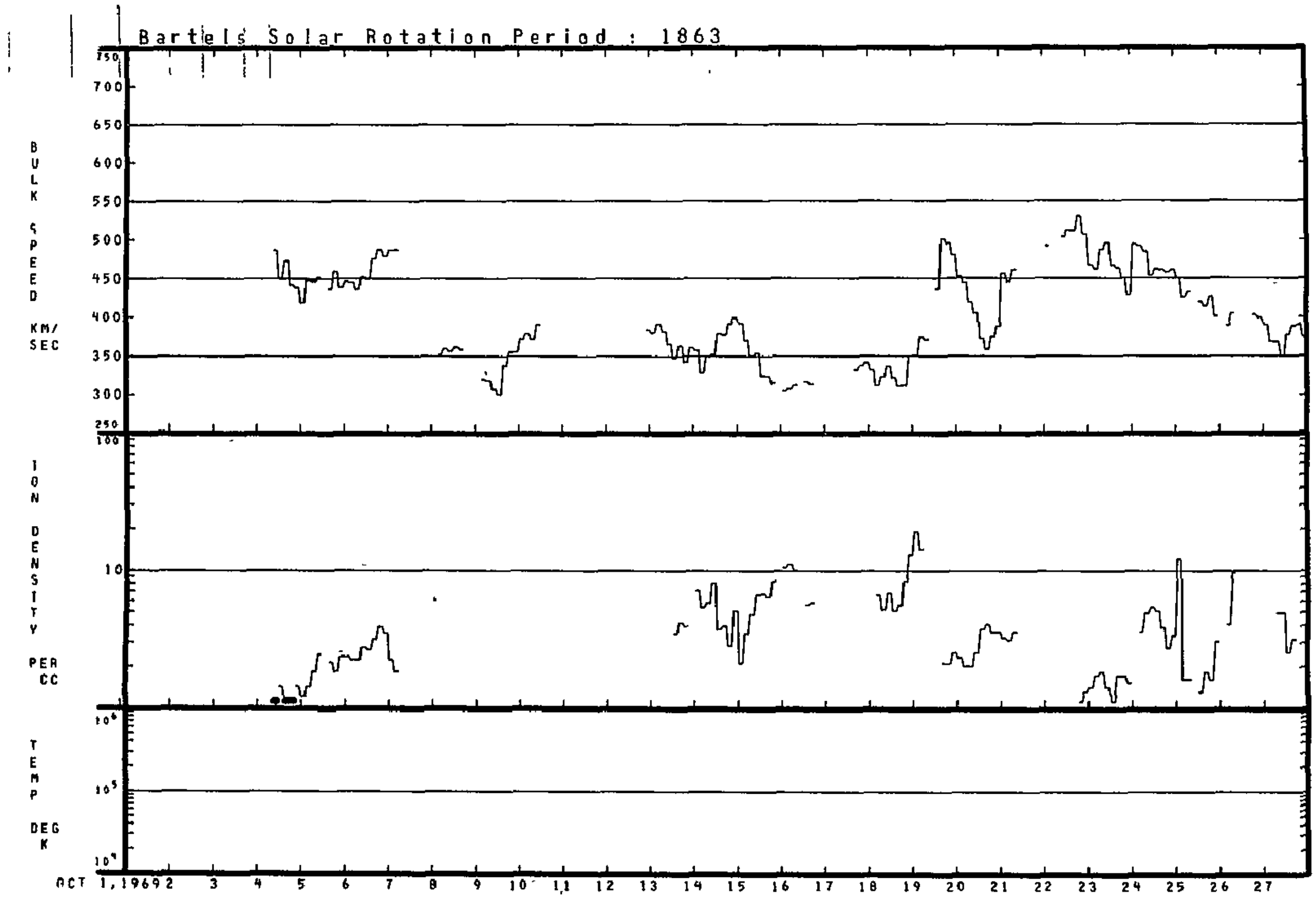
Bartels Solar Rotation Period · 1862



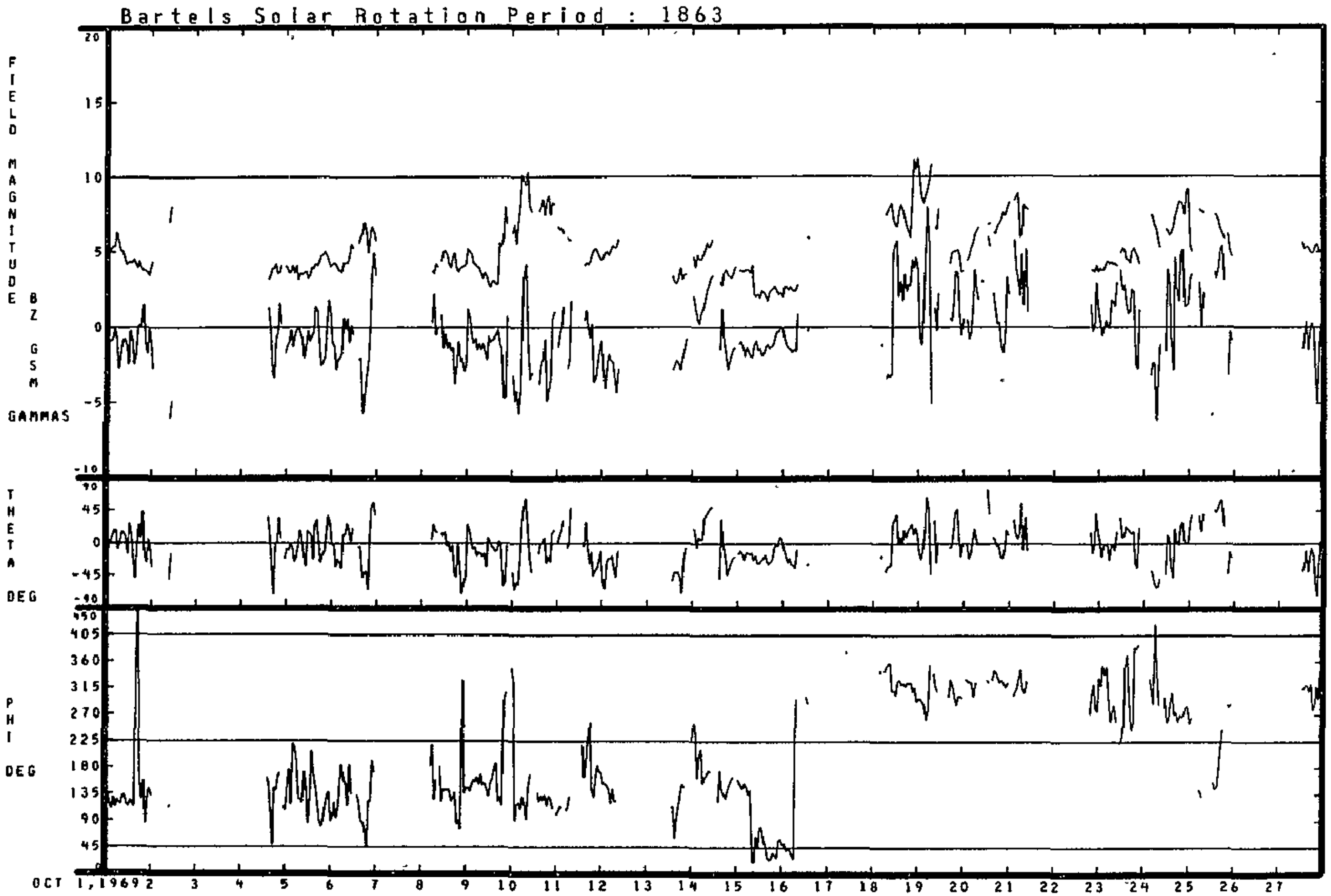
09/04/69 - 09/30/69



09/04/69 - 09/30/69



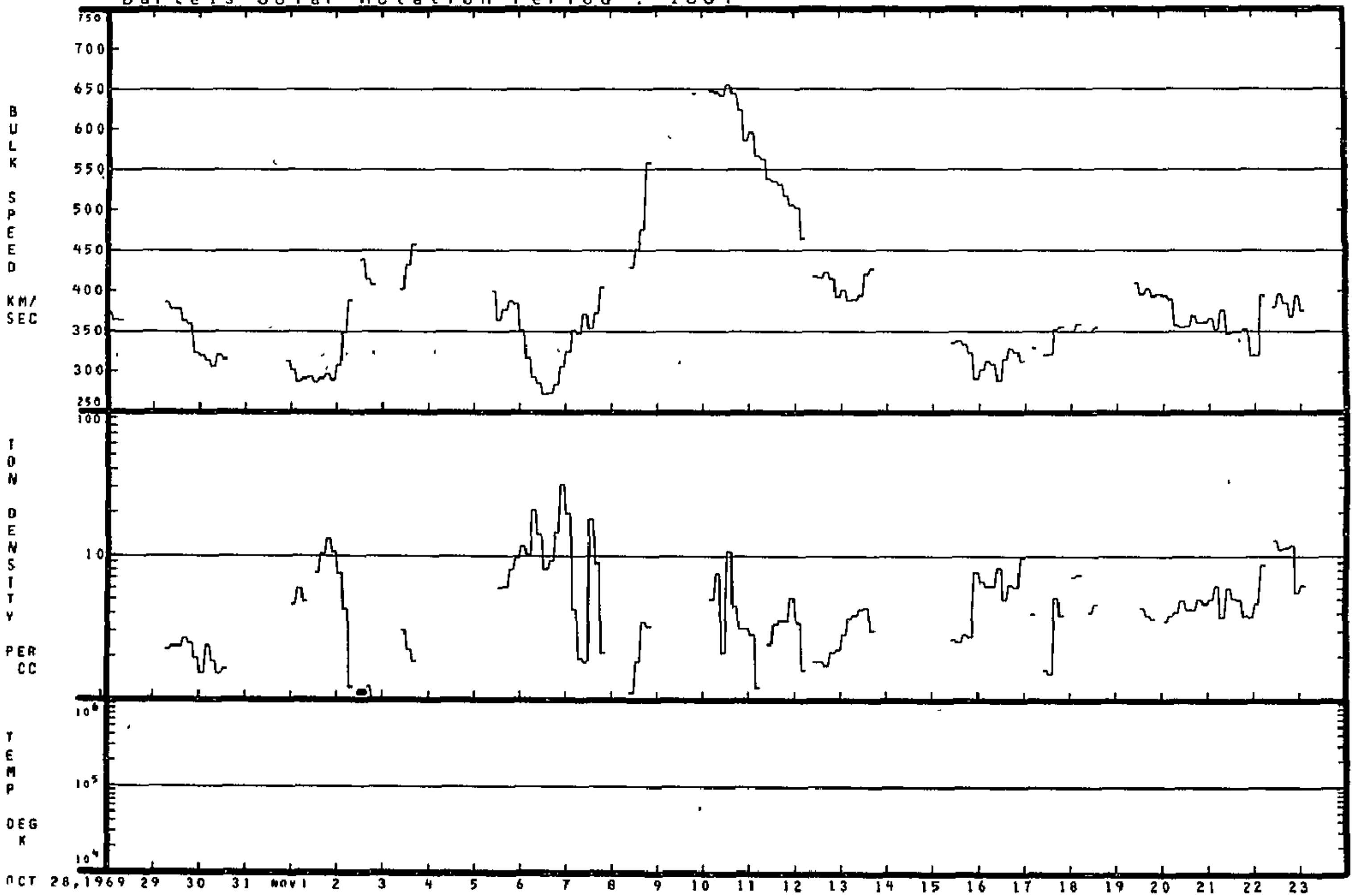
10/01/69 - 10/27/69



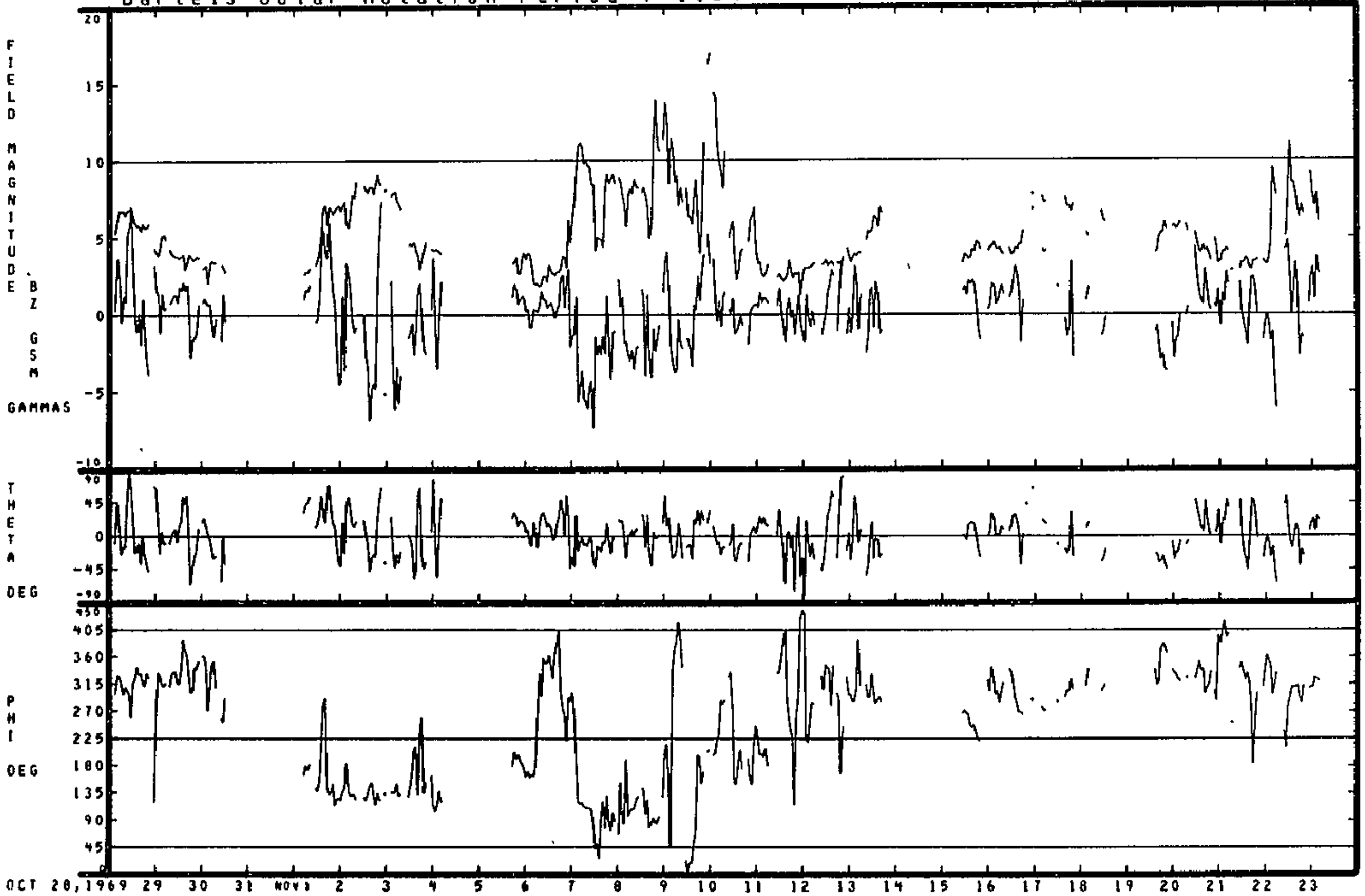
10/01/69 - 10/27/69

10/28/69 - 11/23/69

Bartels Solar Rotation Period : 1864

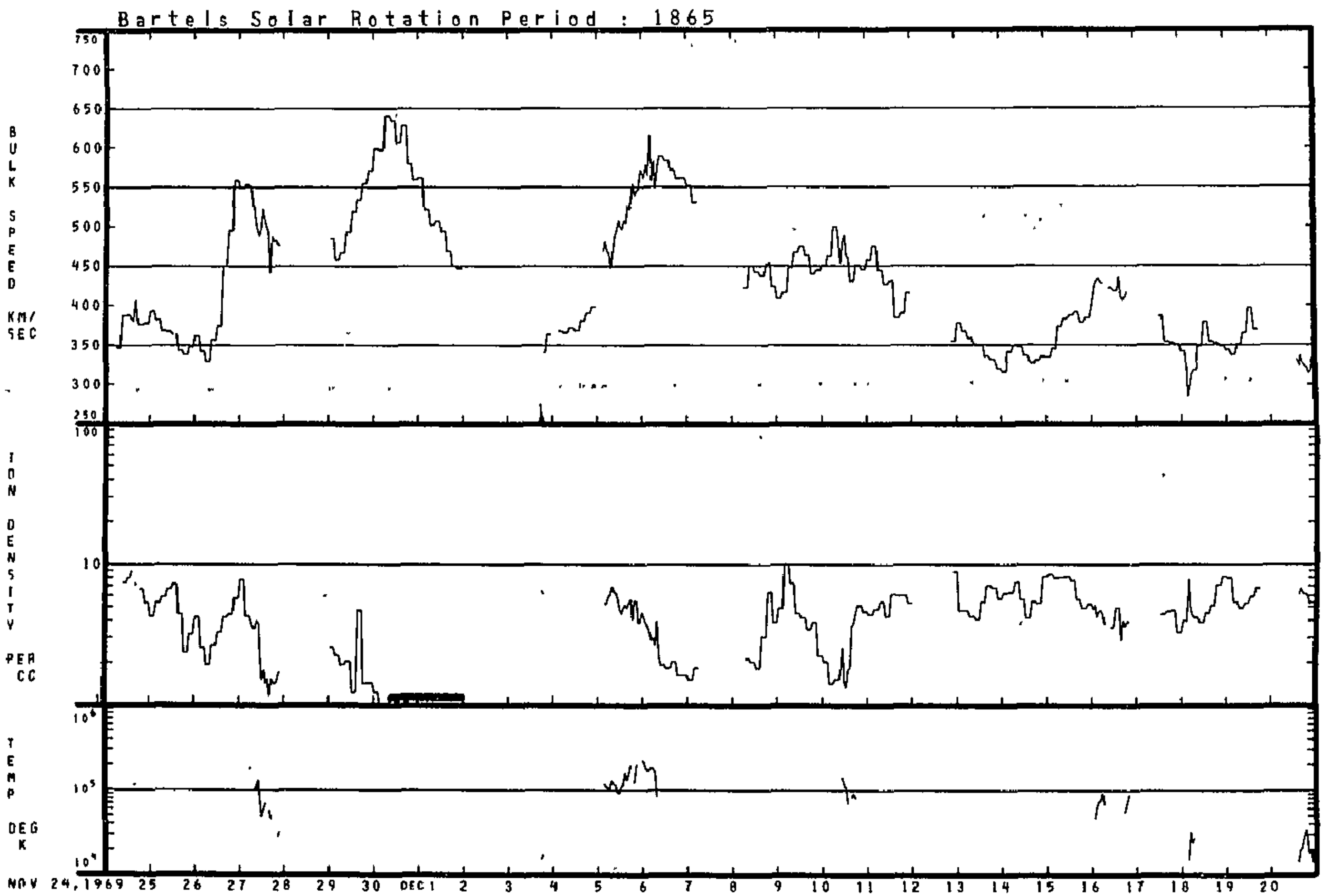


Bartels Solar Rotation Period : 1864

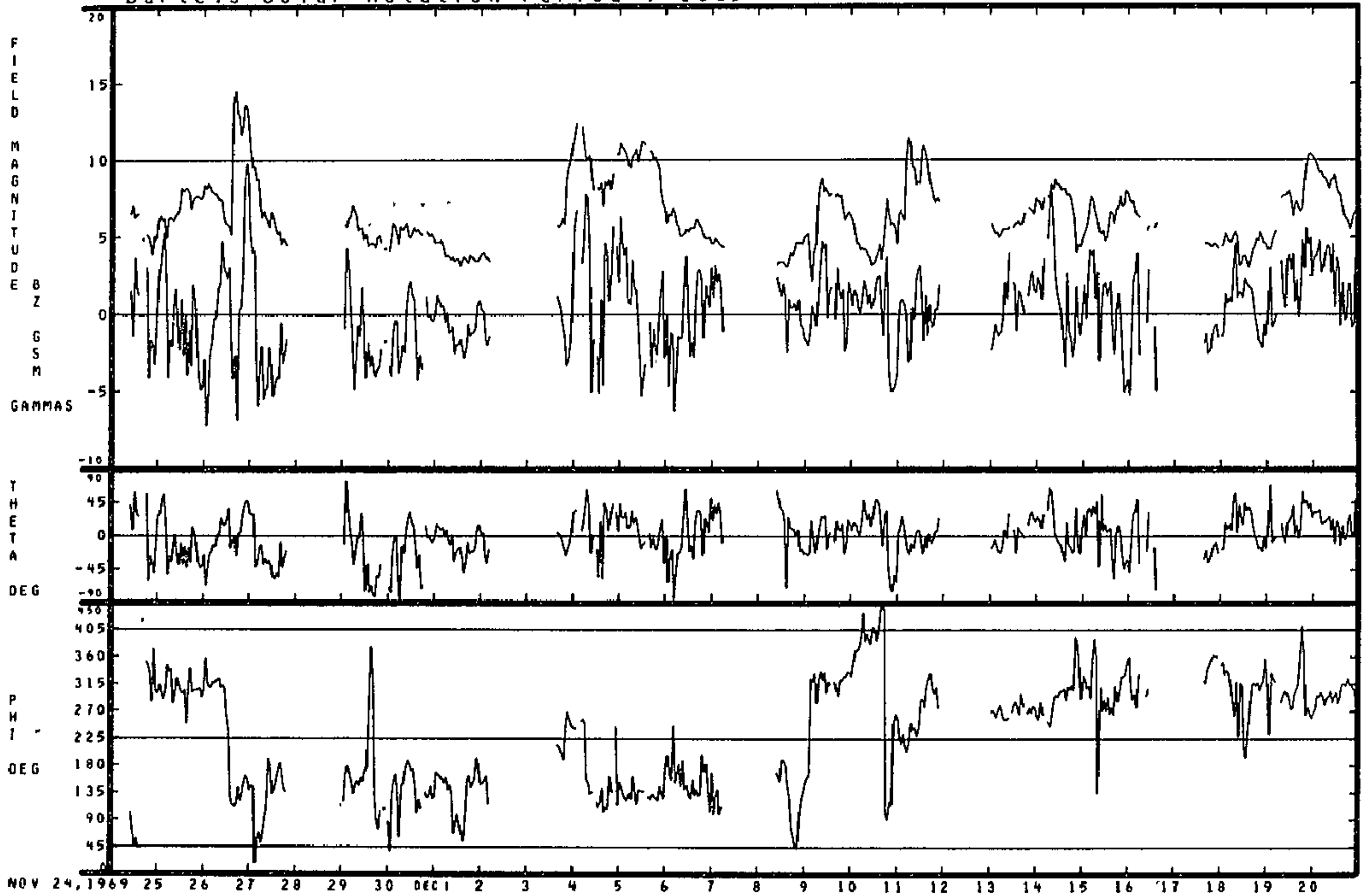


10/28/69 - 11/23/69

11/24/69 - 12/20/69

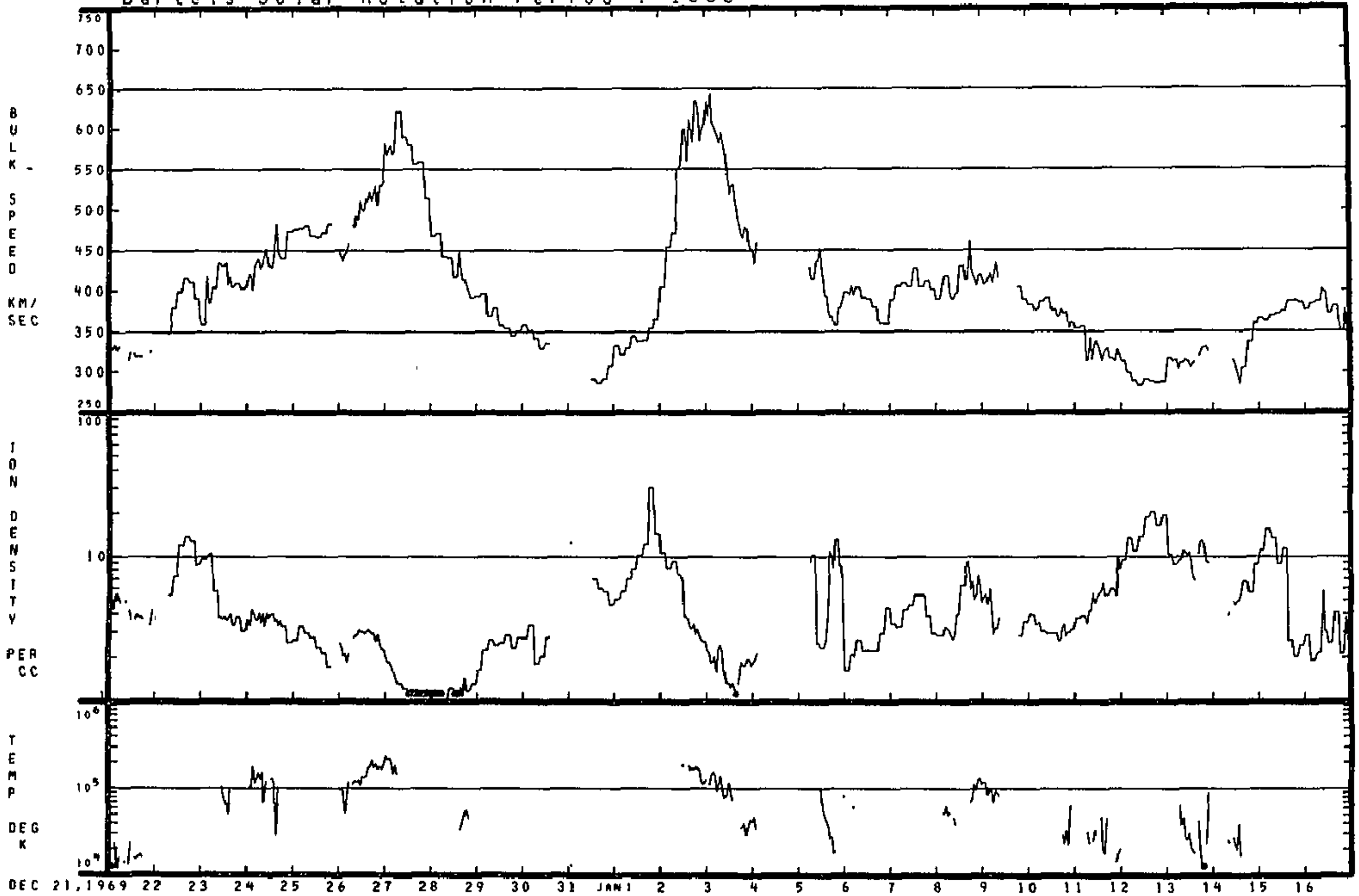


Bartels Solar Rotation Period : 1865



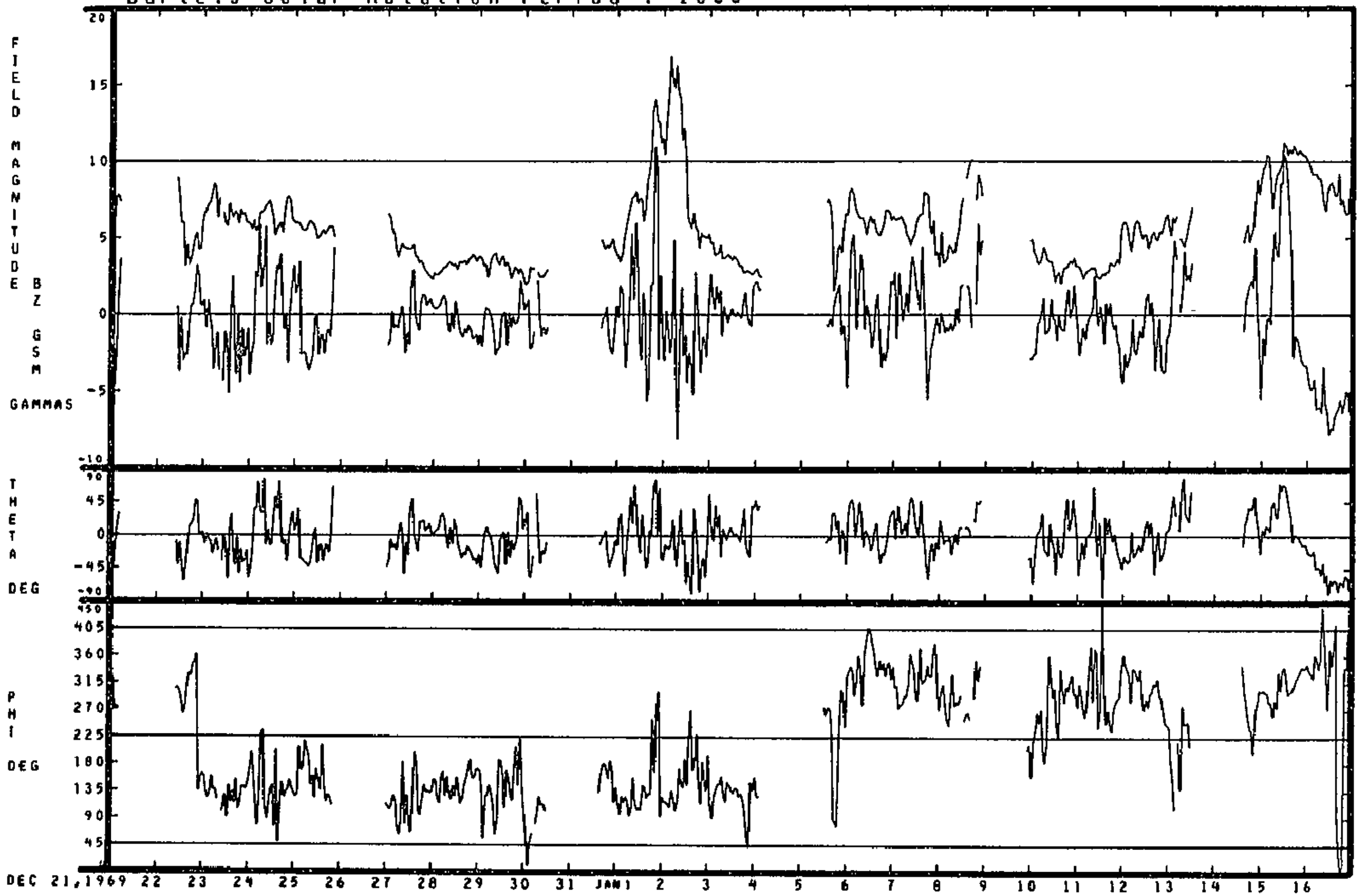
11/24/69 - 12/20/69

Bartels Solar Rotation Period : 1866



12/21/69 - 01/16/70

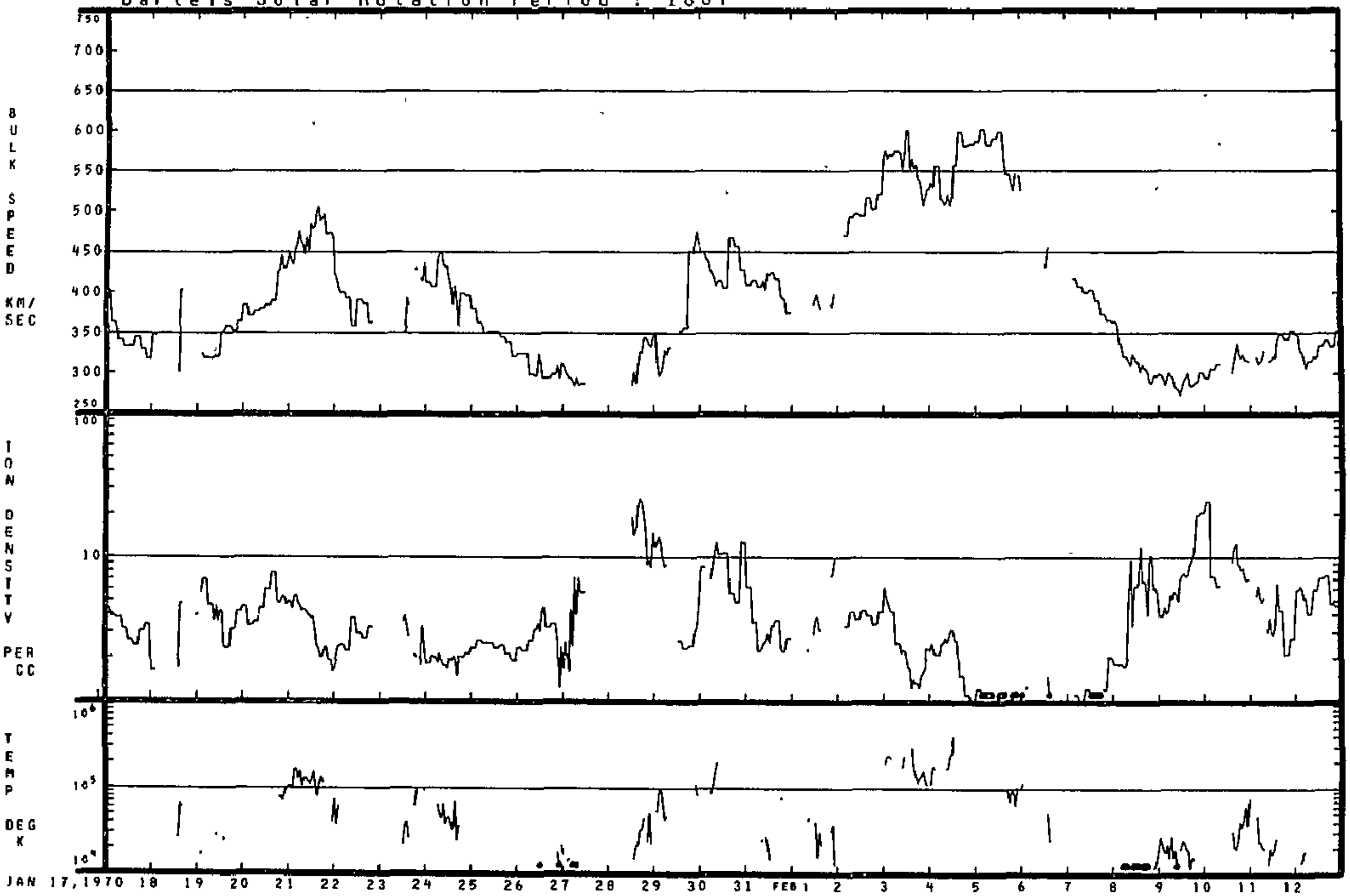
Bartels Solar Rotation Period : 1866

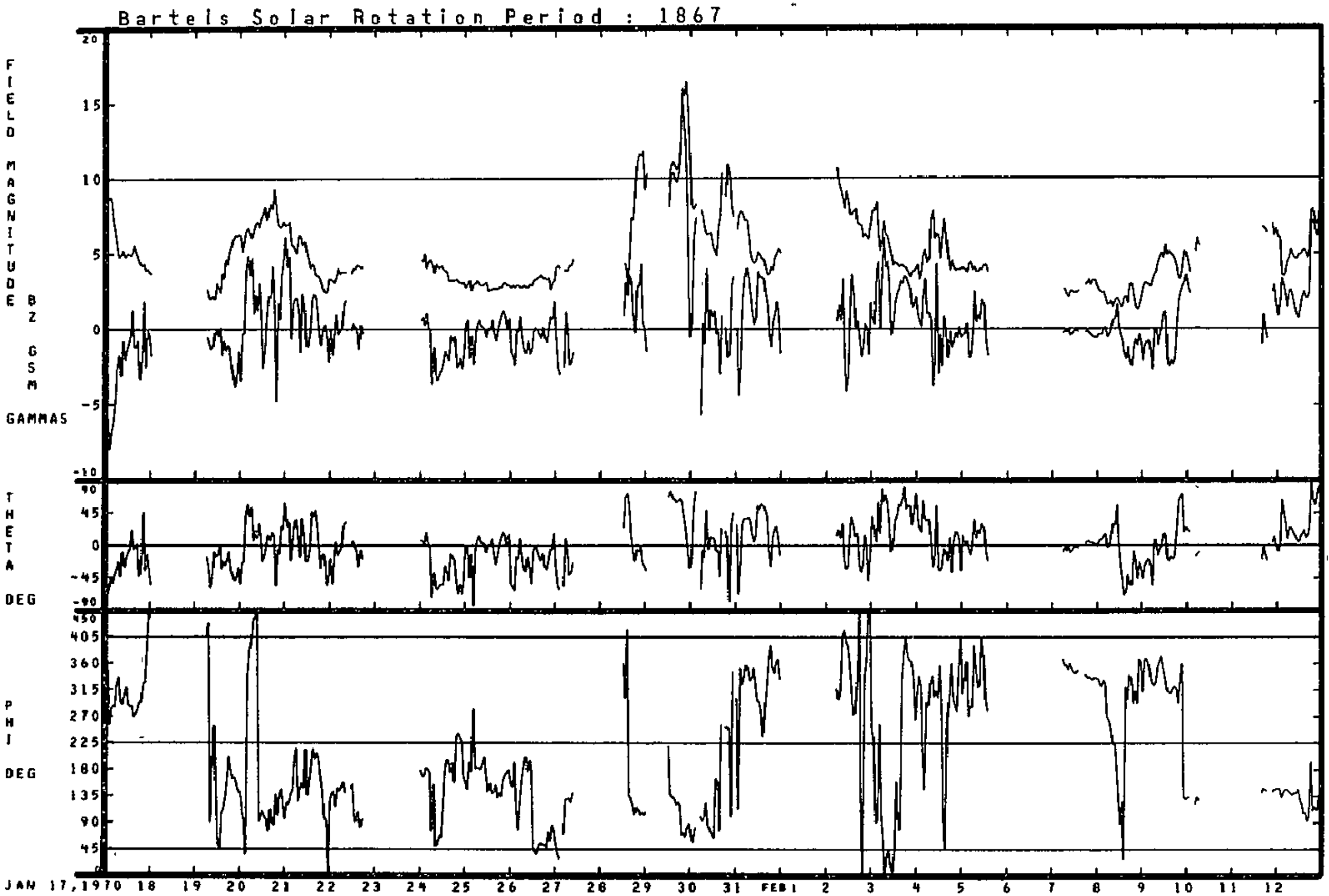


12/21/69 - 01/16/70

01/17/70 - 02/12/70

Bartels Solar Rotation Period : 1867

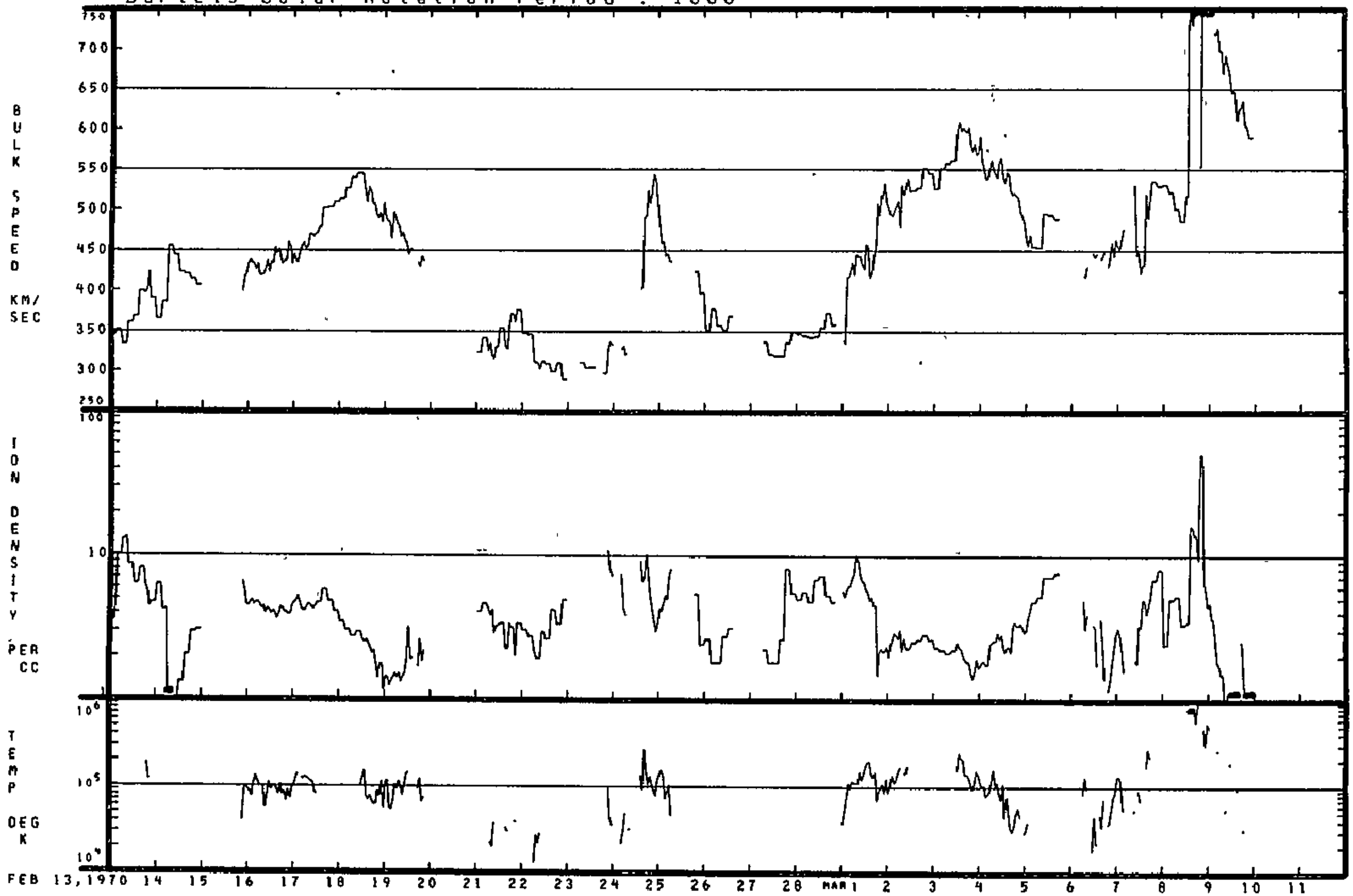


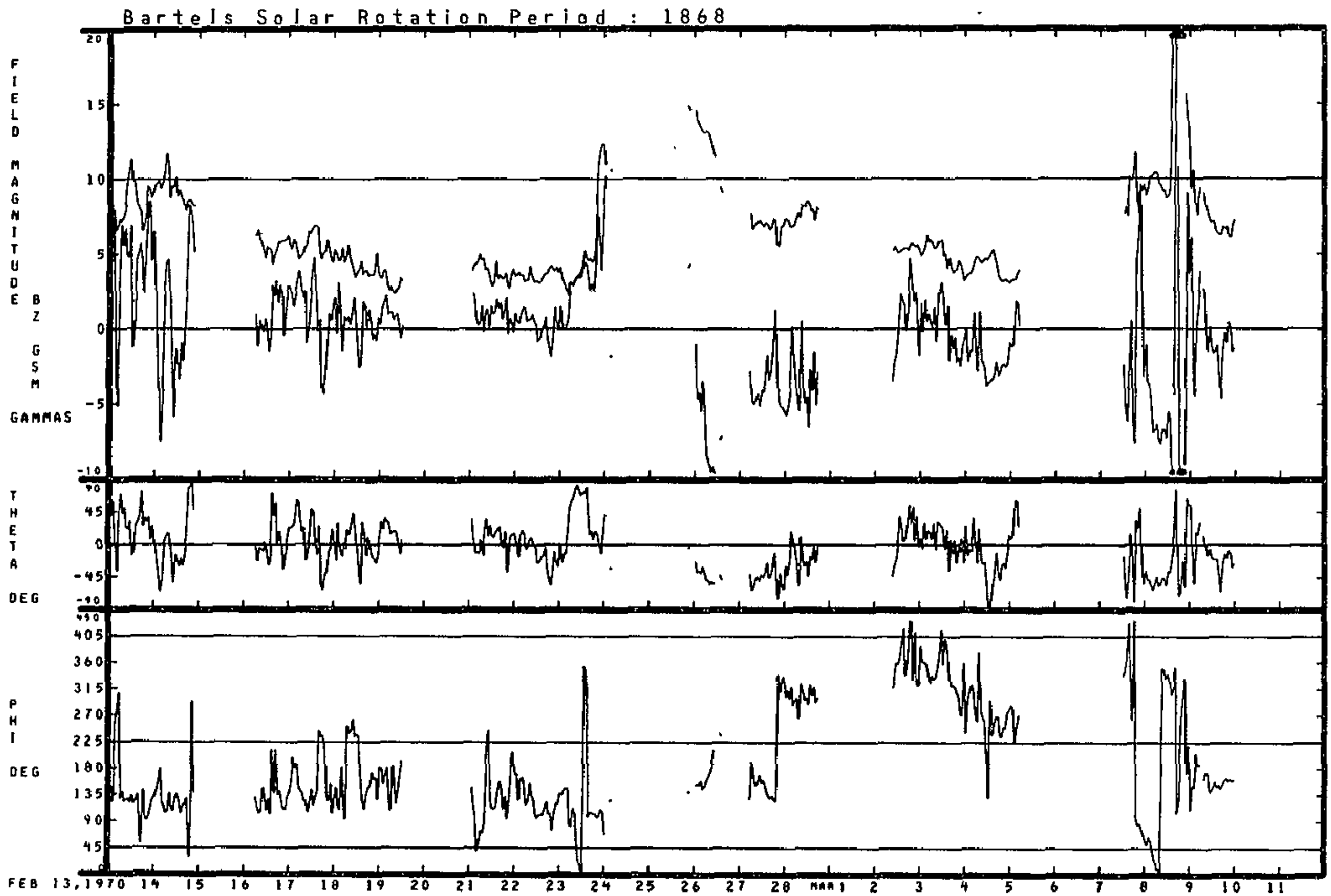


01/17/70 - 02/12/70

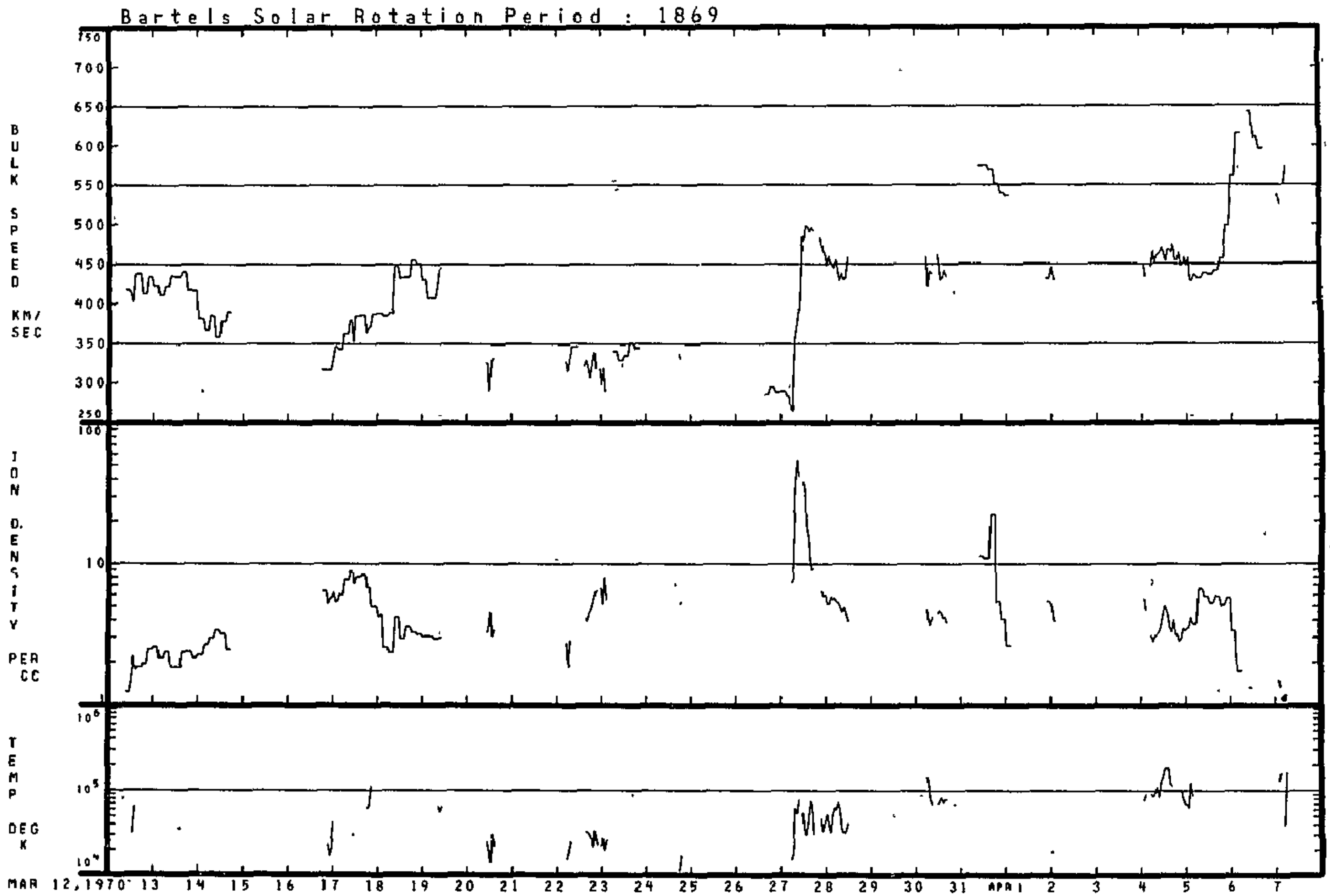
02/13/70 - 03/11/70

Bartels Solar Rotation Period : 1868



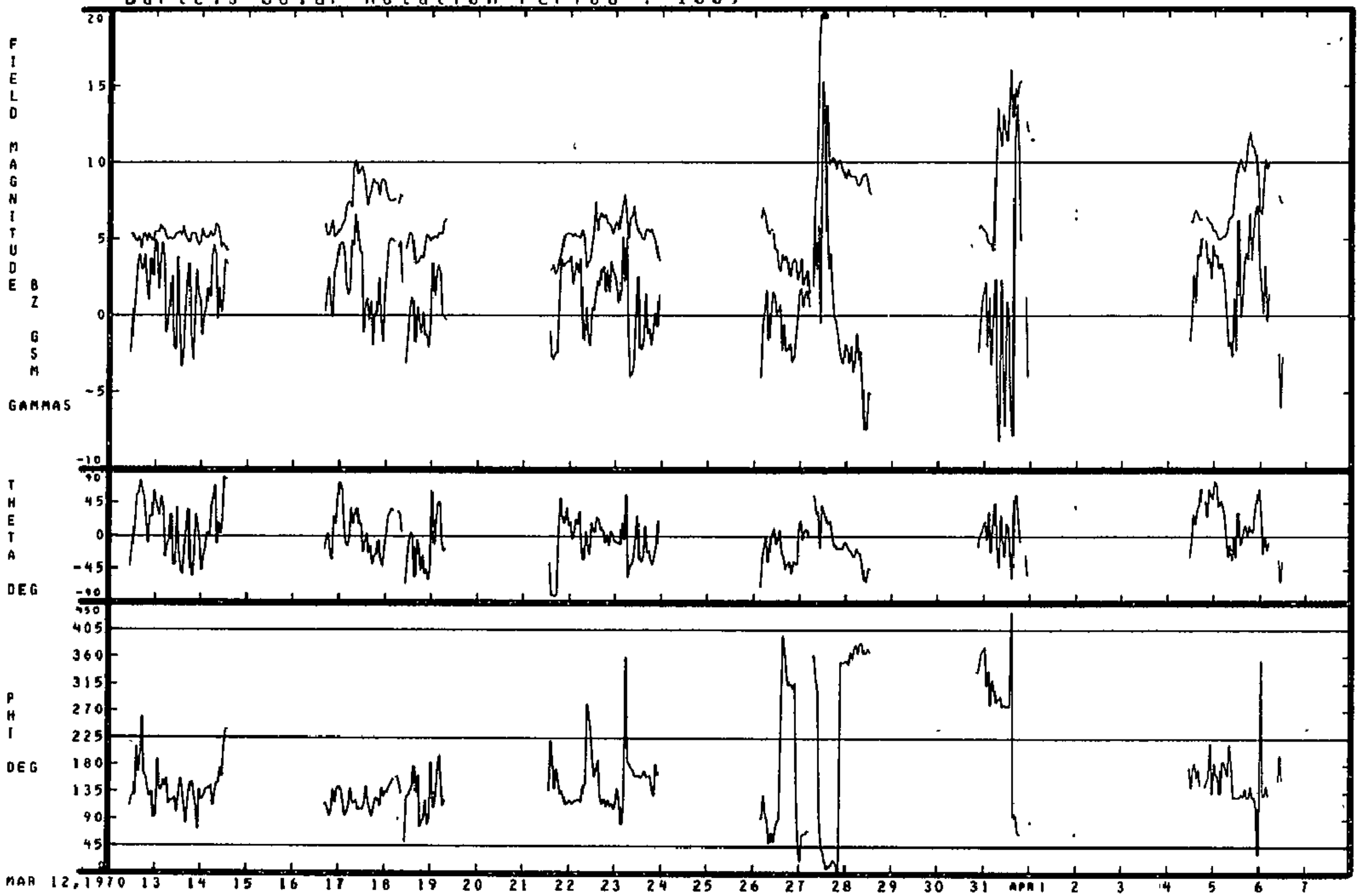


02/13/70 - 03/11/70



03/12/70 - 04/07/70

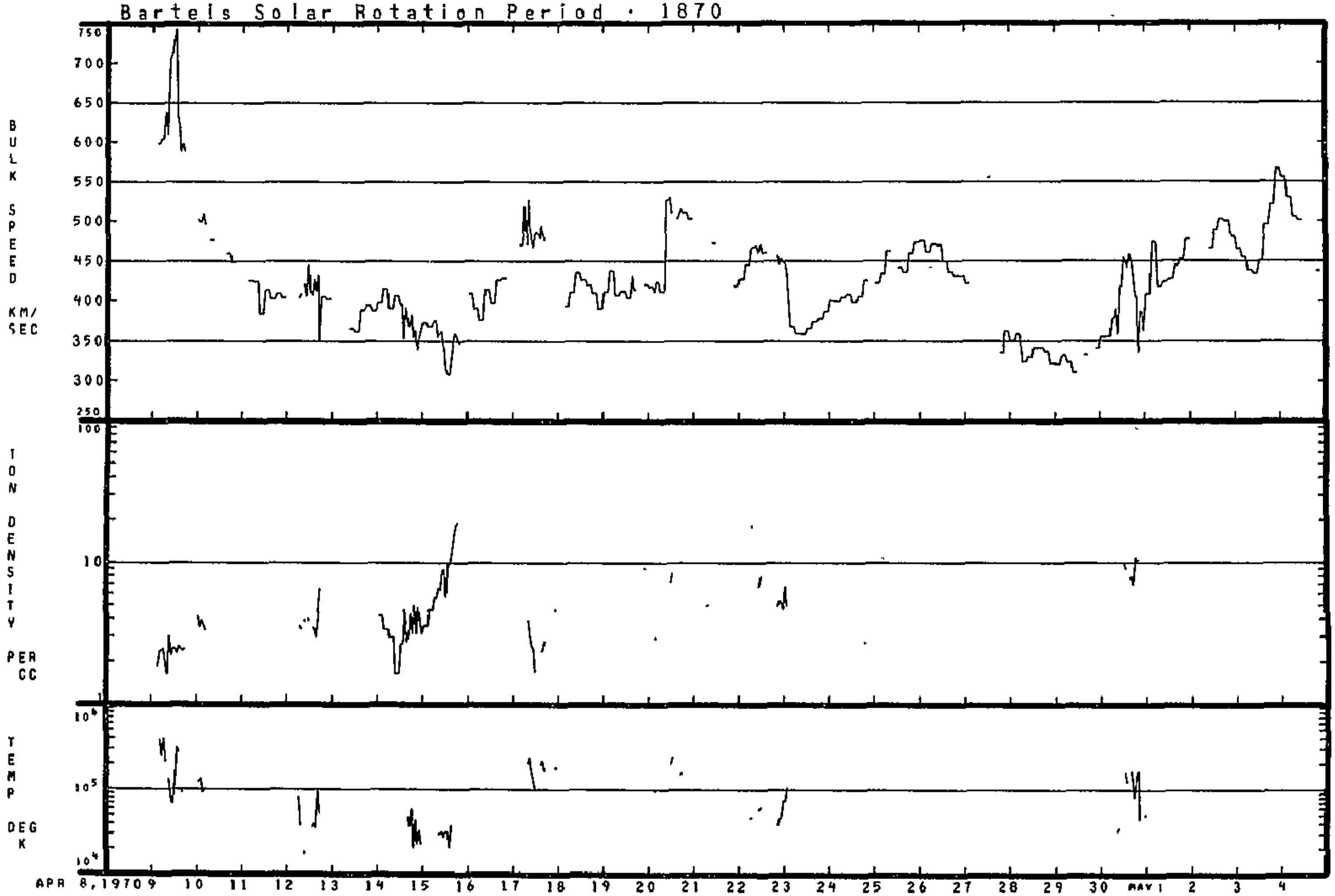
Bartels Solar Rotation Period : 1869

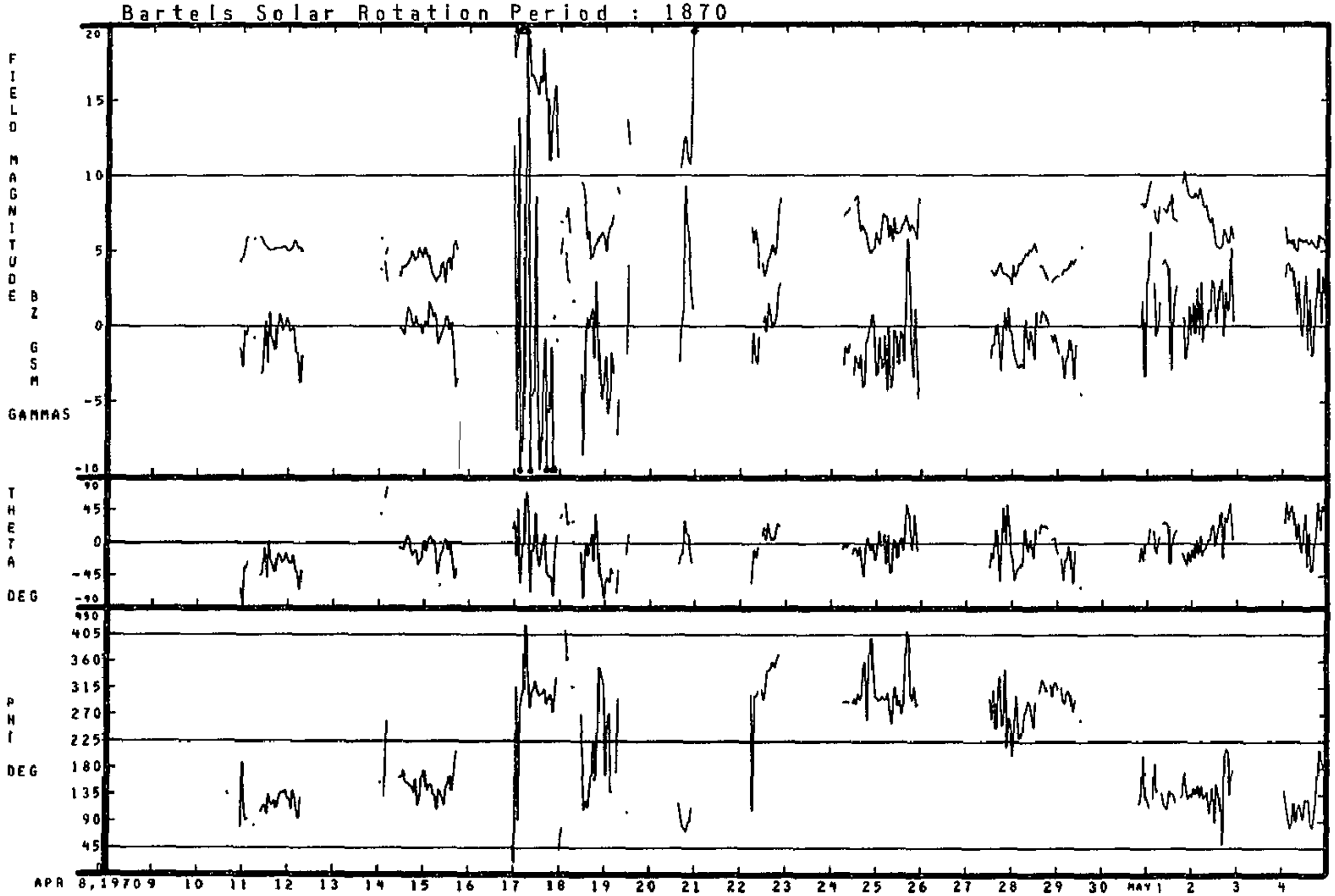


03/12/70 - 04/07/70

04/08/70 - 05/04/70

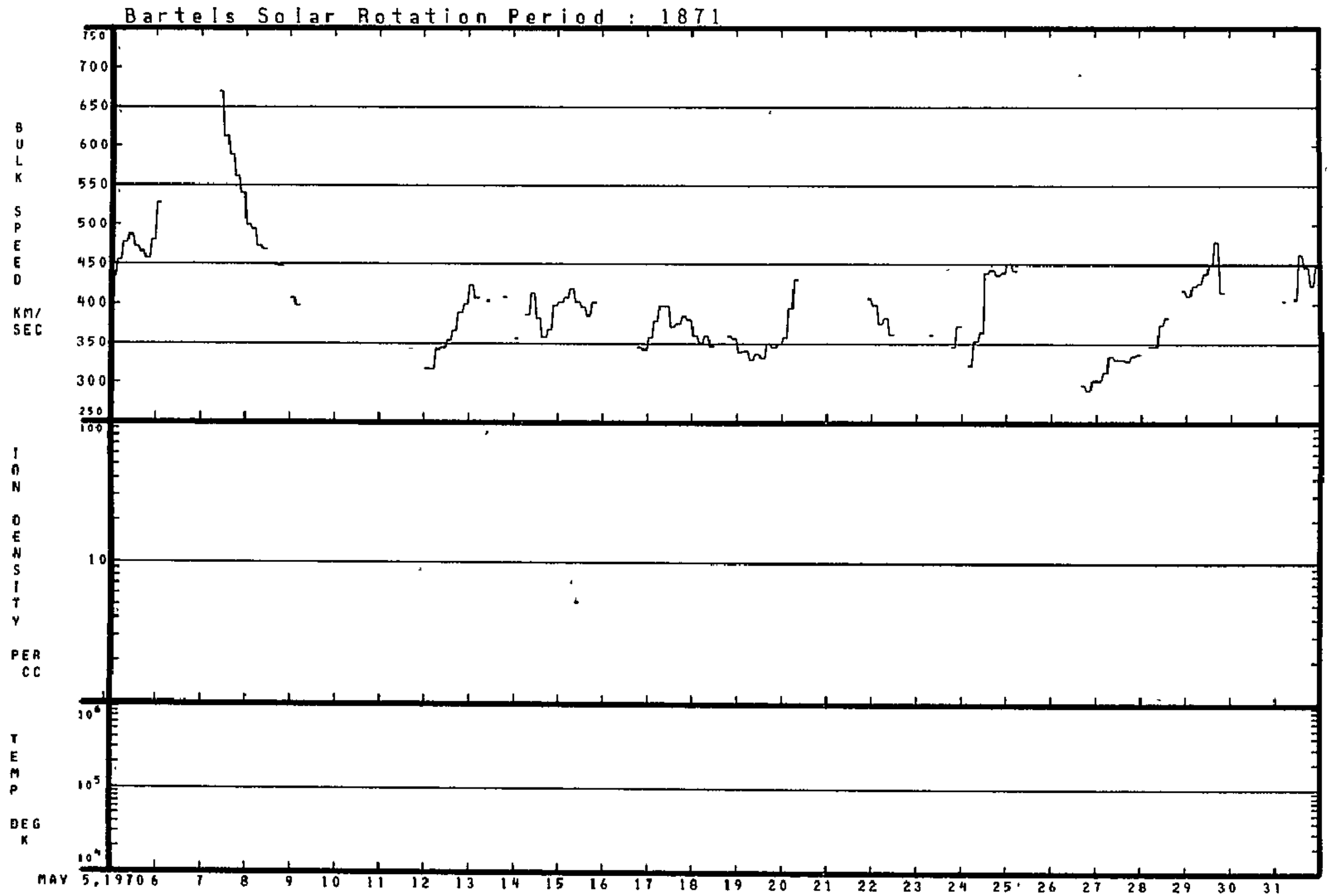
Bartels Solar Rotation Period - 1870

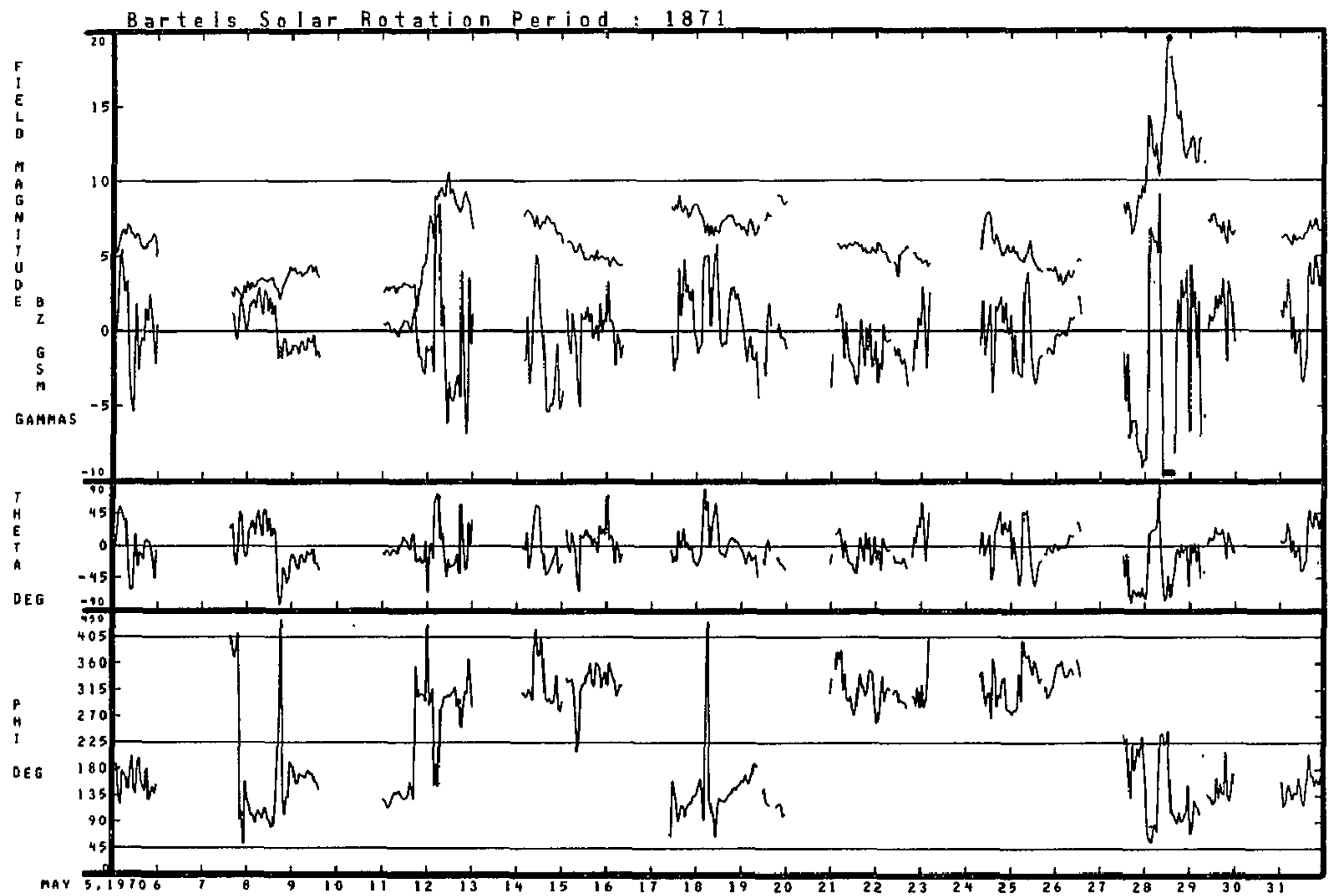




04/08/70 - 05/04/70

05/05/70 - 05/31/70

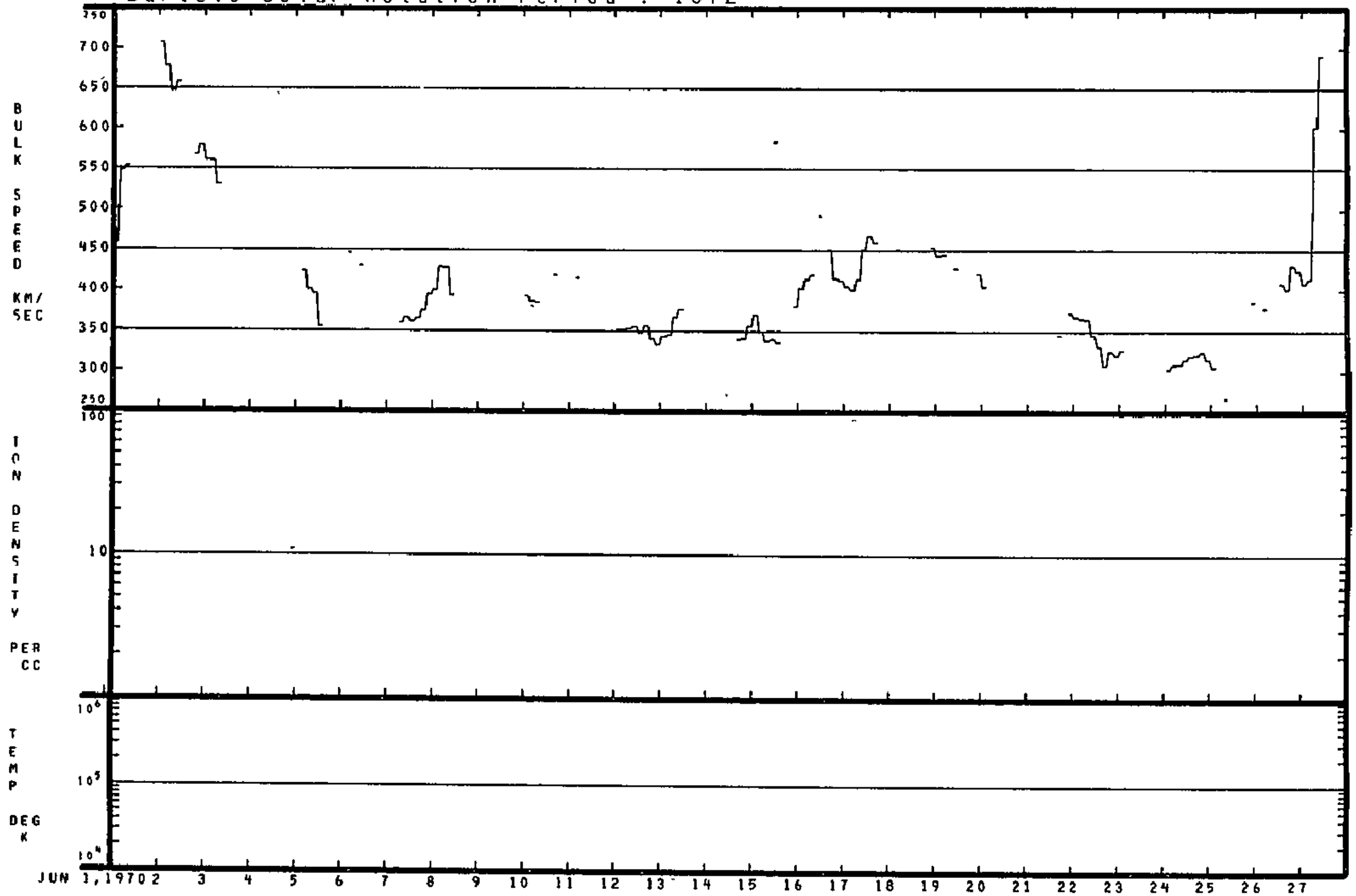




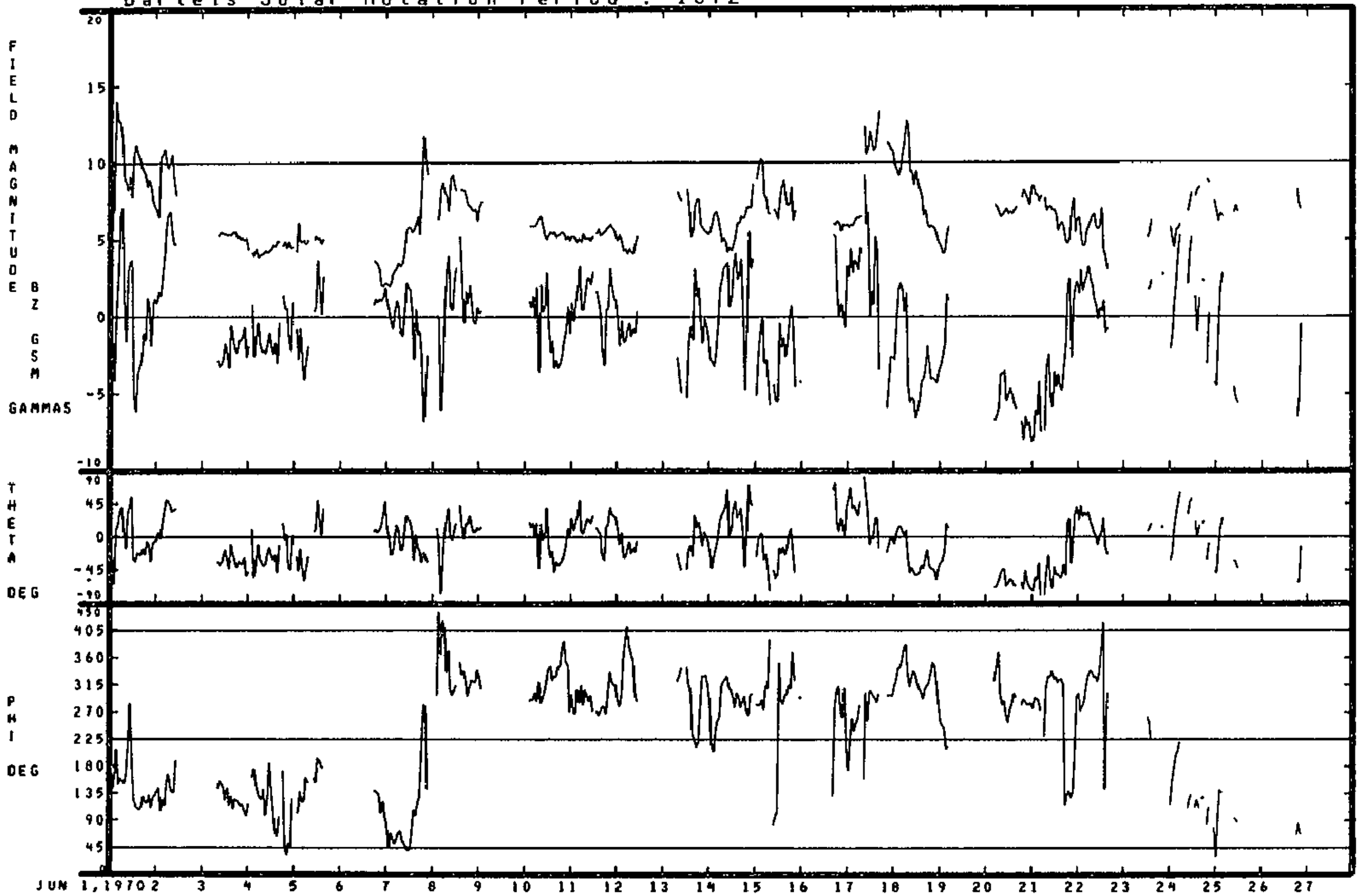
05/05/70 - 05/31/70

06/01/70 - 06/27/70

Bartels Solar Rotation Period : 1872

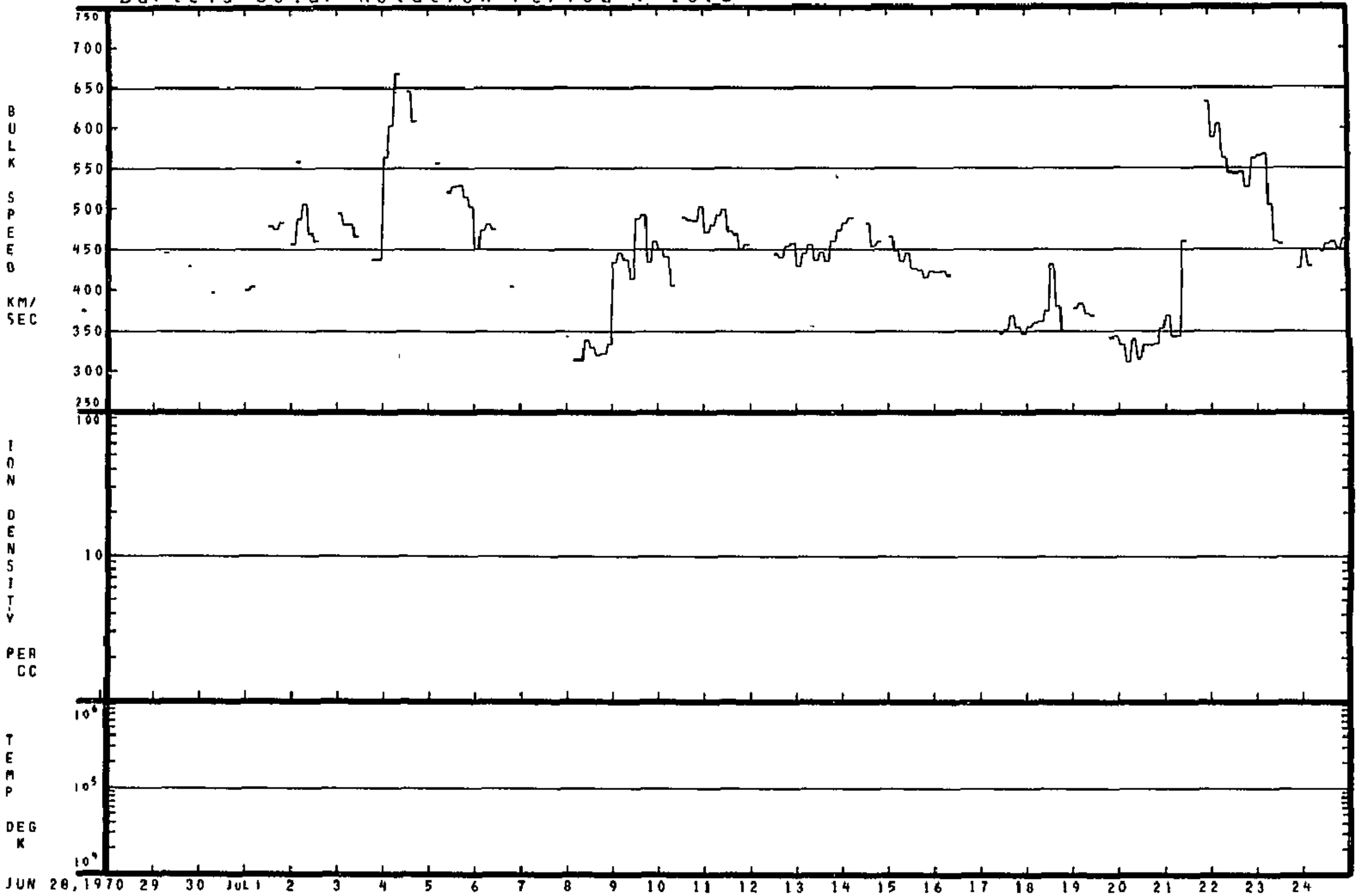


Bartels Solar Rotation Period : 1872

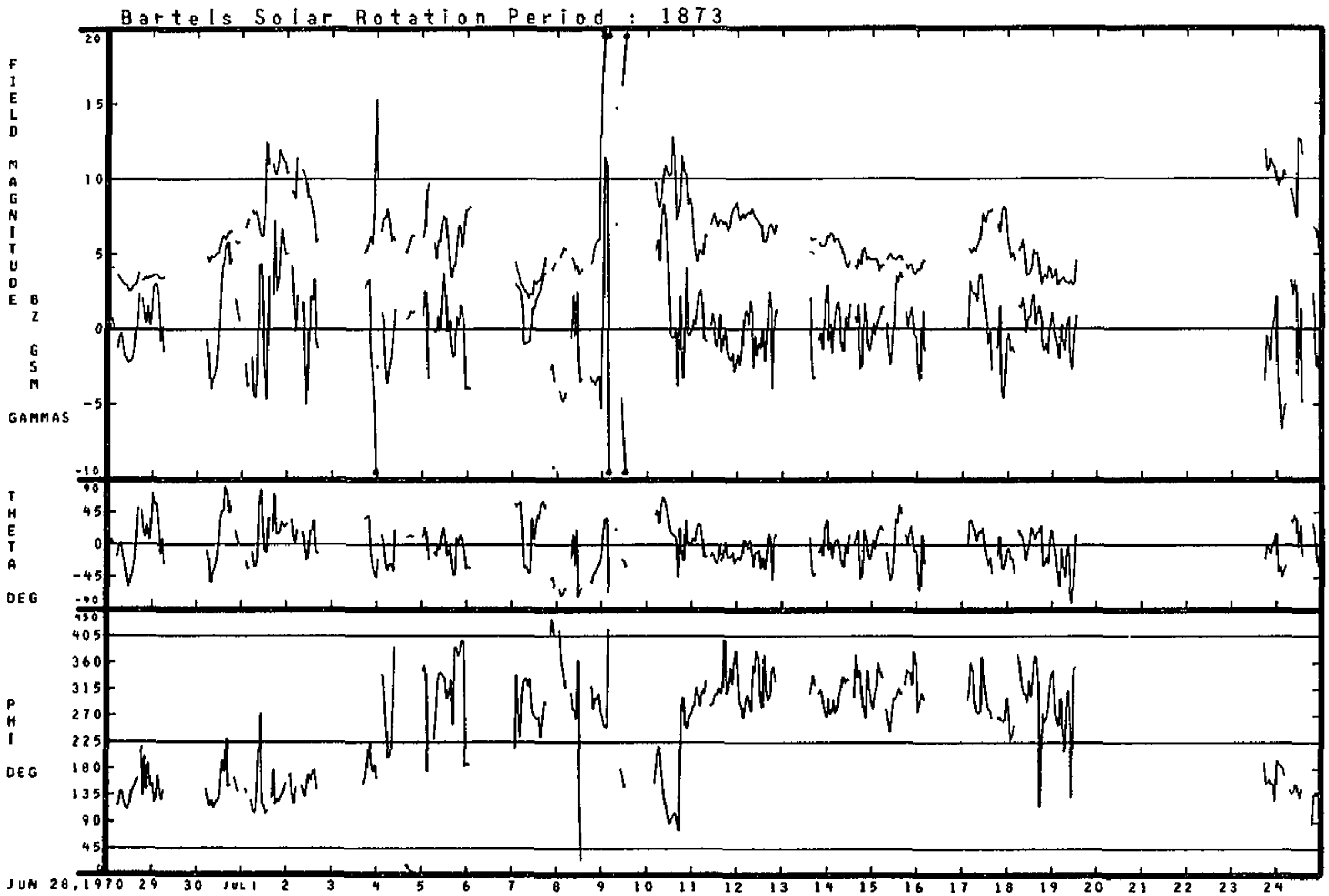


06/01/70 - 06/27/70

Bartels Solar Rotation Period : 1873



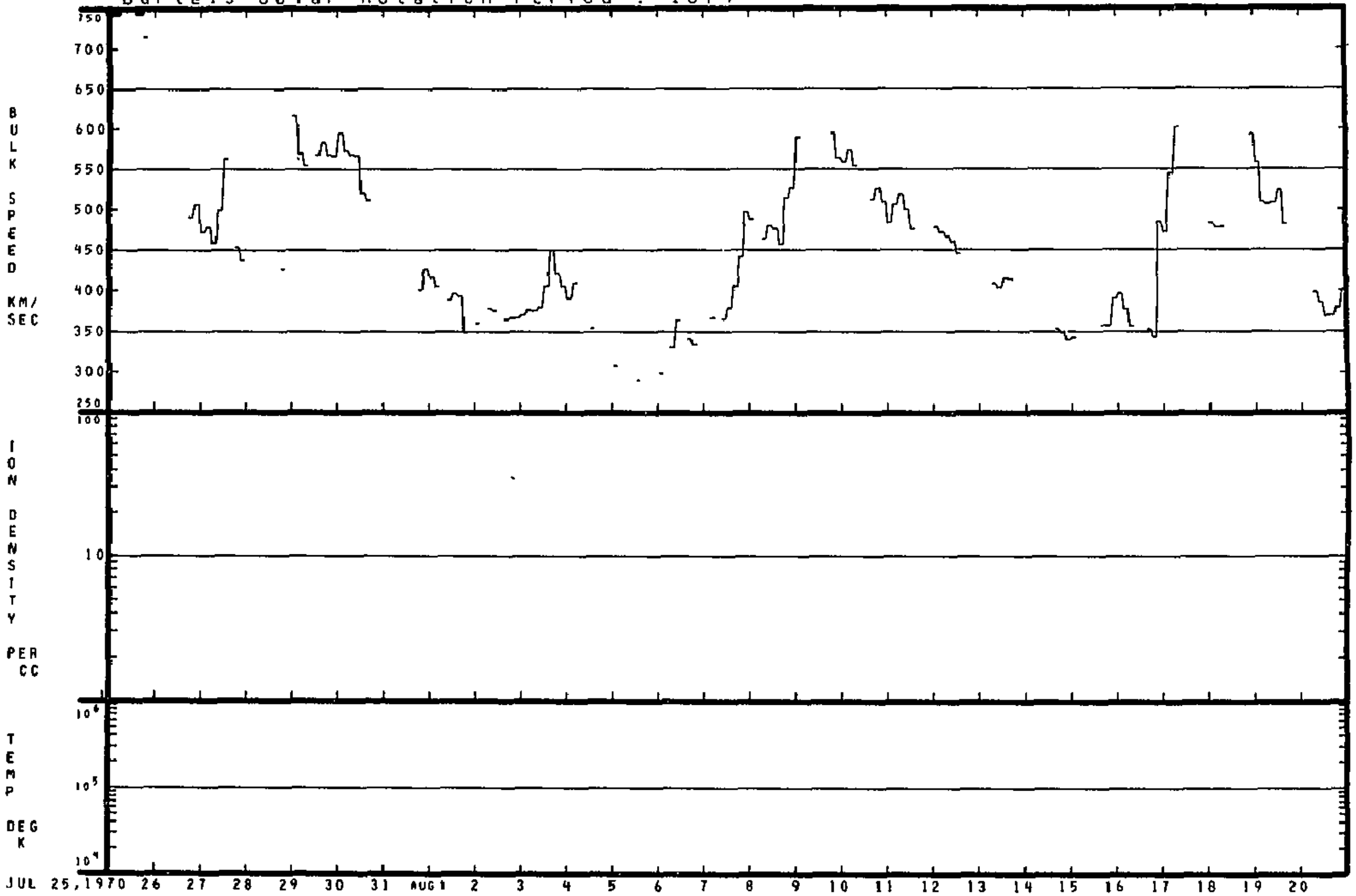
06/28/70 - 07/24/70



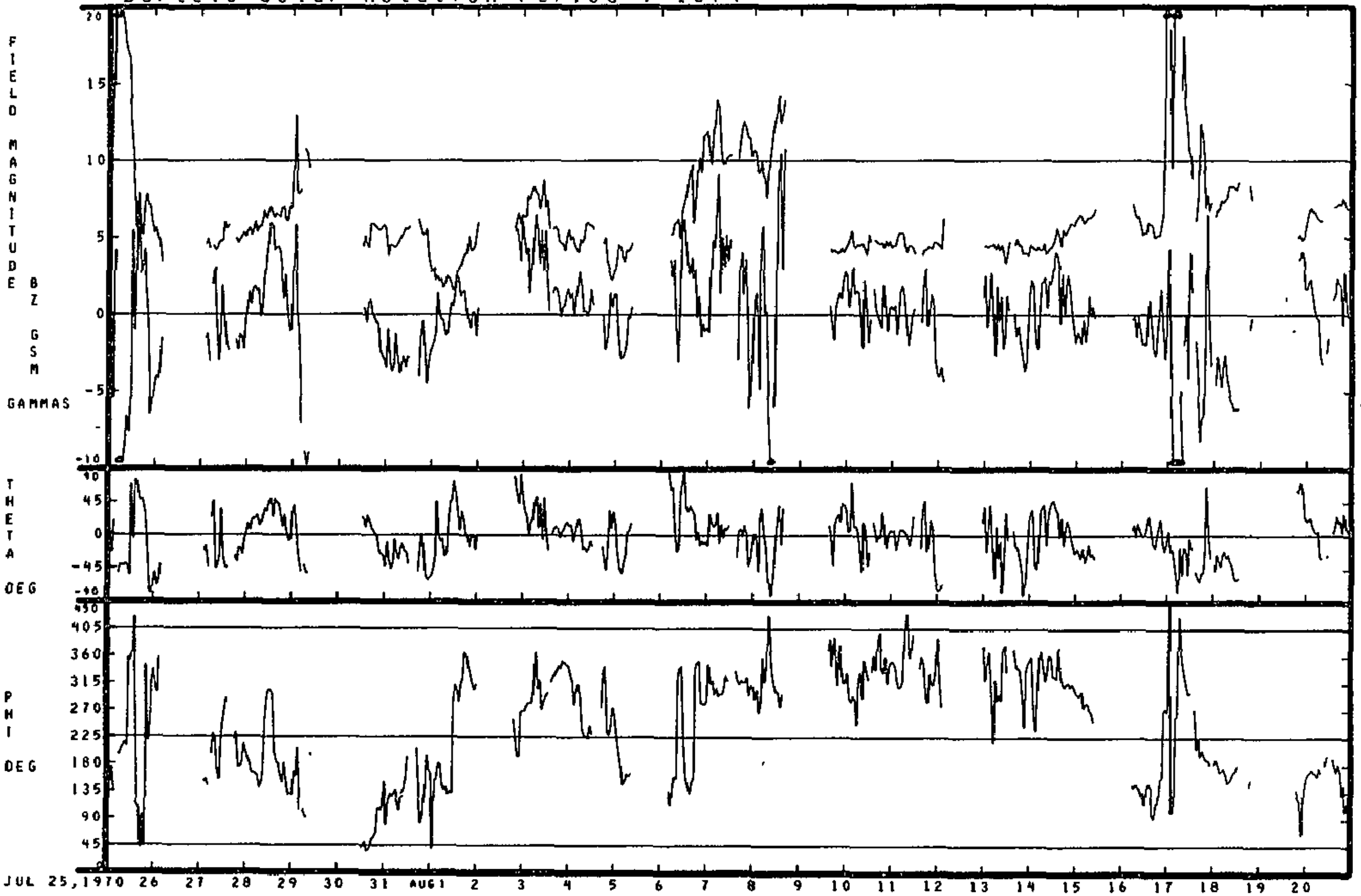
06/28/70 - 07/24/70

07/25/70 - 08/20/70

Bartels Solar Rotation Period : 1874

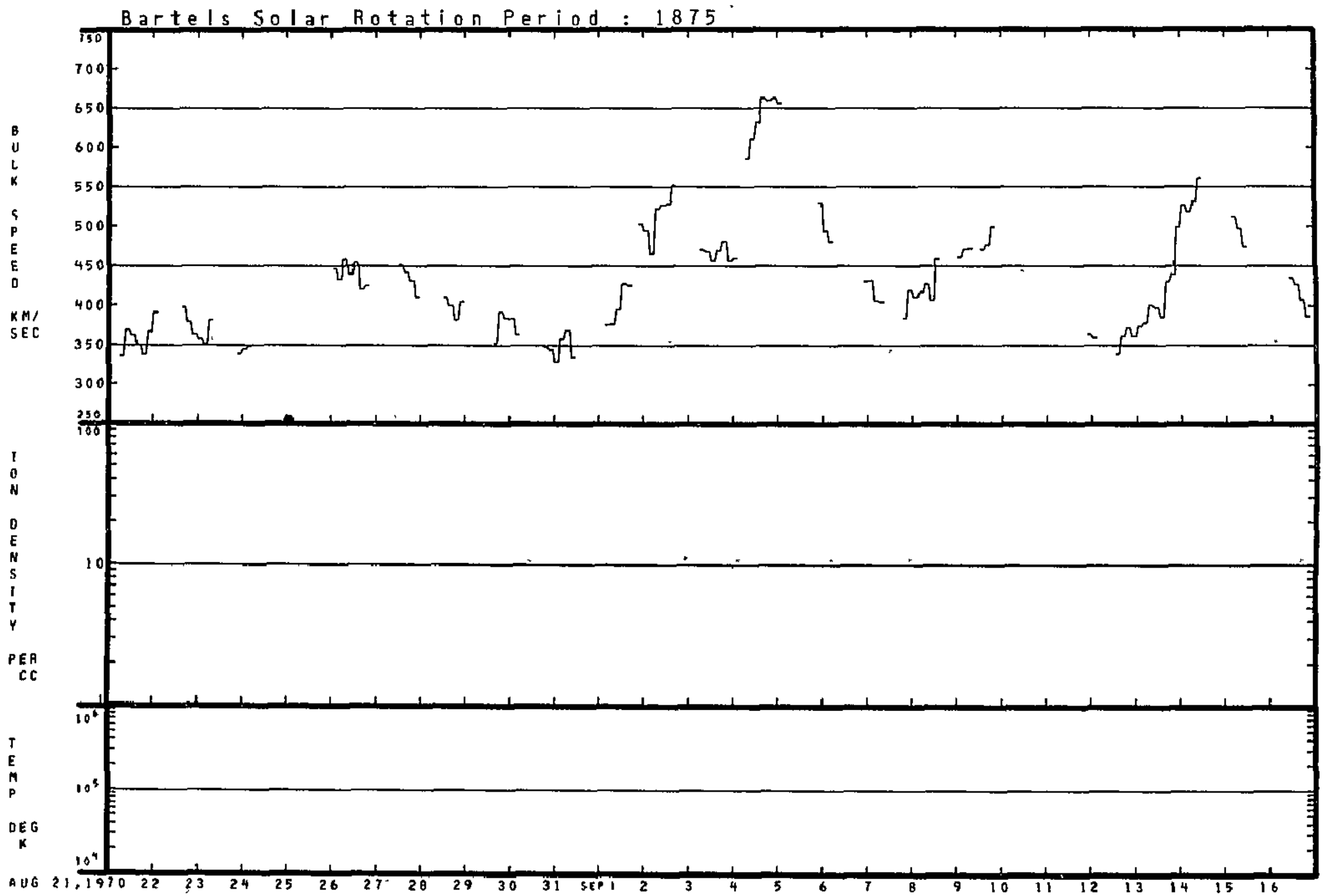


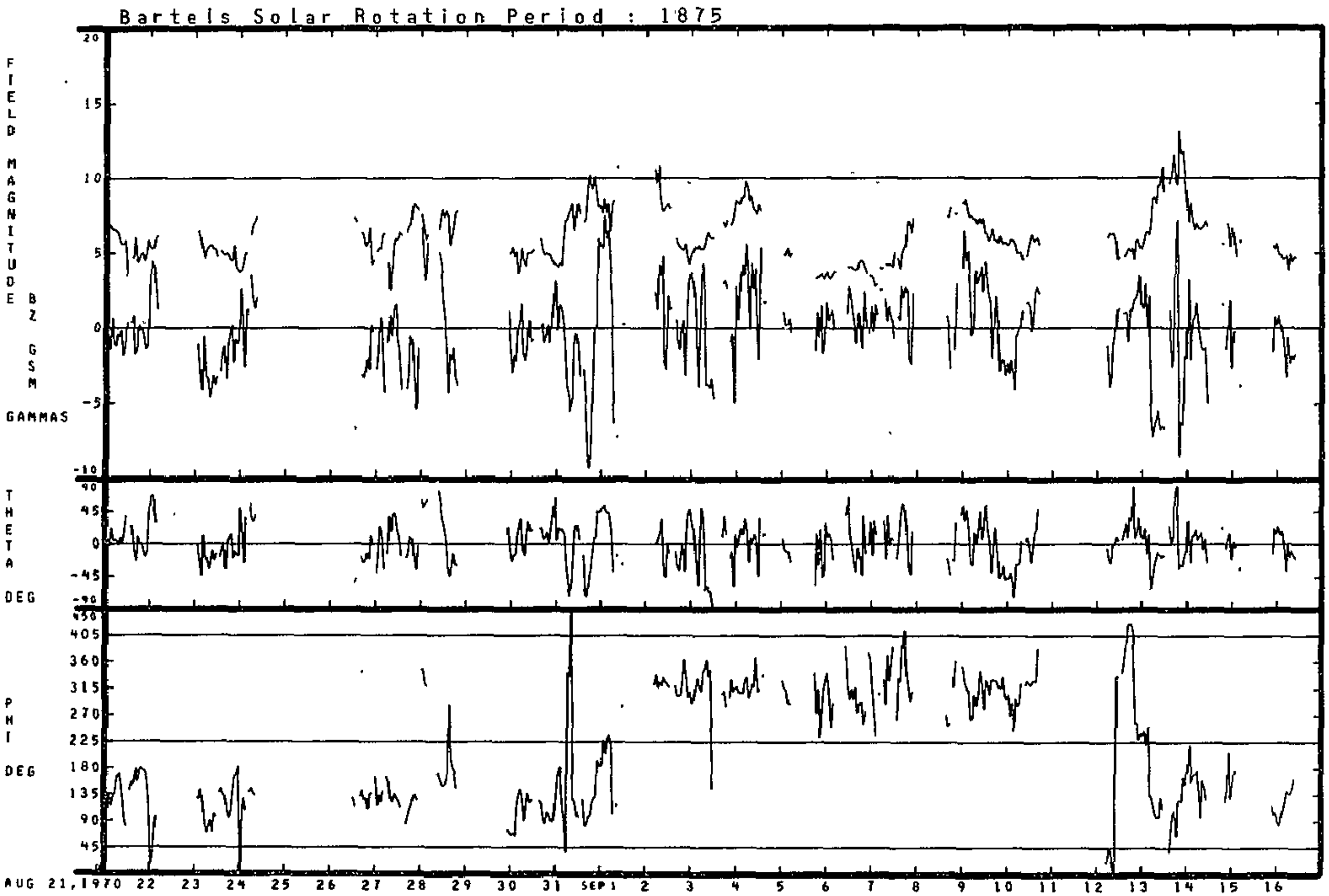
Bartels Solar Rotation Period : 1874



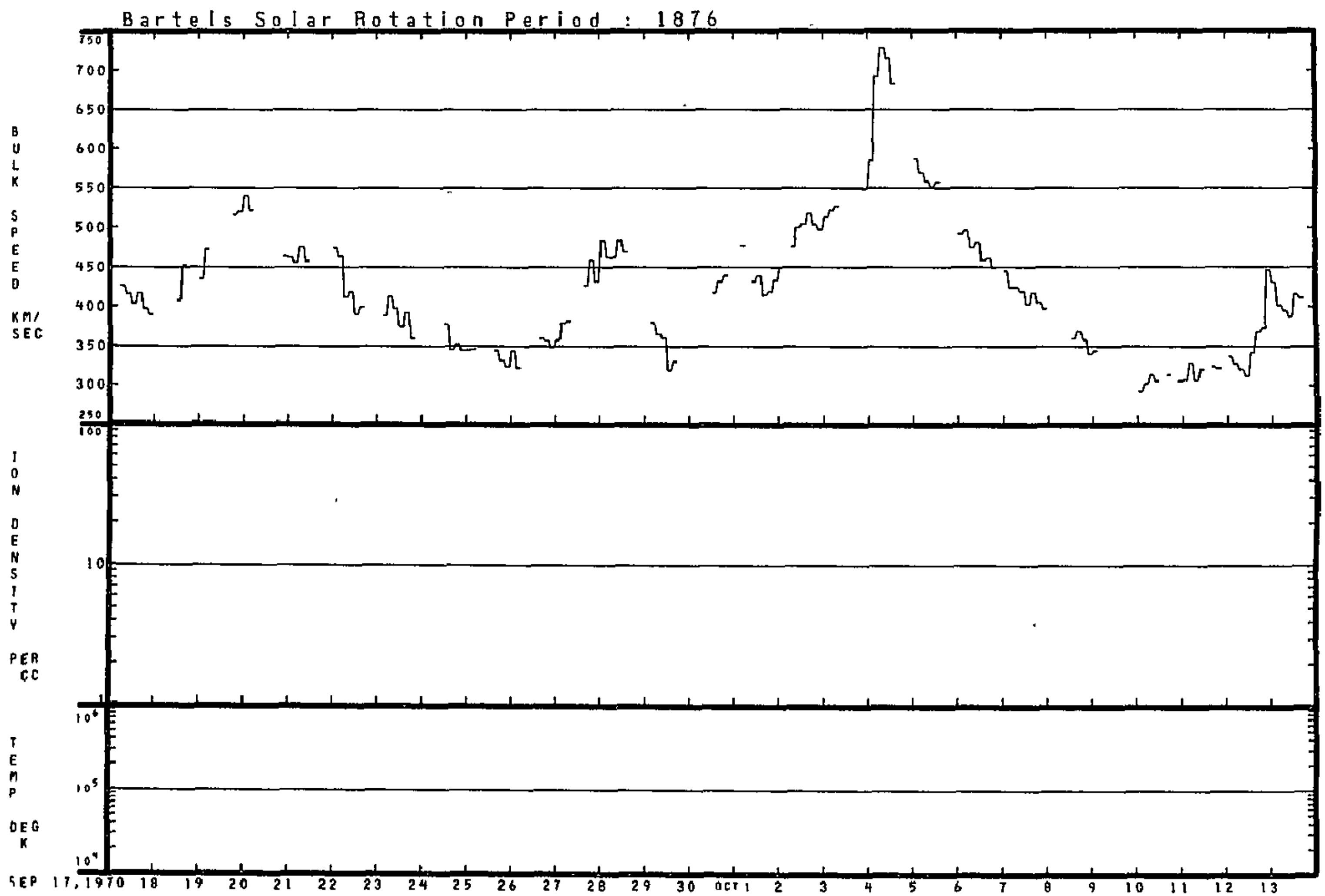
07/25/70 - 08/20/70

08/21/70 - 09/16/70

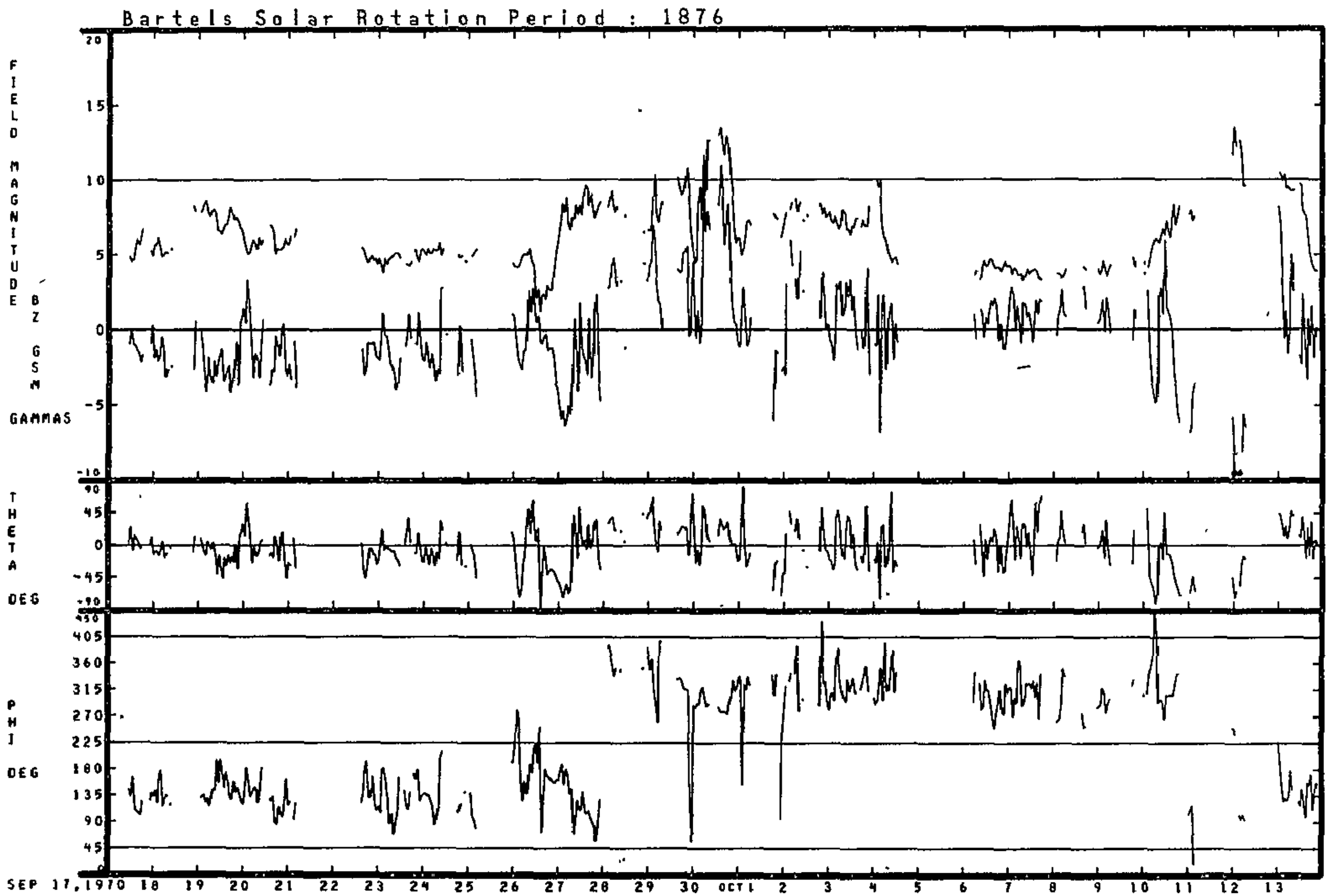




08/21/70 - 09/16/70

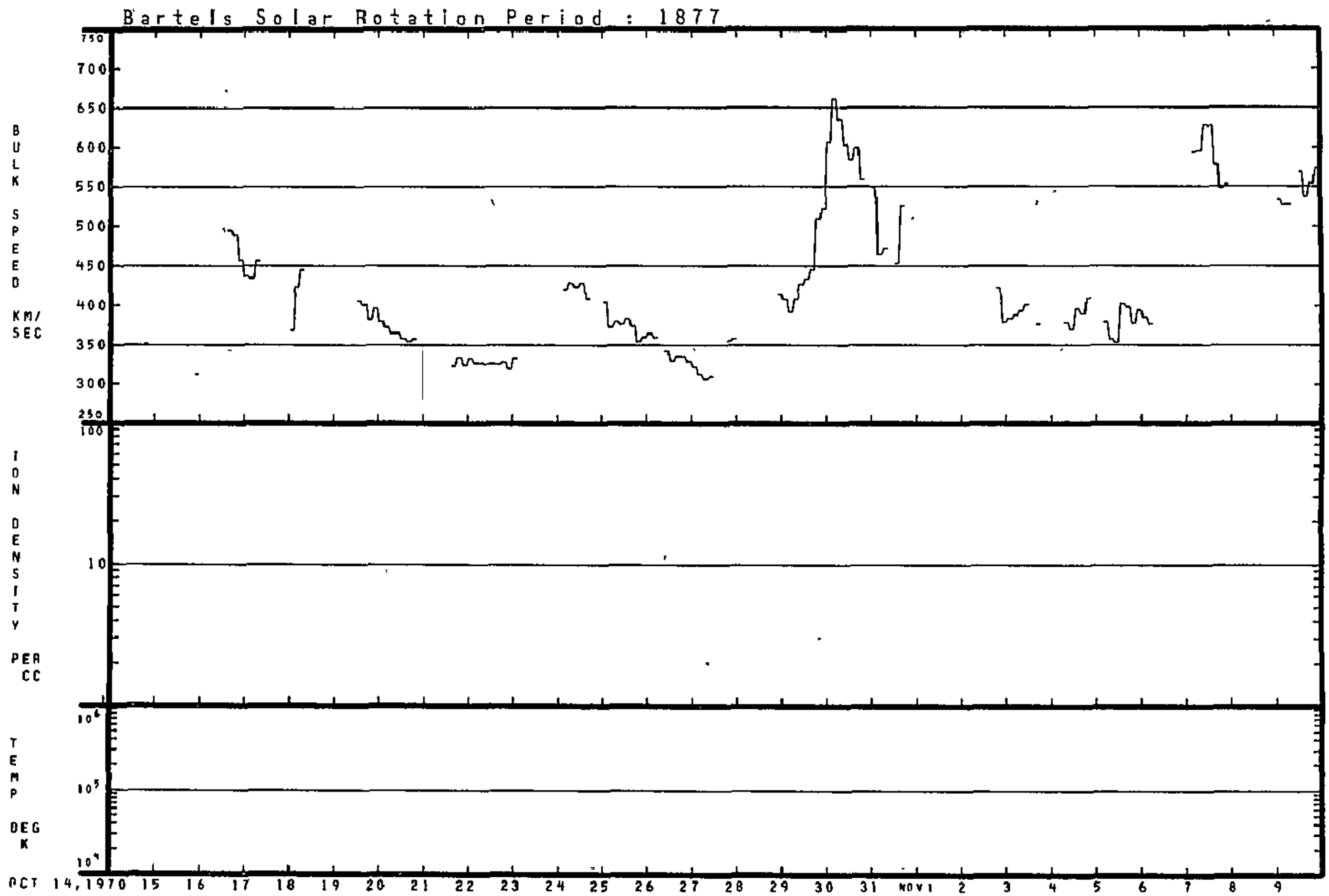


09/17/70 - 10/13/70

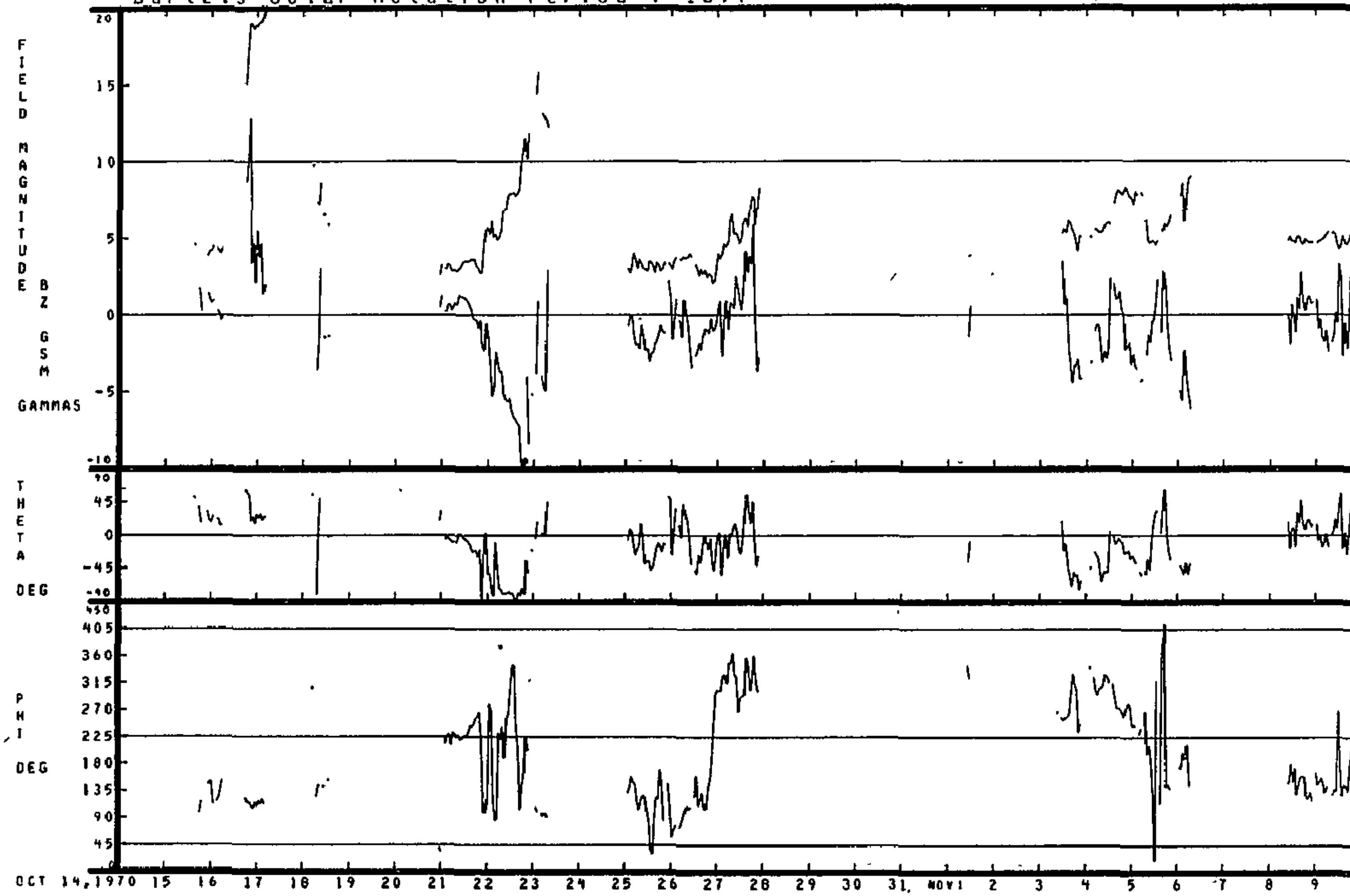


09/17/70 - 10/13/70

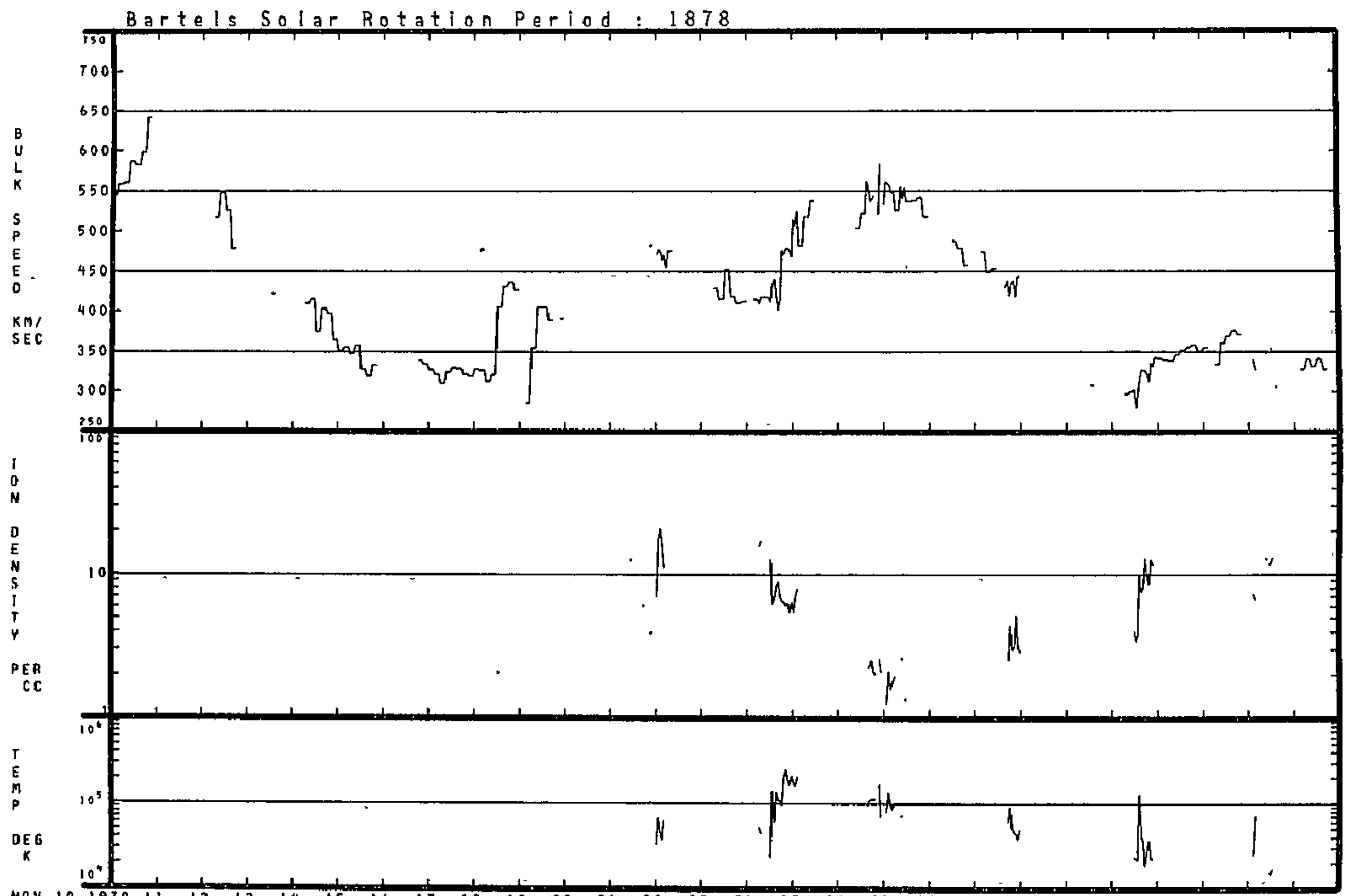
10/14/70 - 11/09/70



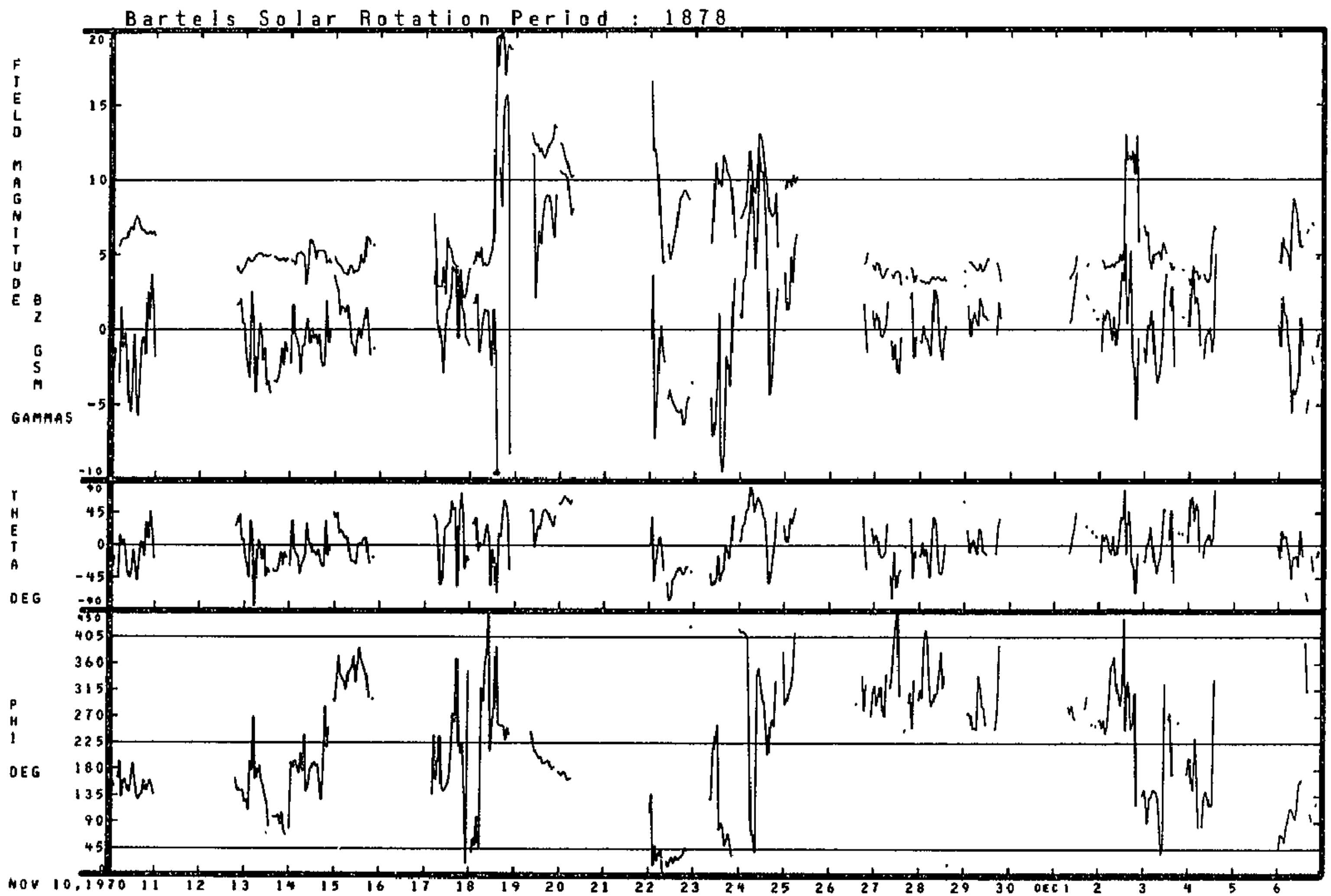
Bartels Solar Rotation Period : 1877



10/14/70 - 11/09/70



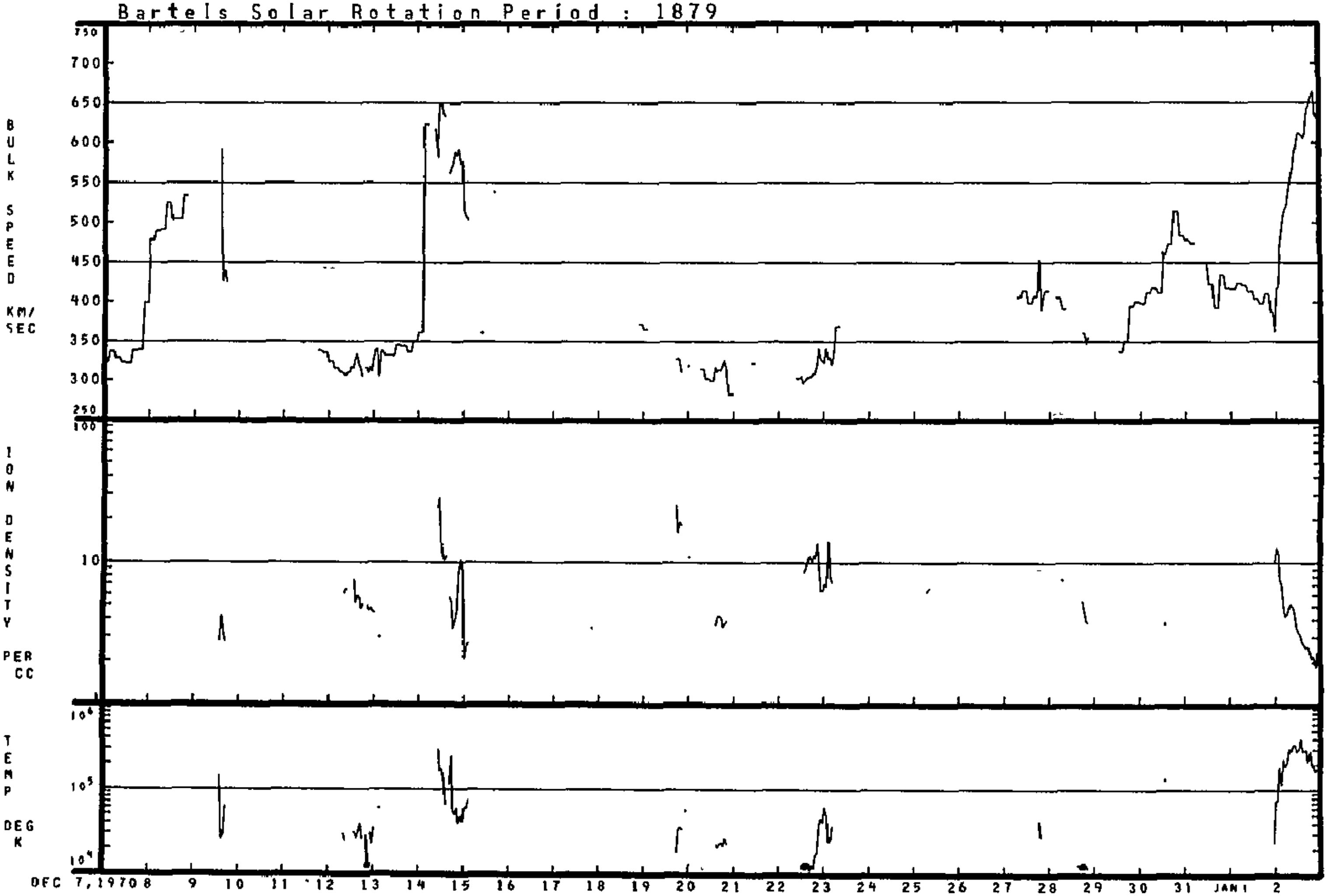
11/10/70 - 12/06/70



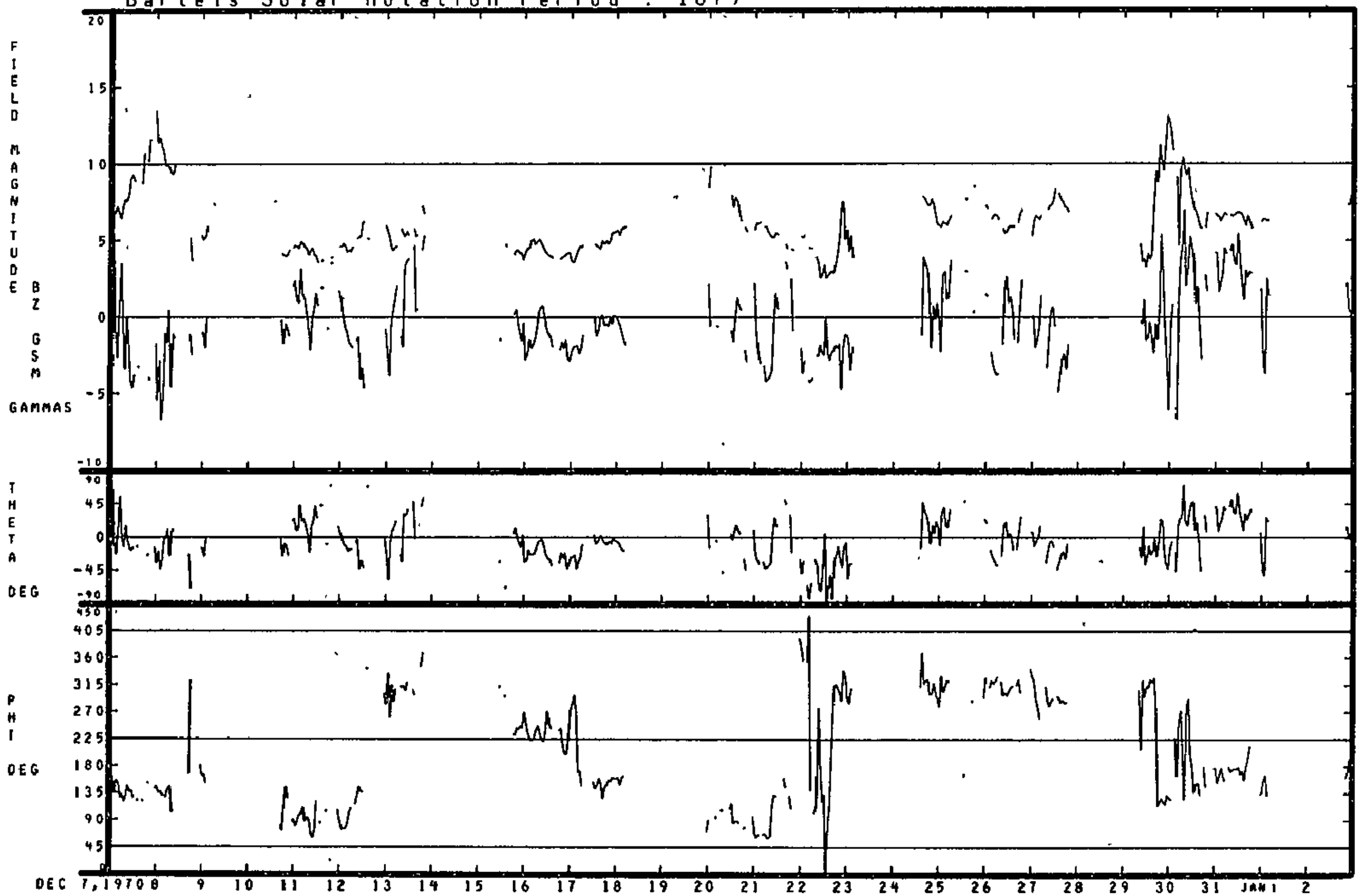
11/10/70 - 12/06/70

12/07/70 - 01/02/71

Bartels Solar Rotation Period : 1879

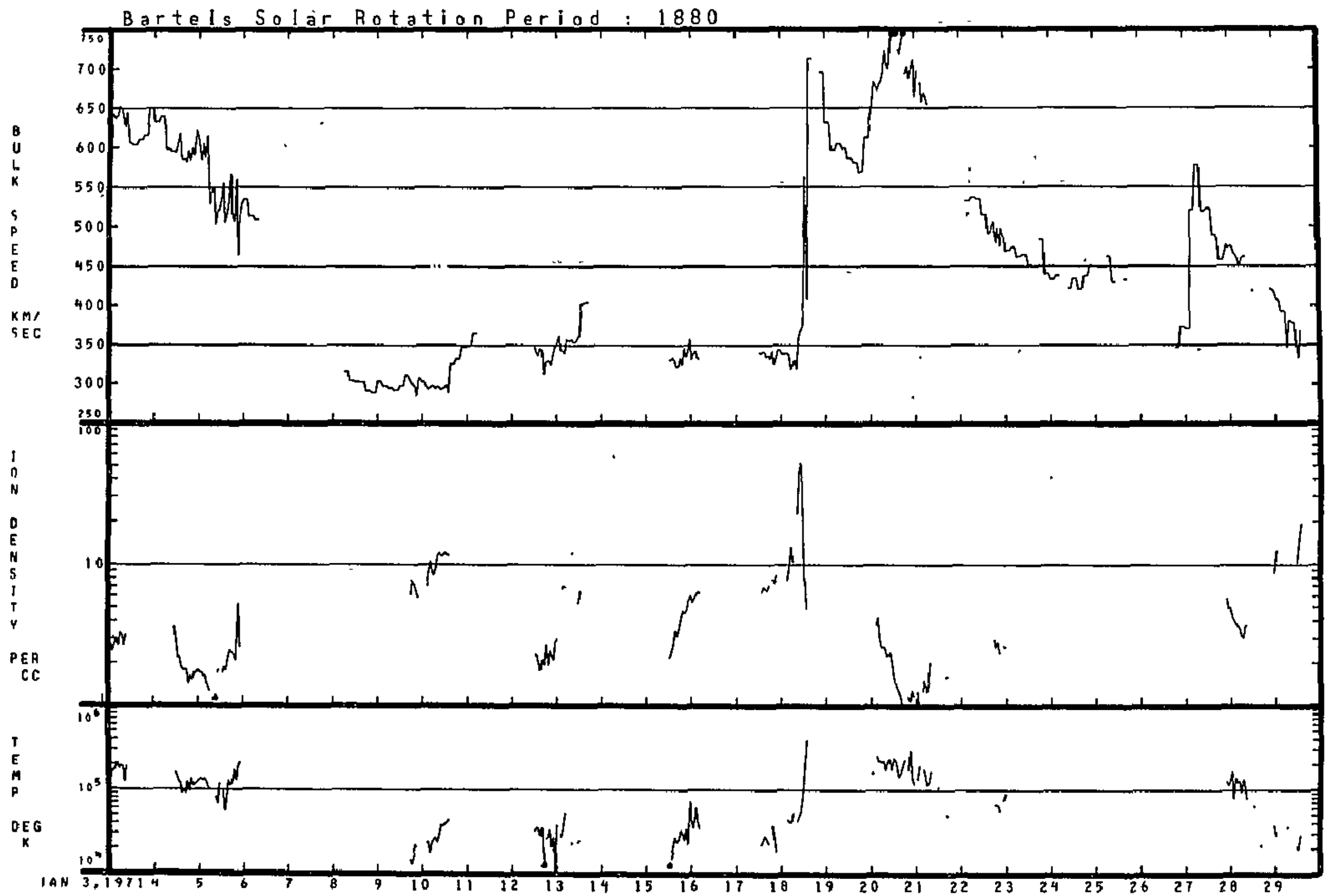


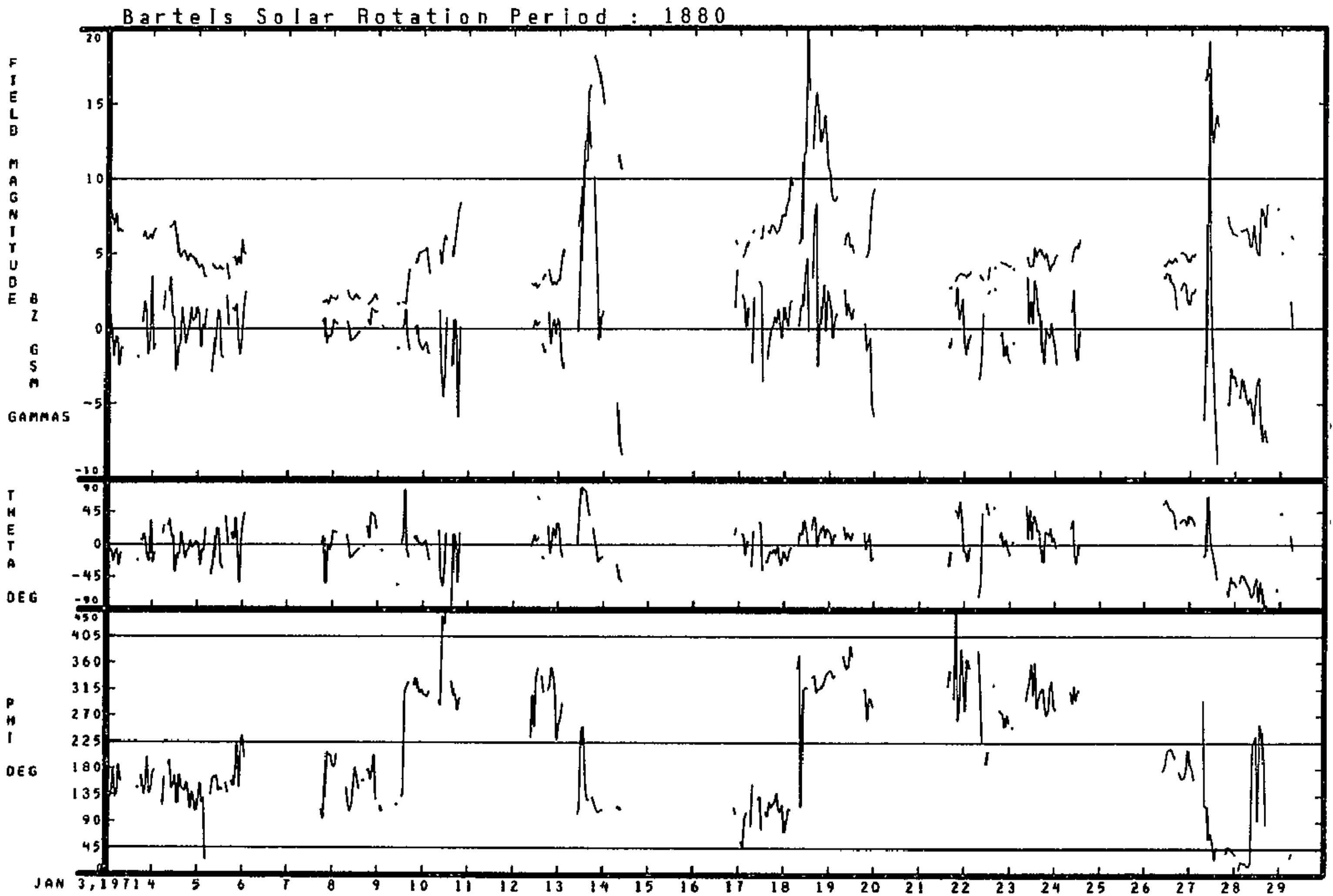
Bartels Solar Rotation Period : 1879



12/07/70 - 01/02/71

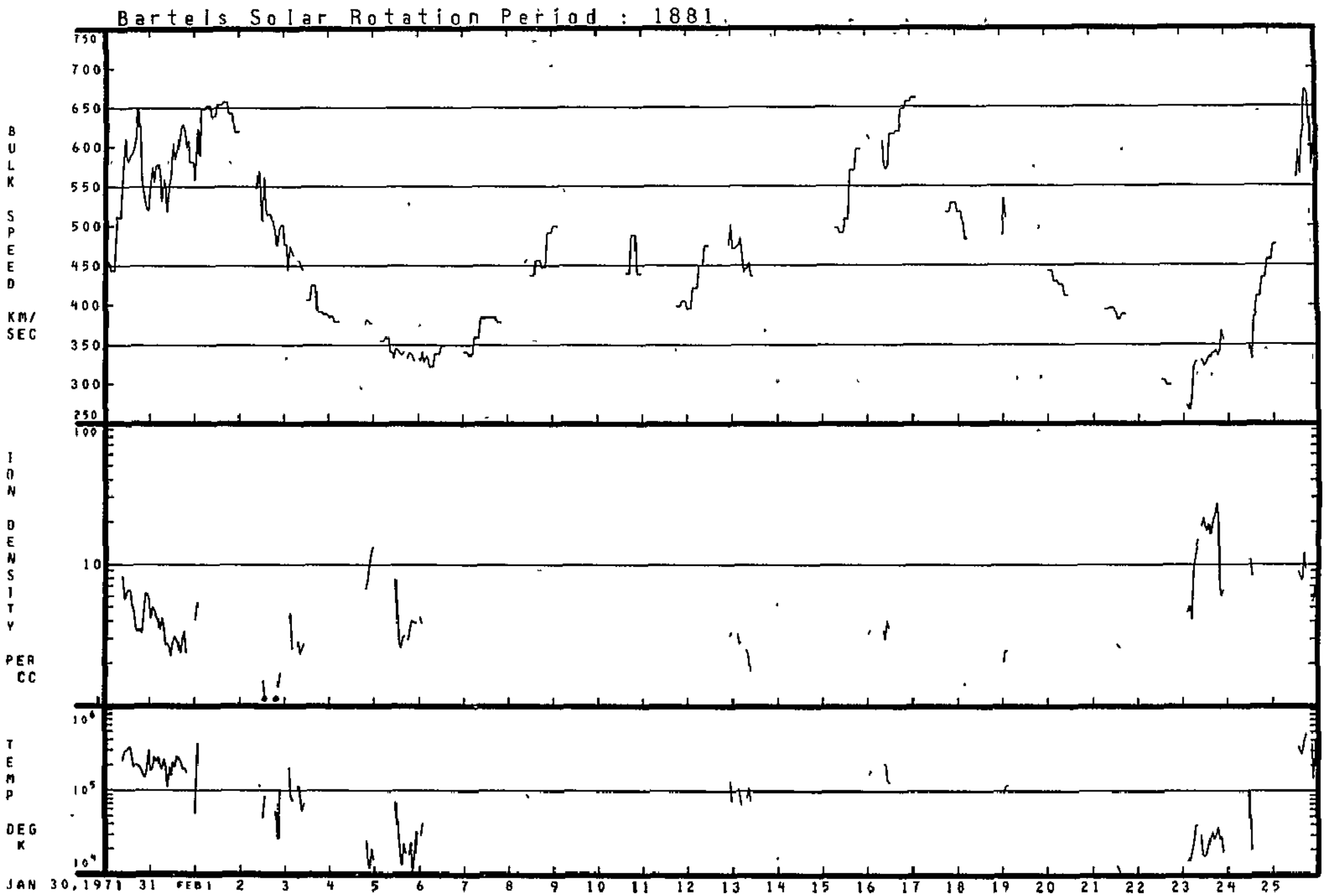
01/03/71 - 01/29/71



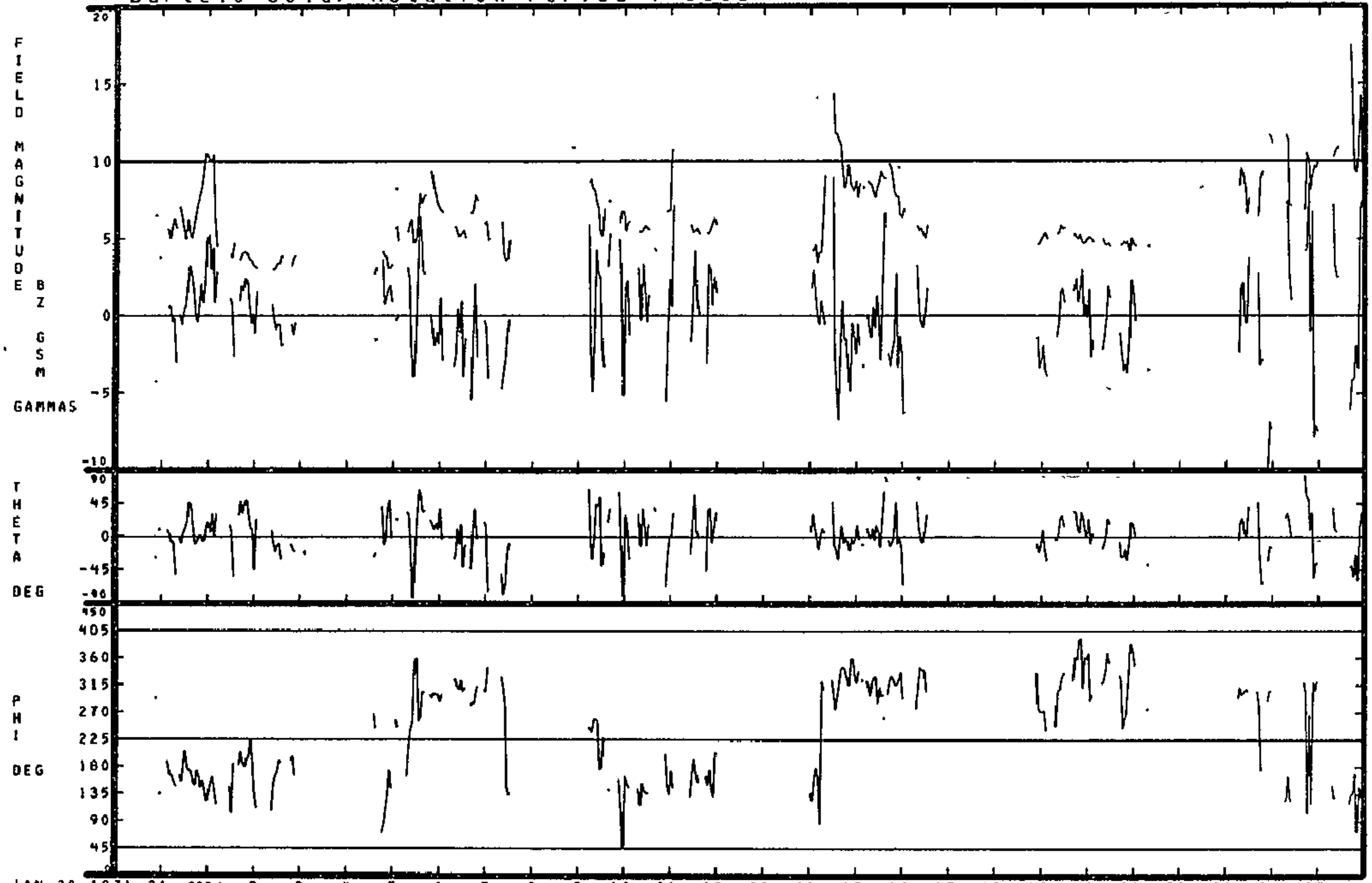


01/03/71 - 01/29/71

01/30/71 - 02/25/71



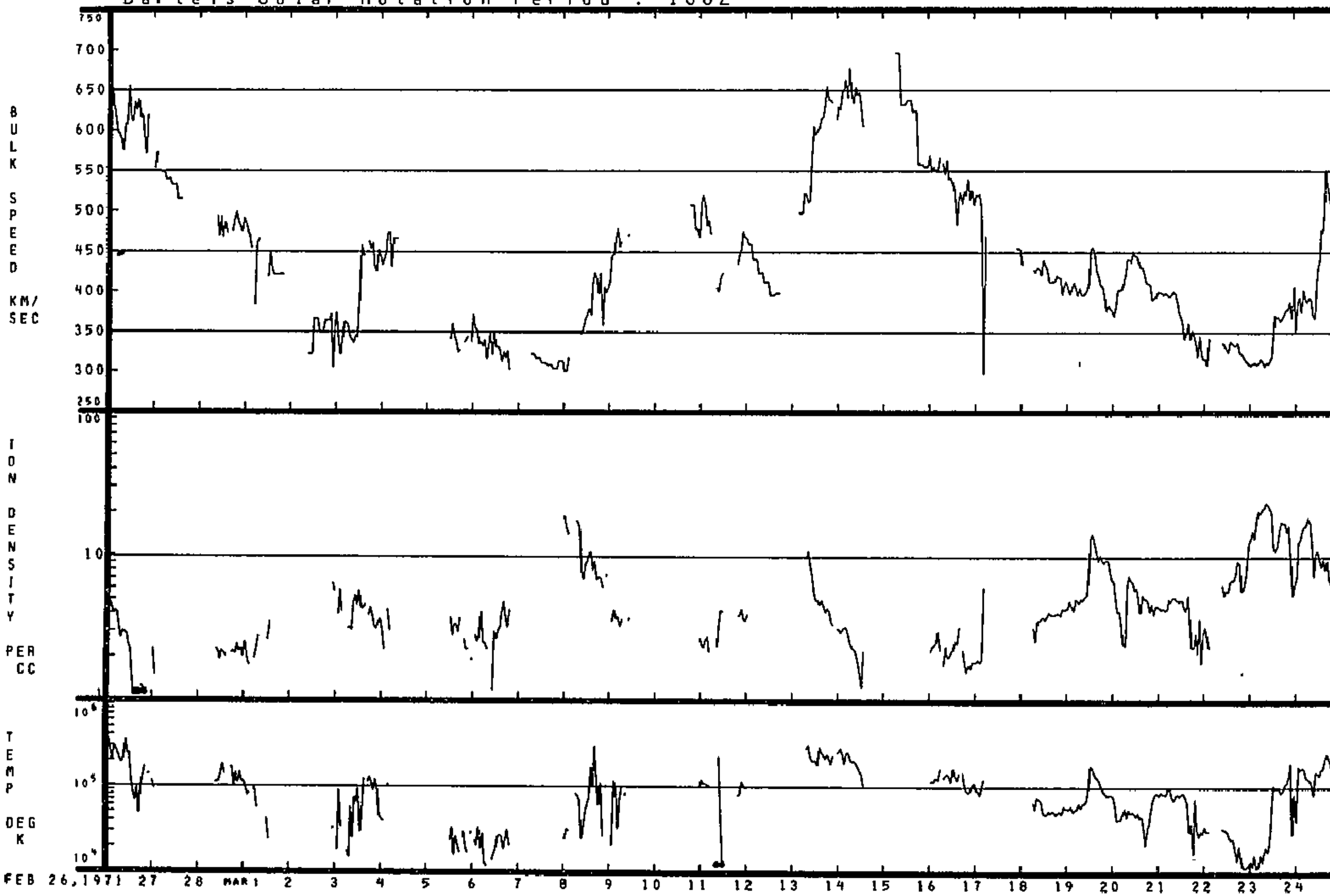
Bartels Solar Rotation Period : 1881

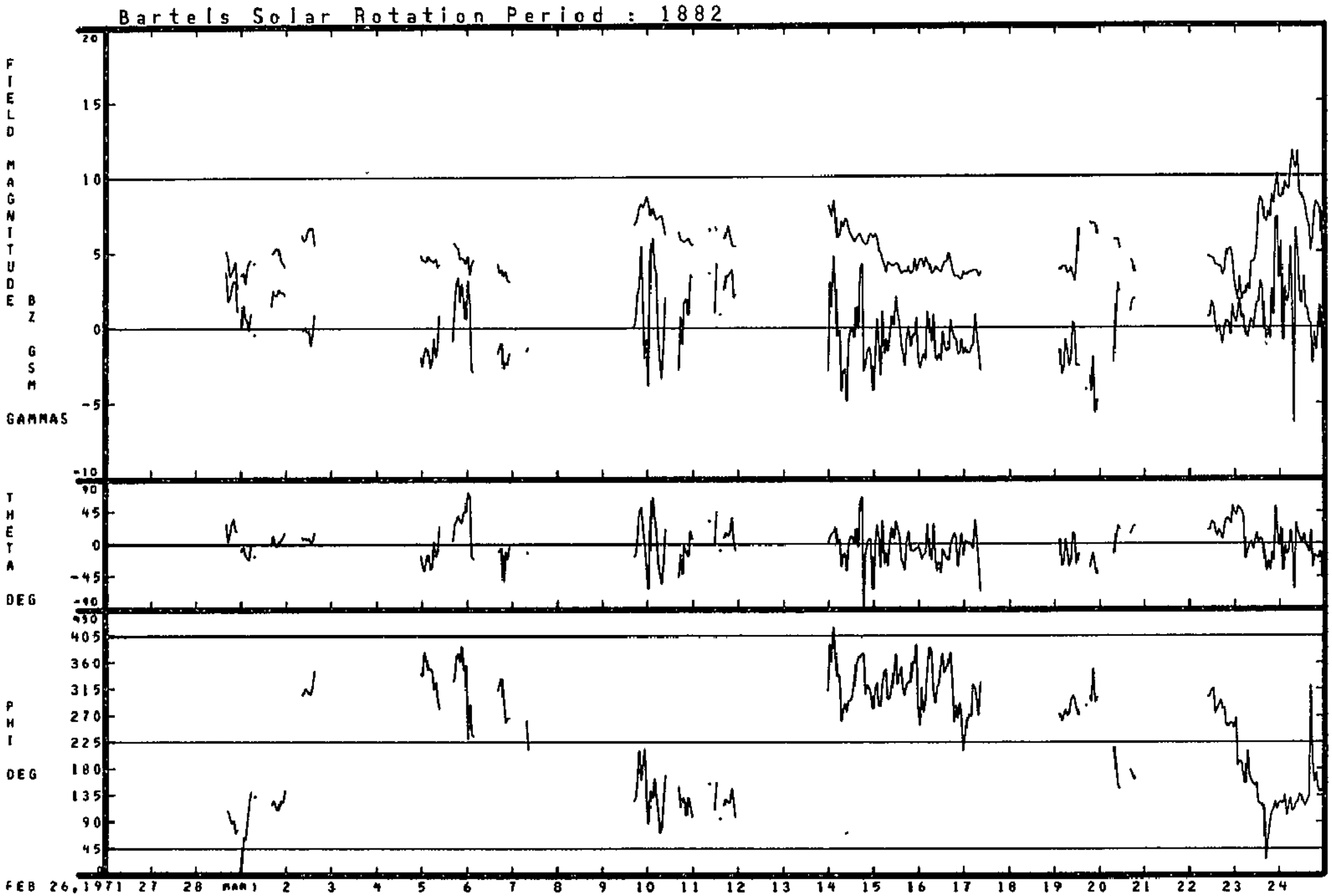


01/30/71 - 02/25/71

02/26/71 - 03/24/71

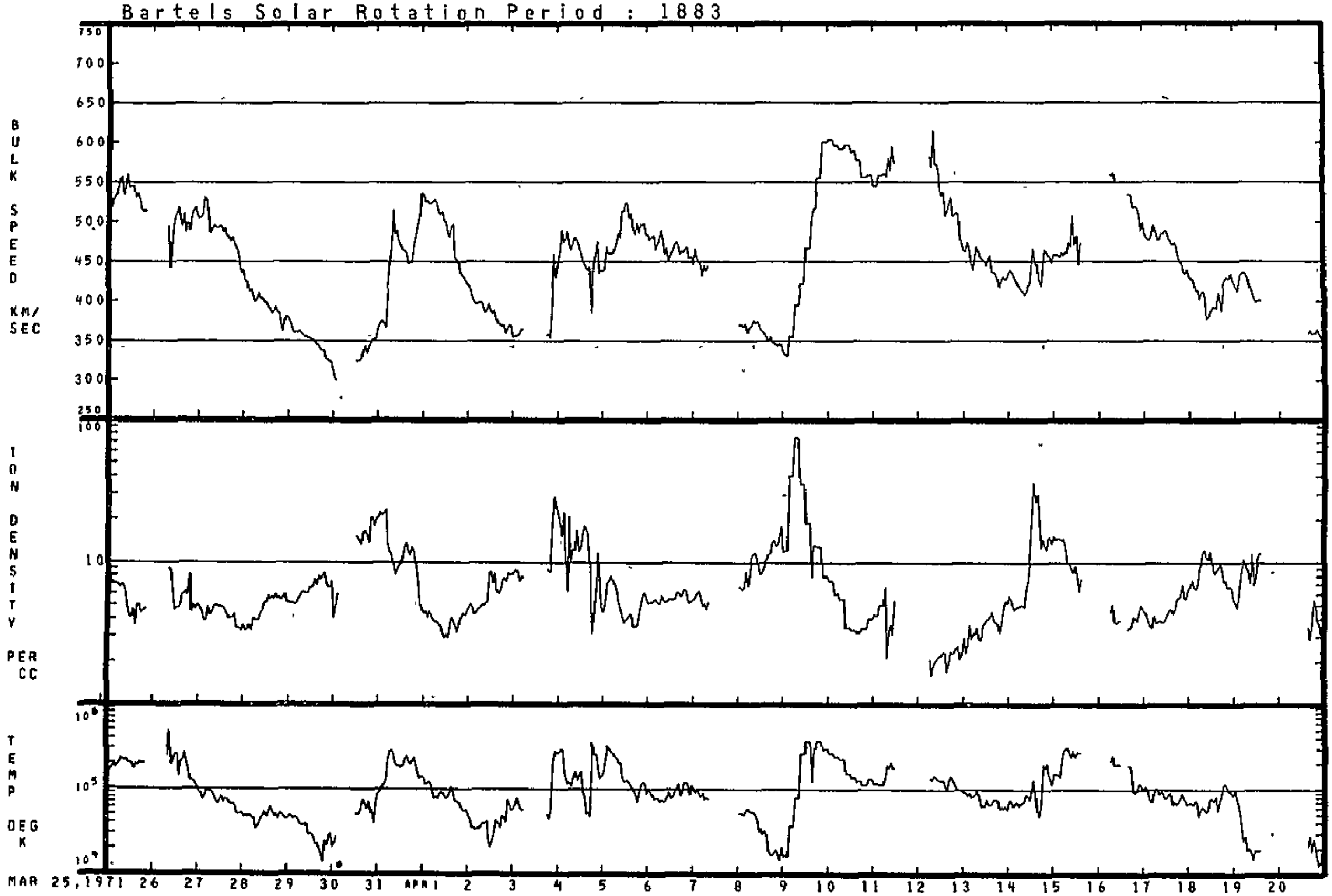
Bartels Solar Rotation Period : 1882




Bartels Solar Rotation Period : 1882
FIELD MAGNITUDE
BZ GSM
GAMMAS
THETA
DEG
PHI
DEG
FEB 26,1971 27 28 MAR 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24
02/26/71 - 03/24/71

03/25/71 - 04/20/71

Bartels Solar Rotation Period : 1883



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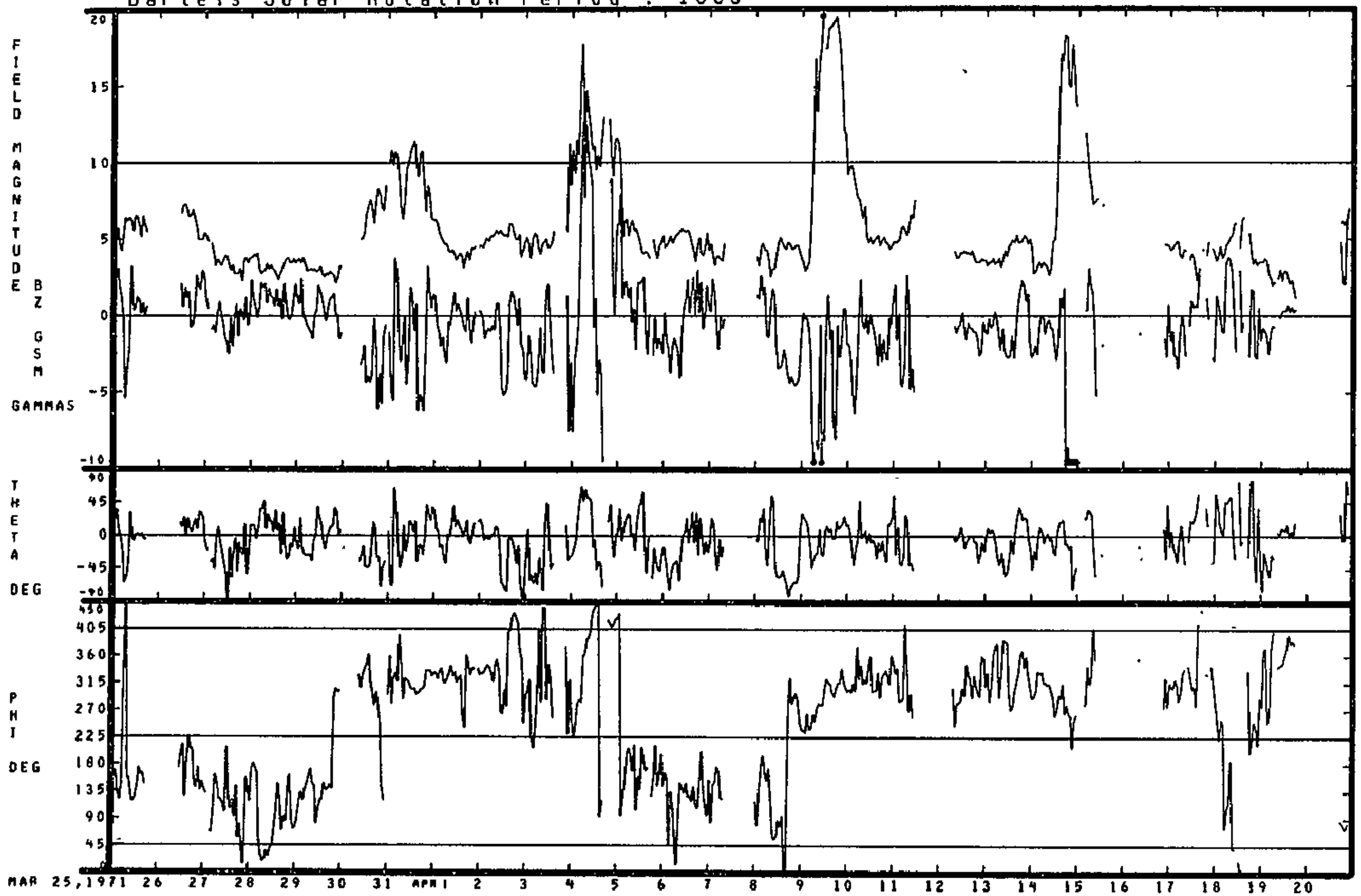
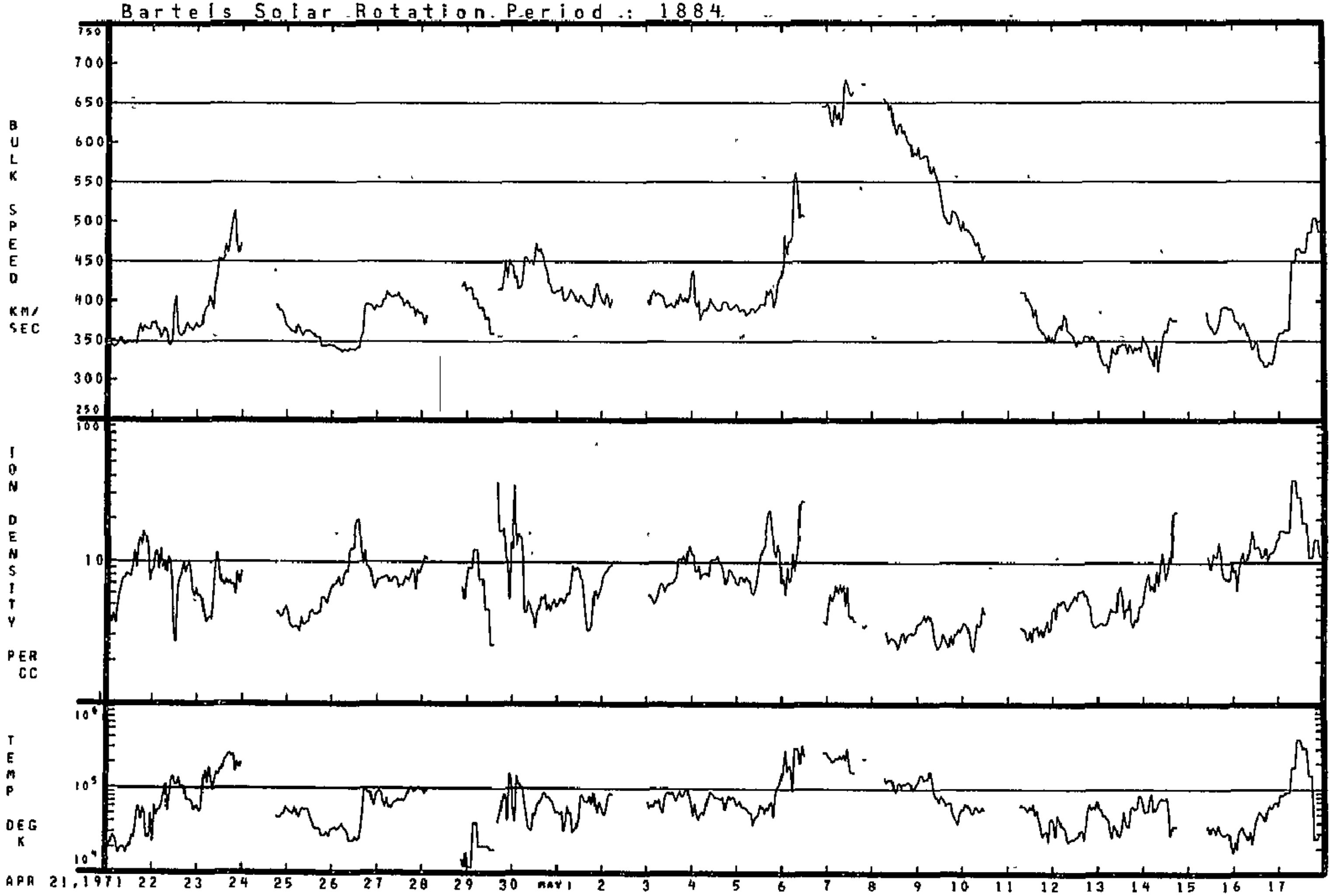
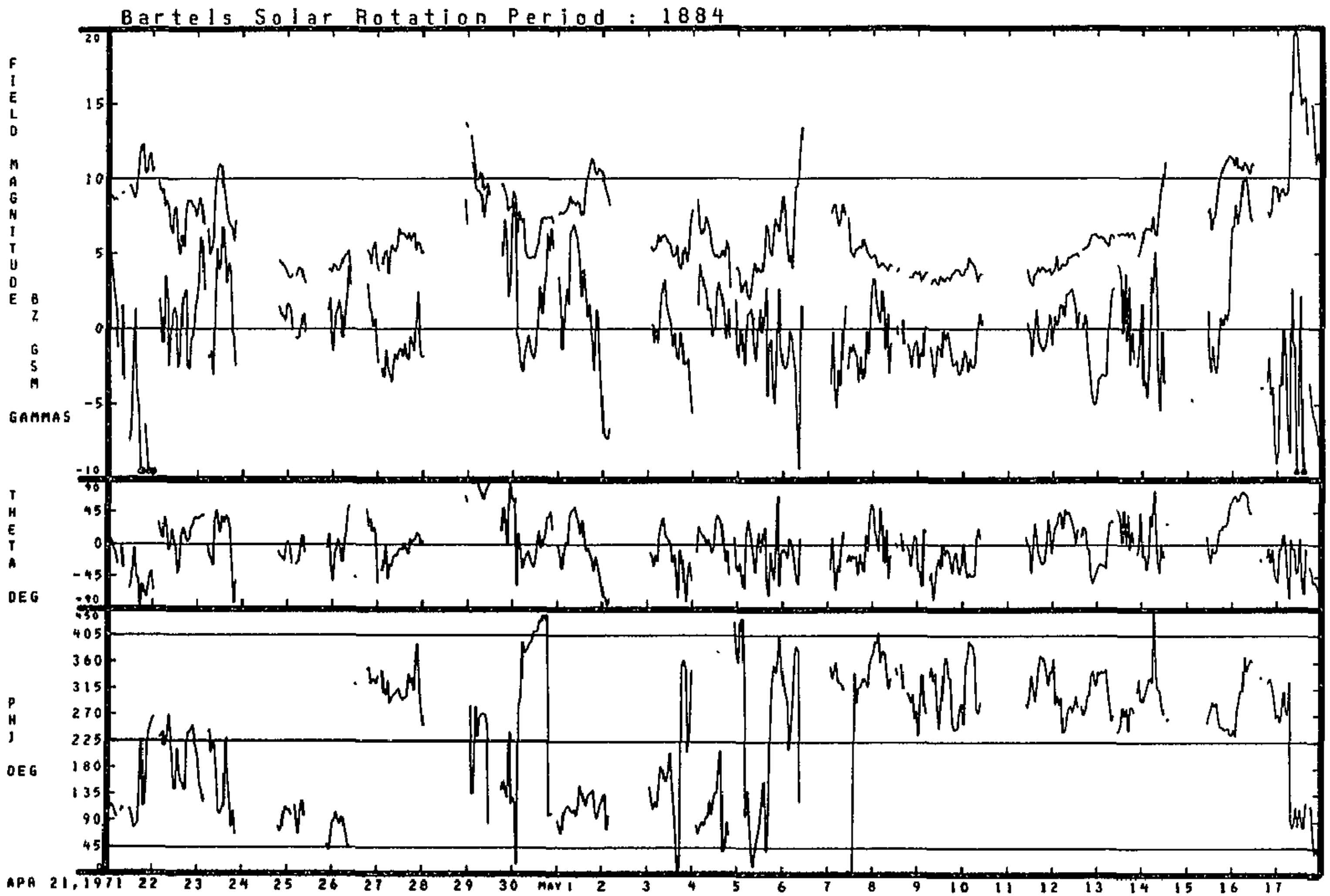


03/25/71 - 04/20/71

04/21/71 - 05/17/71

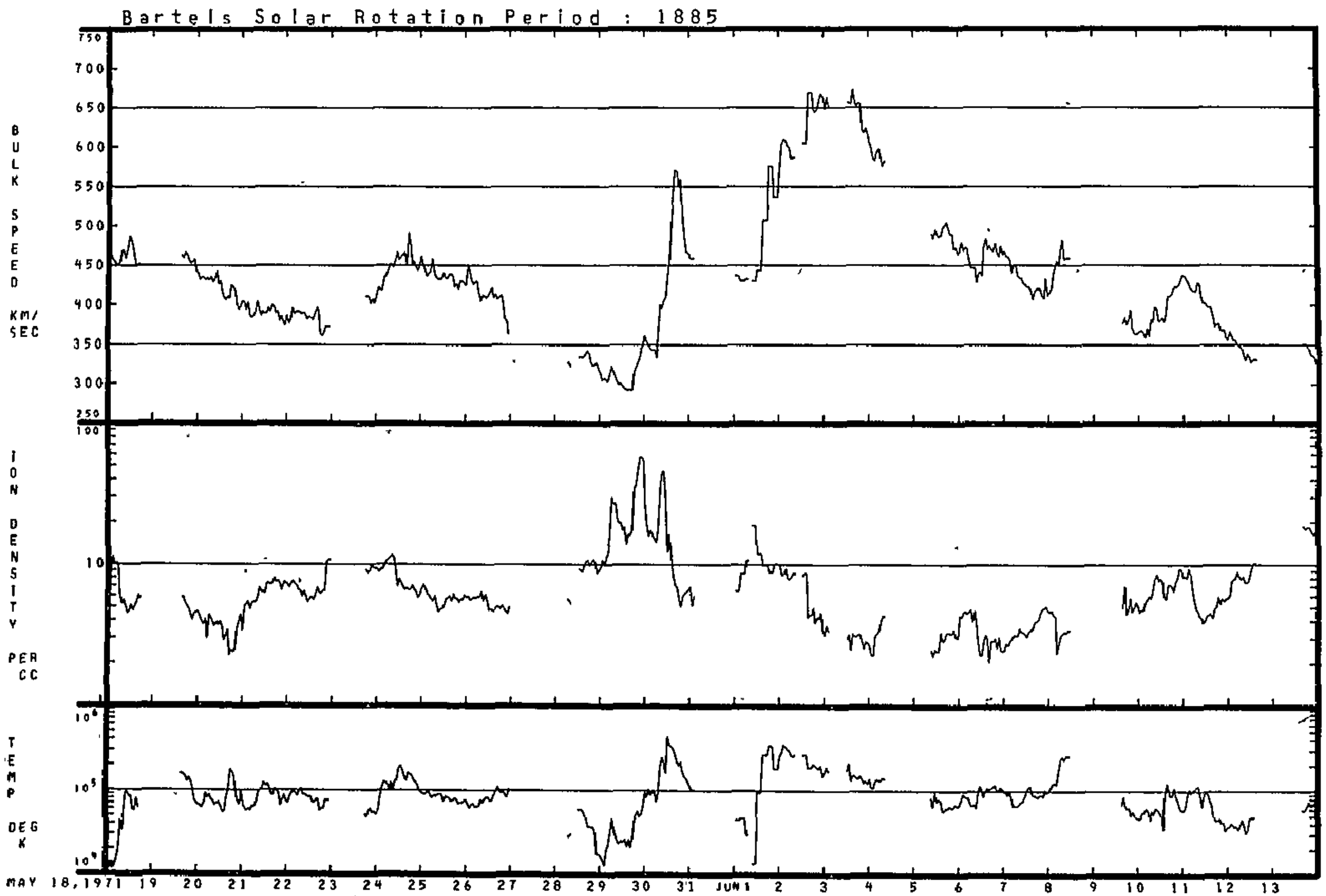
Bartels Solar Rotation Period : 1884



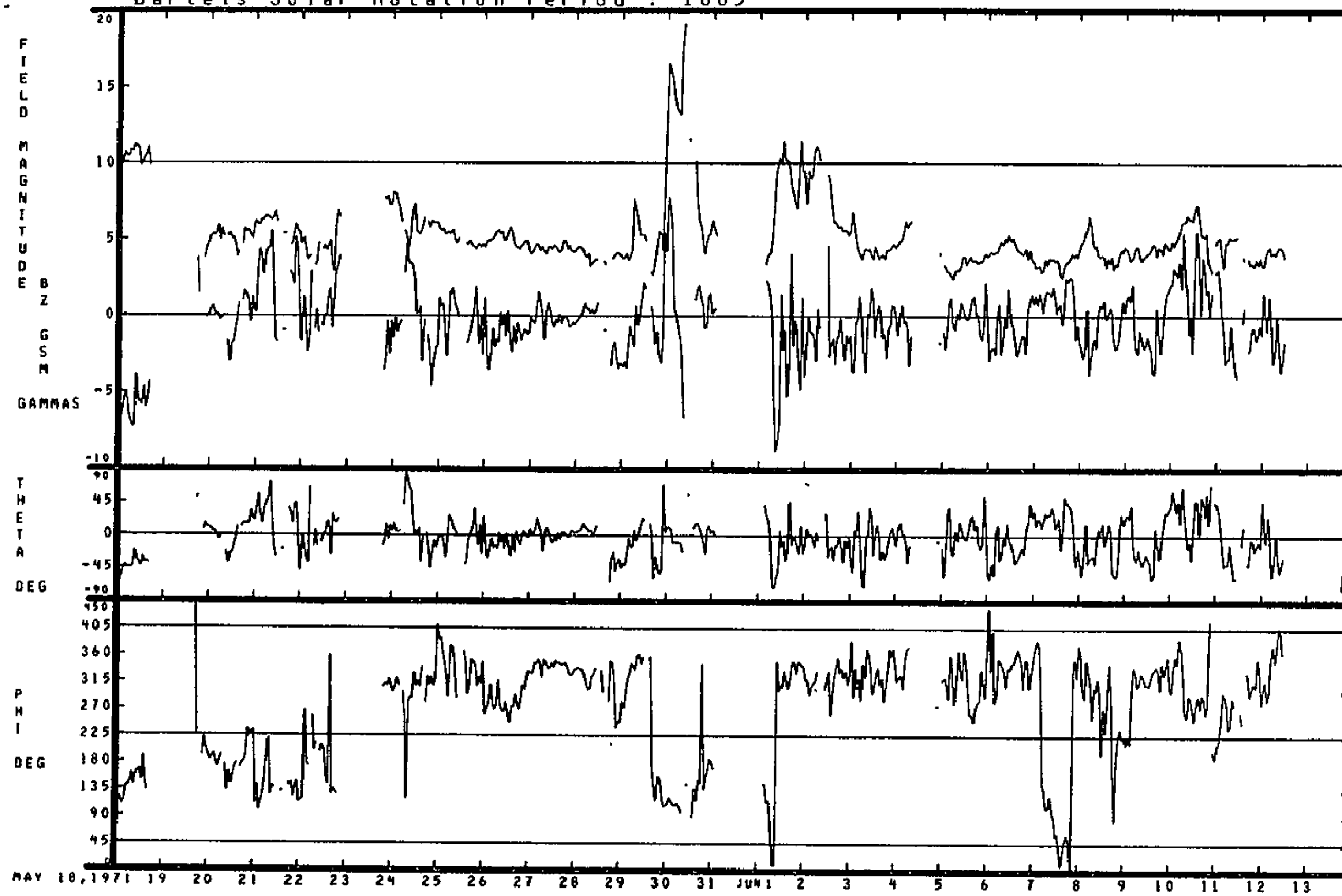


04/21/71 - 05/17/71

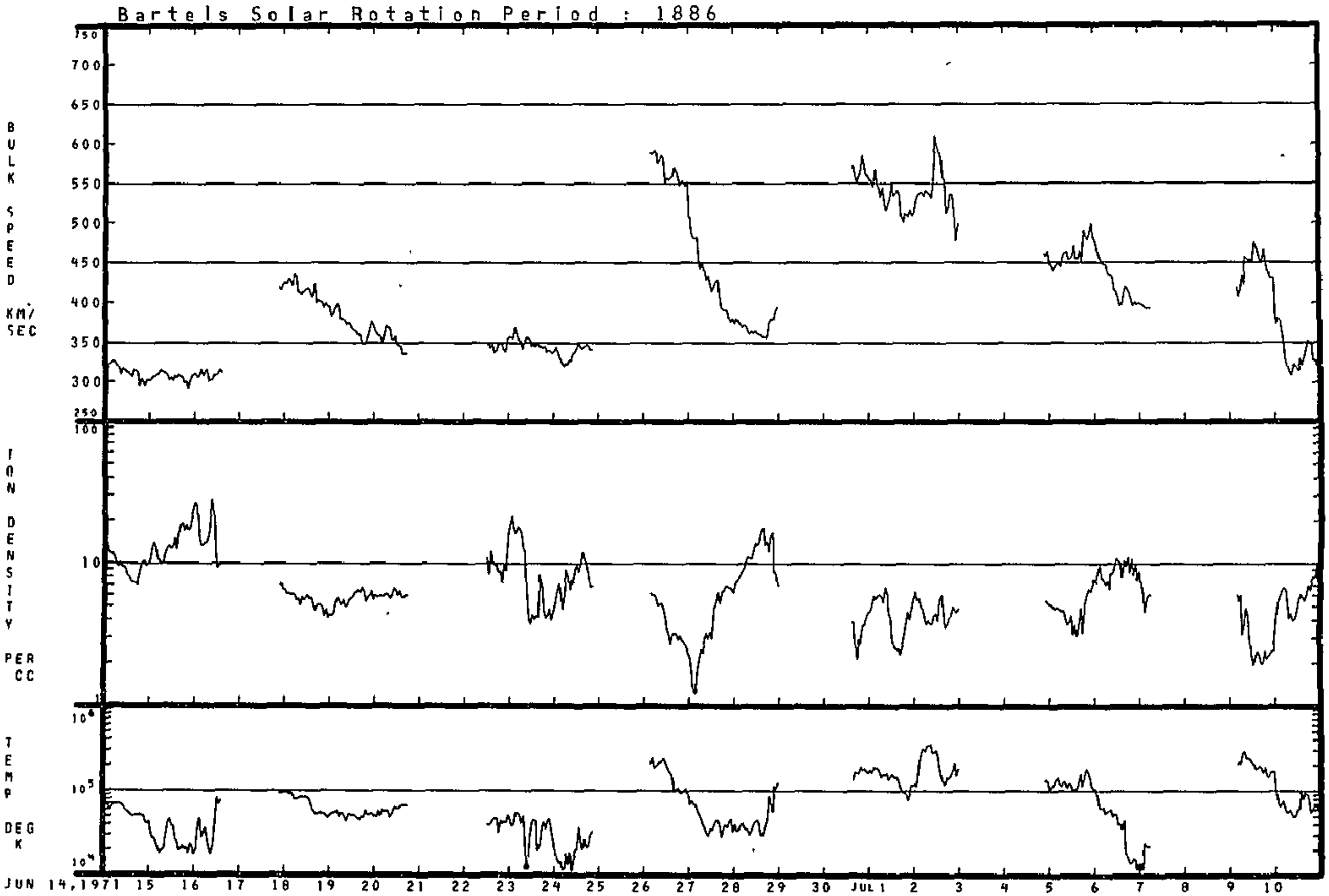
05/18/71 - 06/13/71



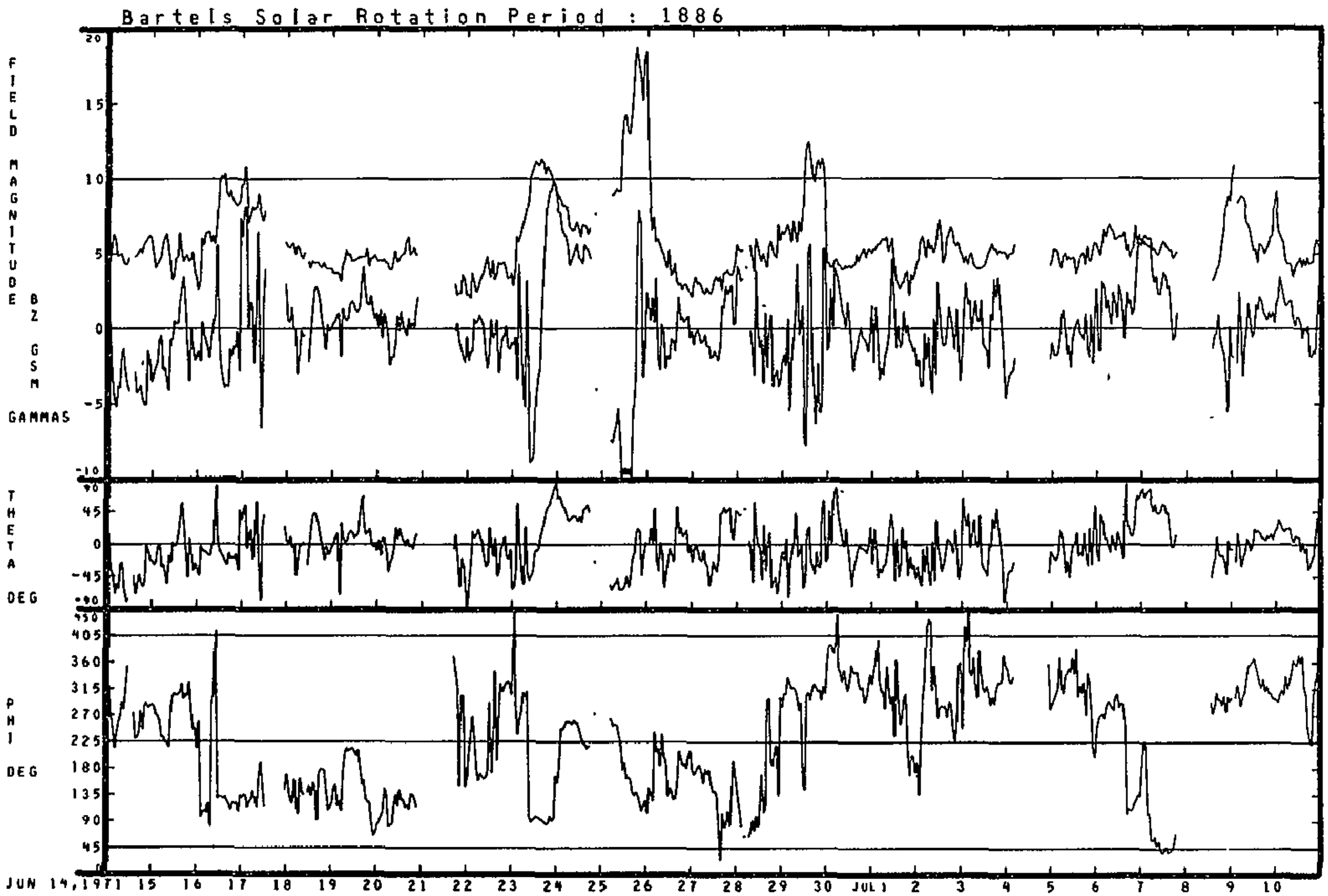
Bartels Solar Rotation Period : 1885



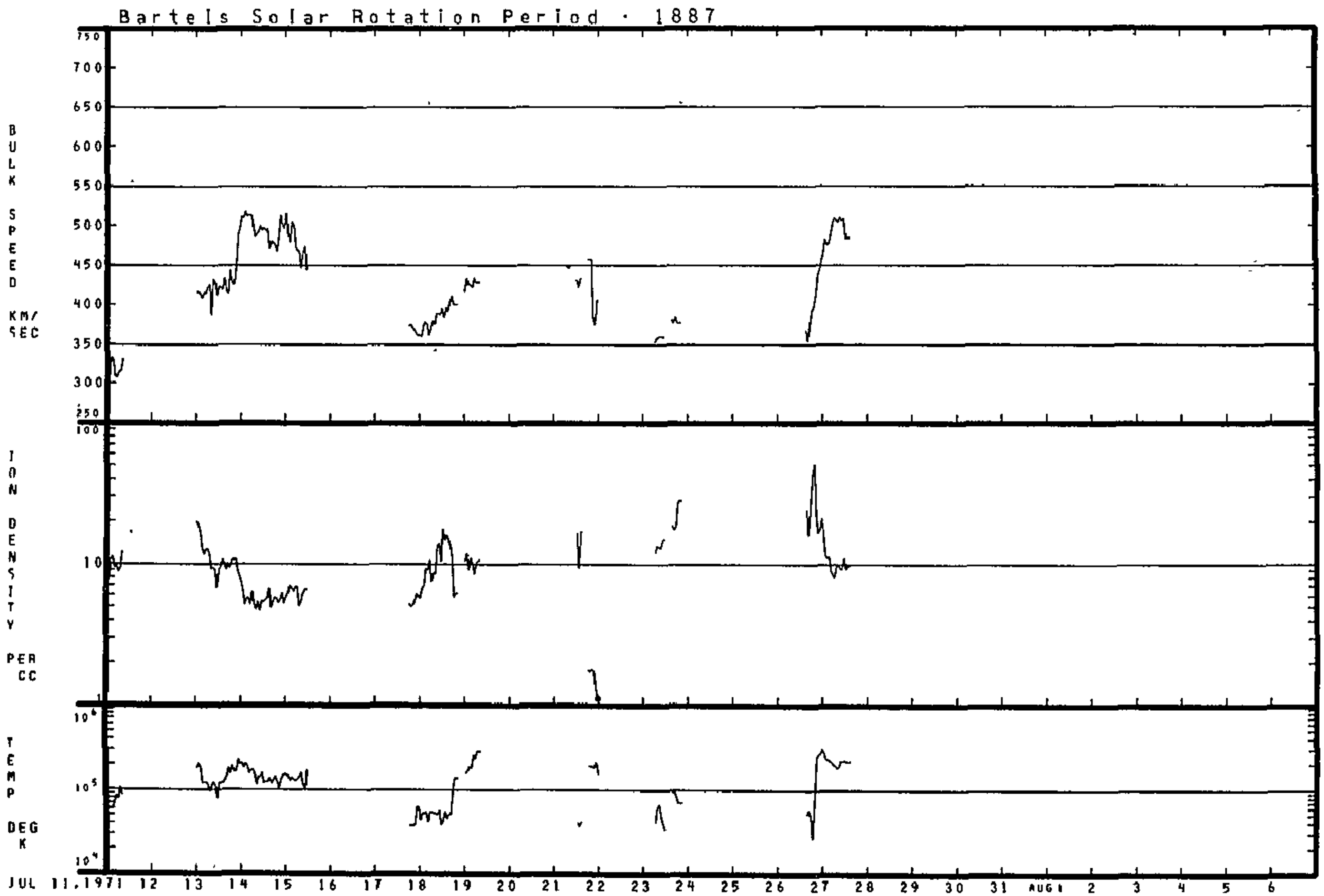
05/18/71 - 06/13/71



06/14/71 - 07/10/71

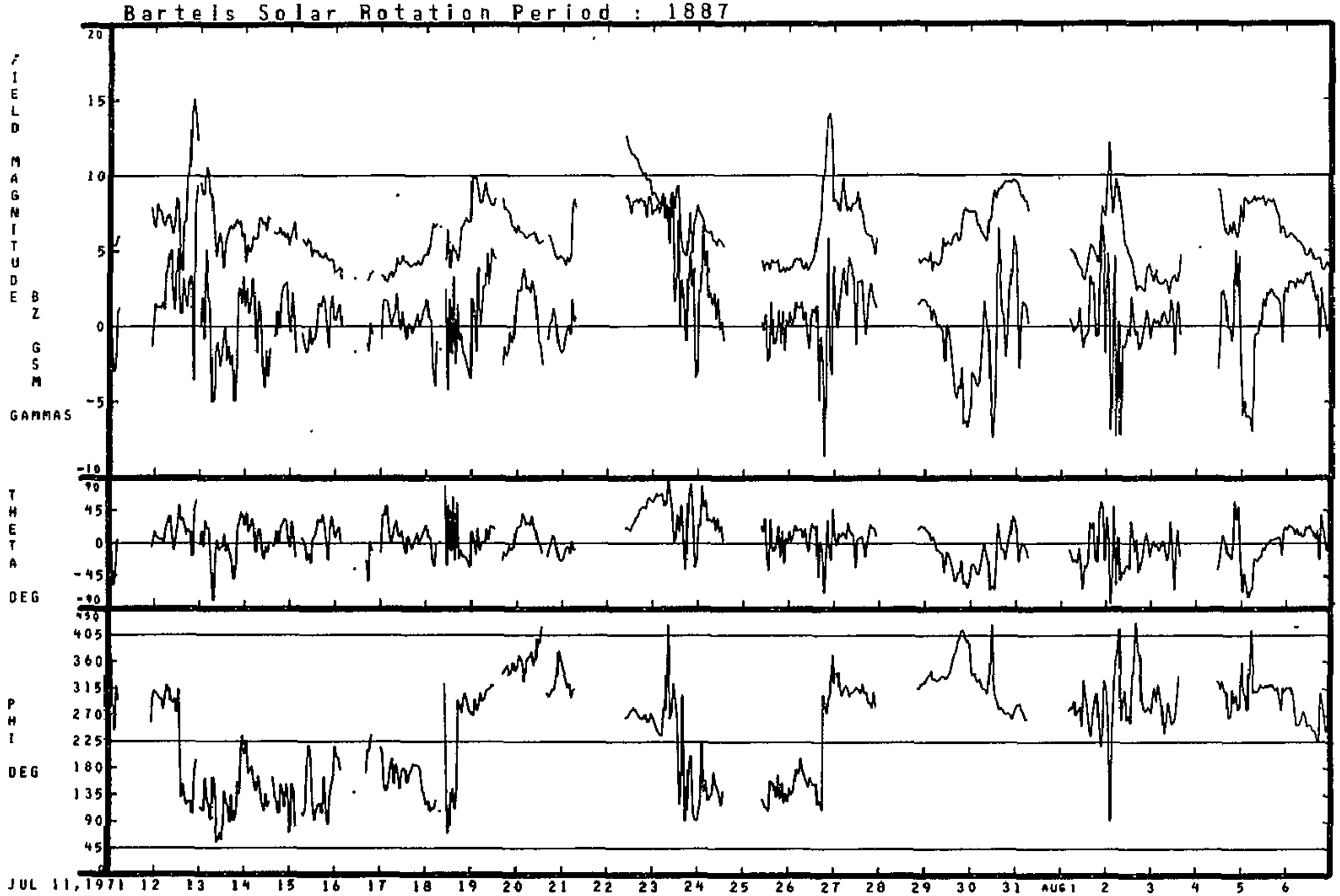


06/14/71 - 07/10/71



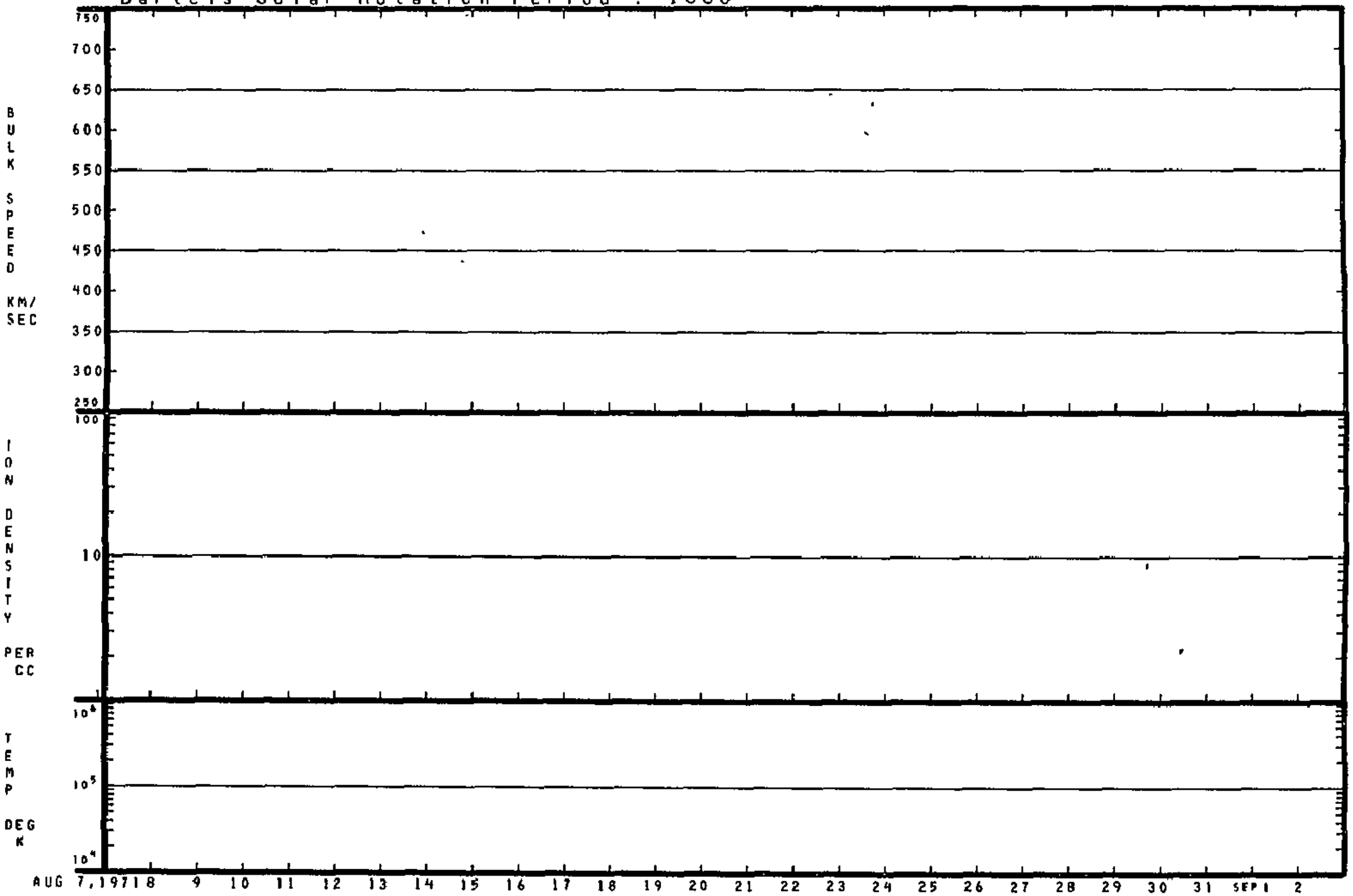
07/11/71 - 08/06/71

Bartels Solar Rotation Period : 1887



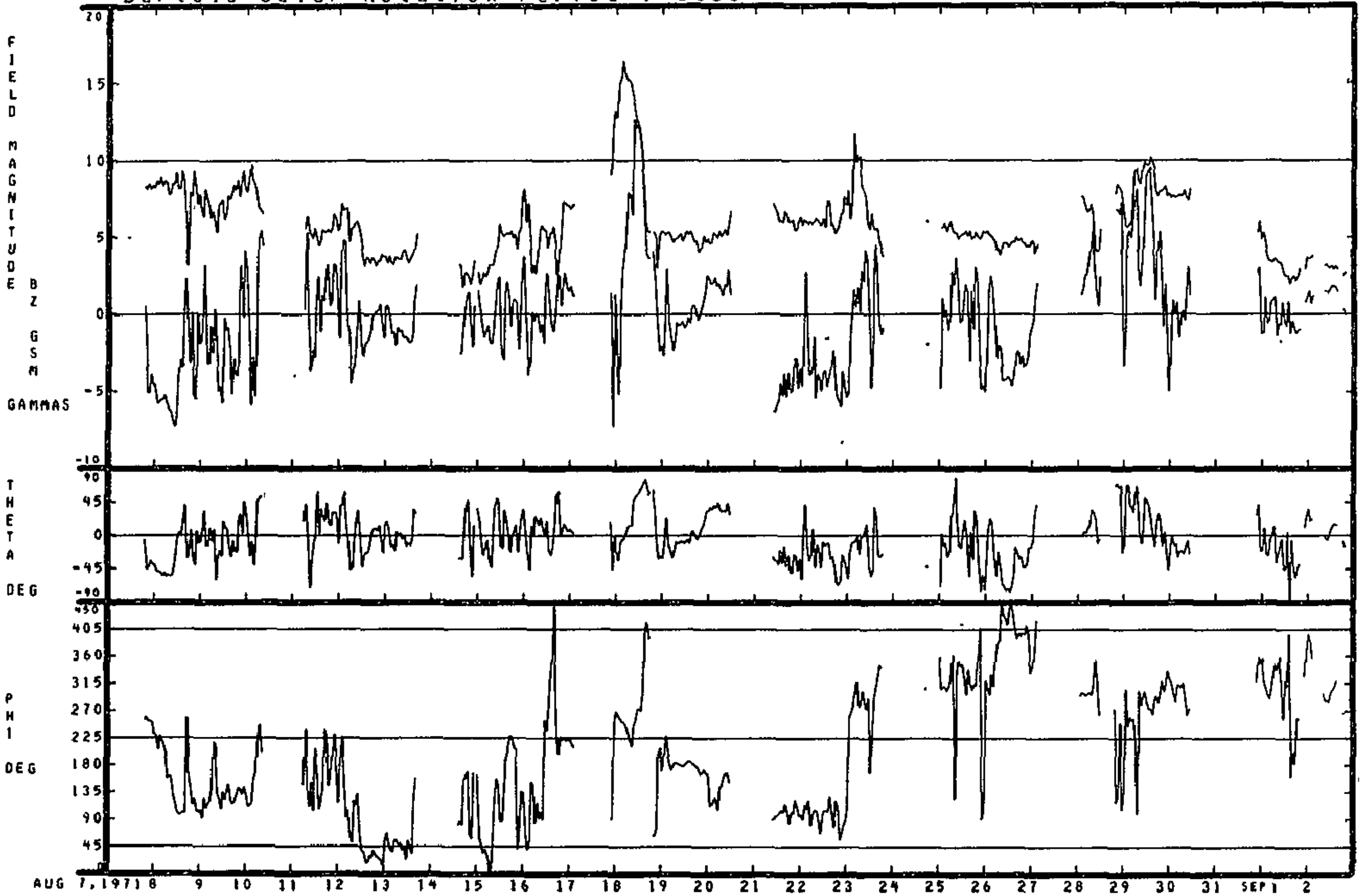
07/11/71 - 08/06/71

Bartels Solar Rotation Period : 1888



08/07/71 - 09/02/71

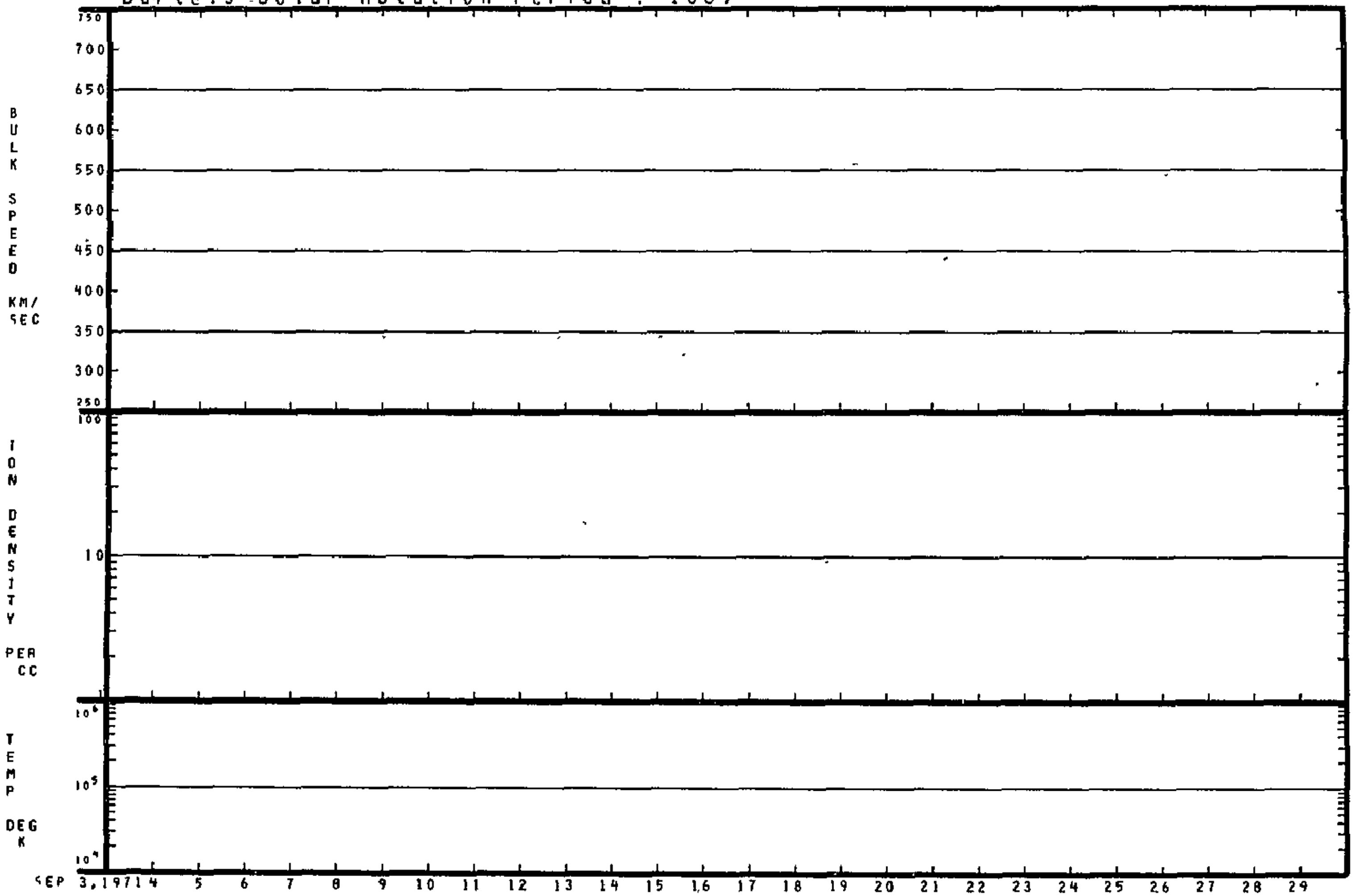
Bartels Solar Rotation Period : 1888



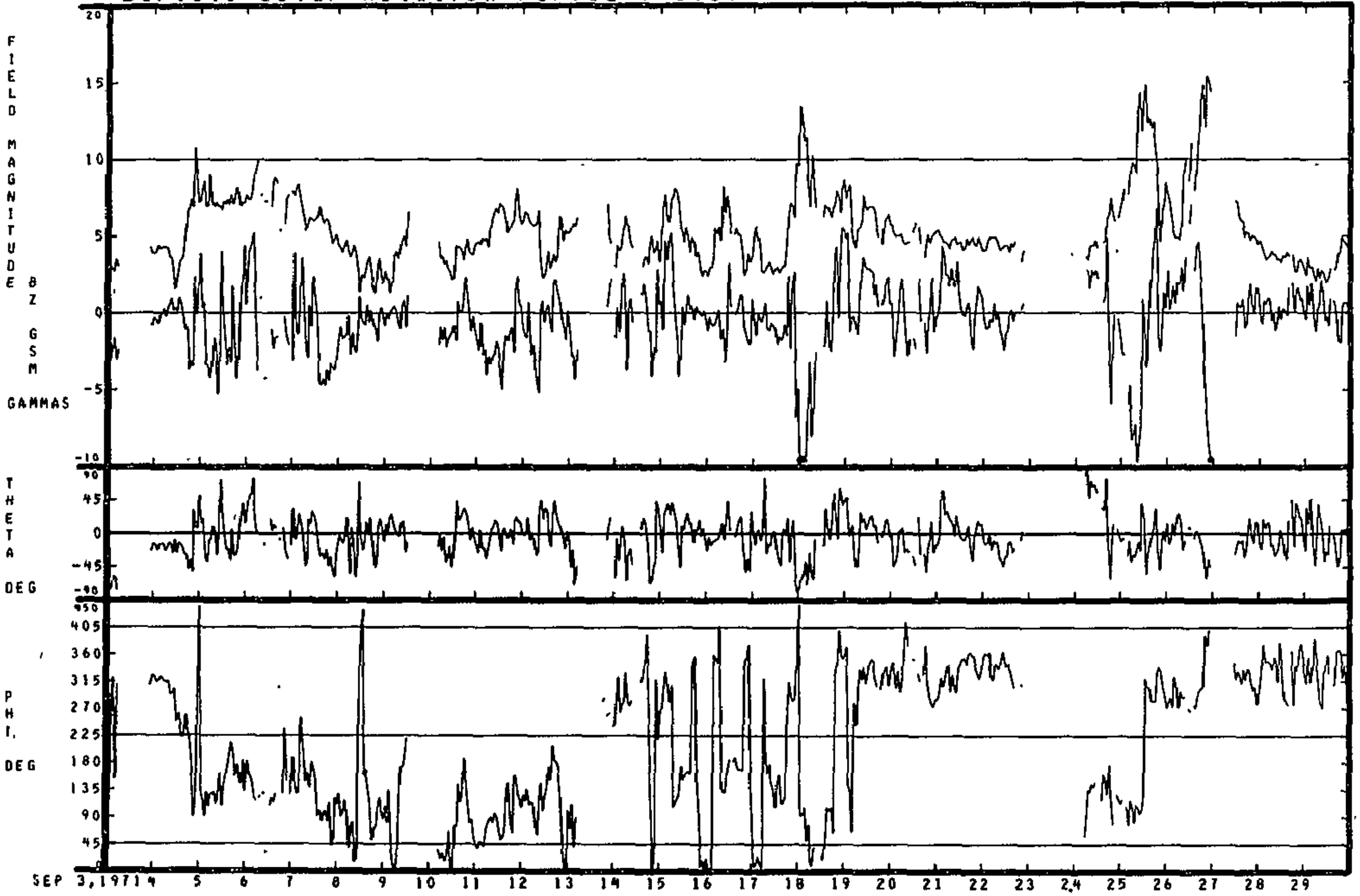
08/07/71 - 09/02/71

09/03/71 - 09/29/71

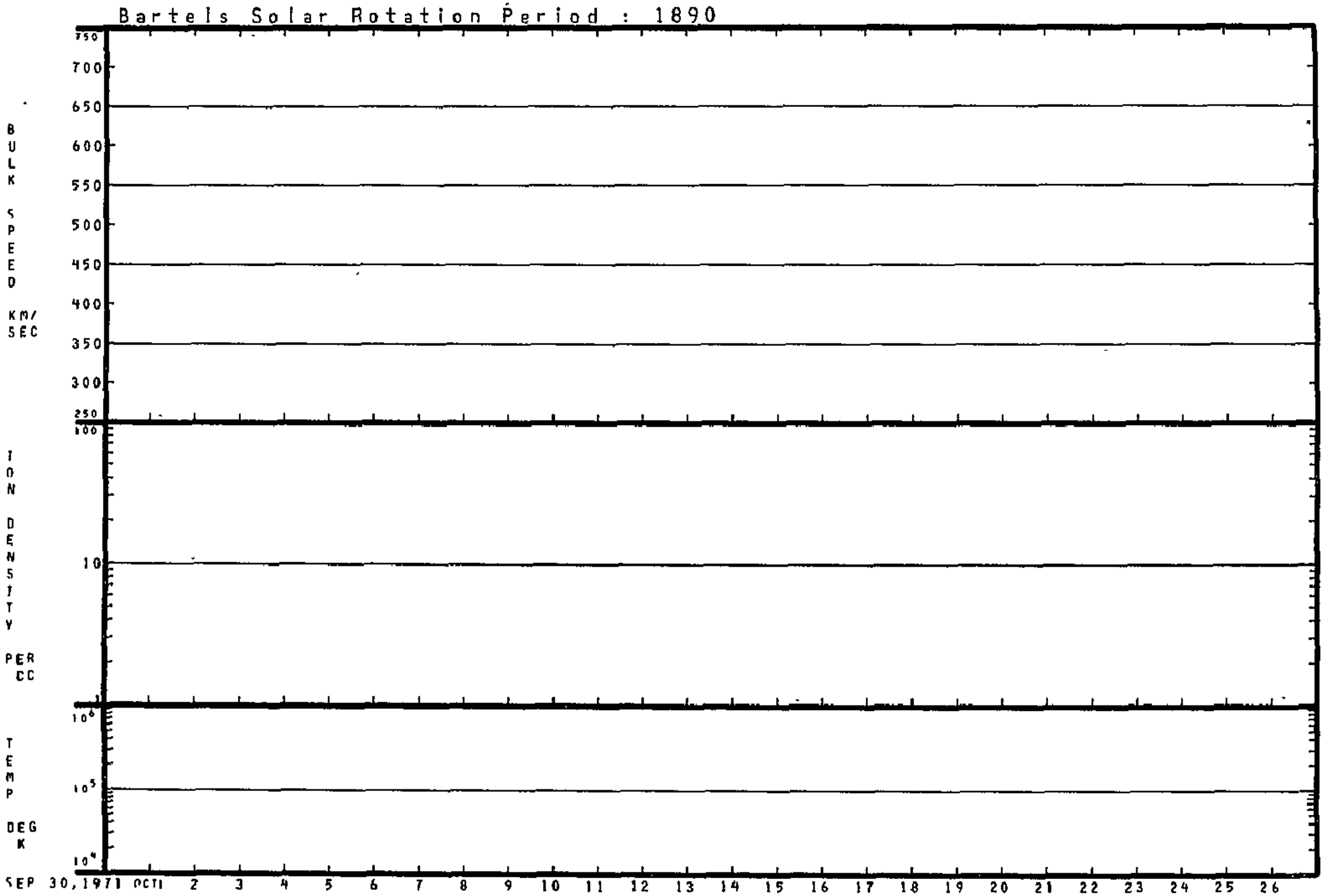
Bartels Solar Rotation Period : 1889



Bartels Solar Rotation Period : 1889

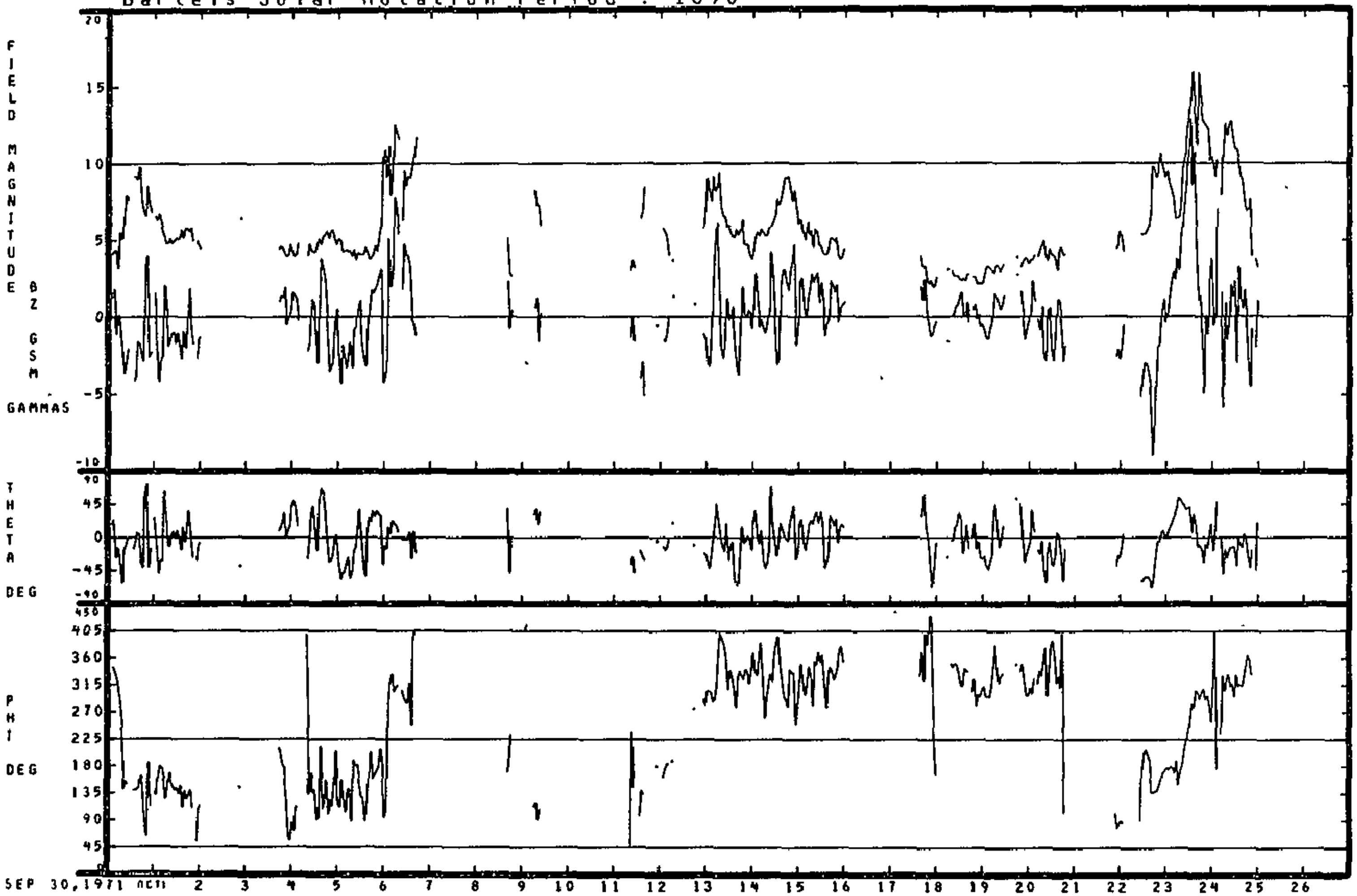


09/03/71 - 09/29/71



09/30/71 - 10/26/71

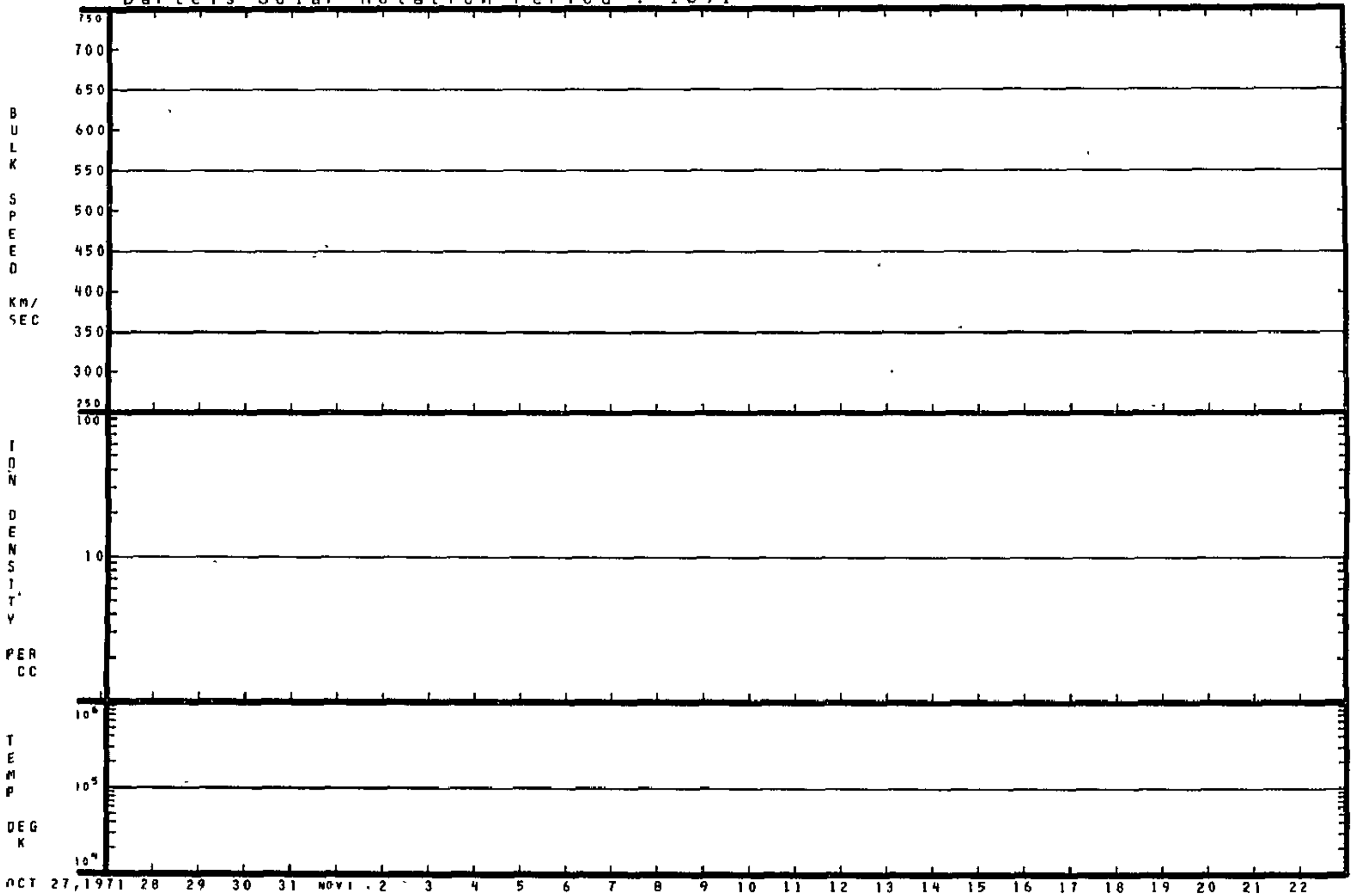
Bartels Solar Rotation Period : 1890

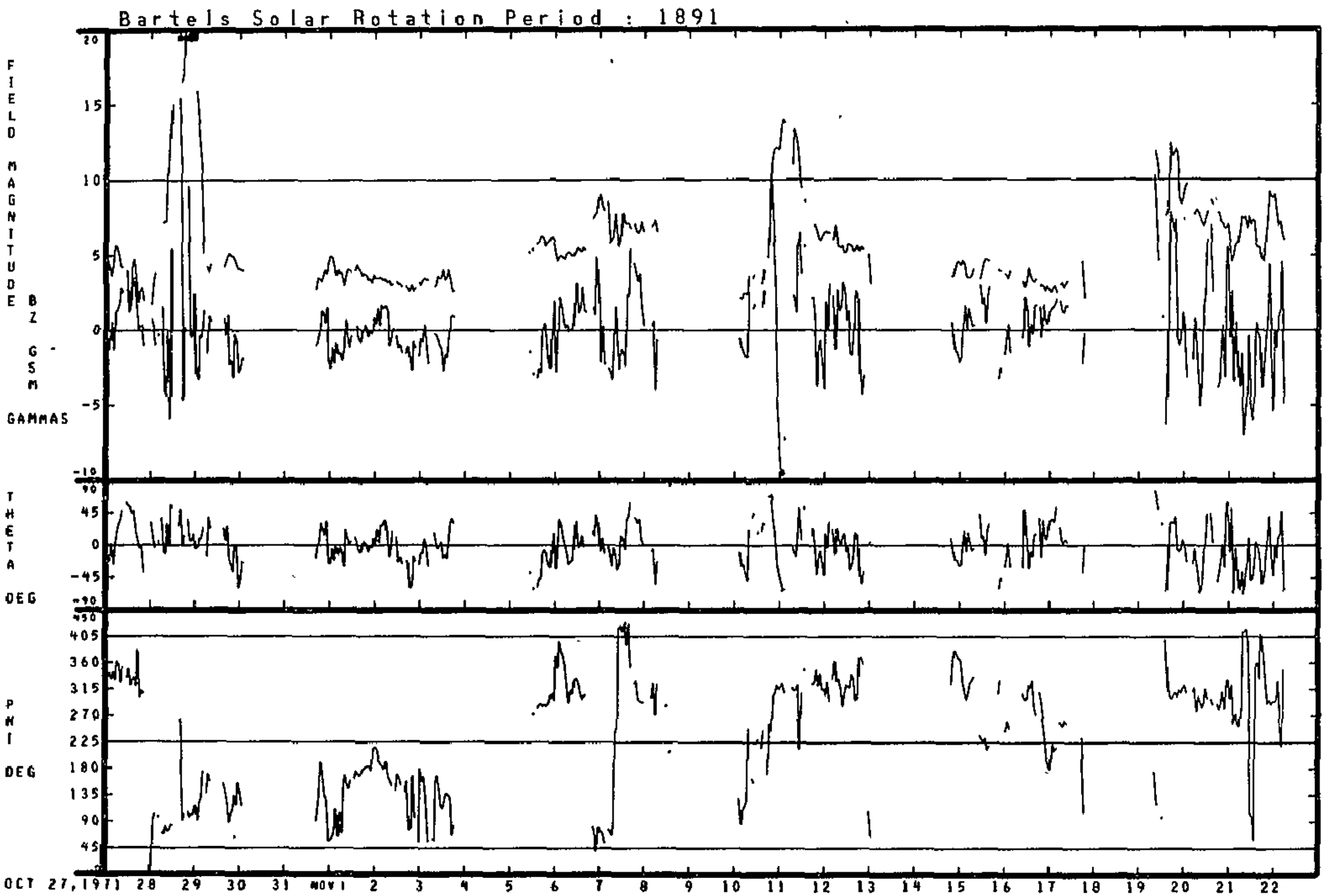


09/30/71 - 10/26/71

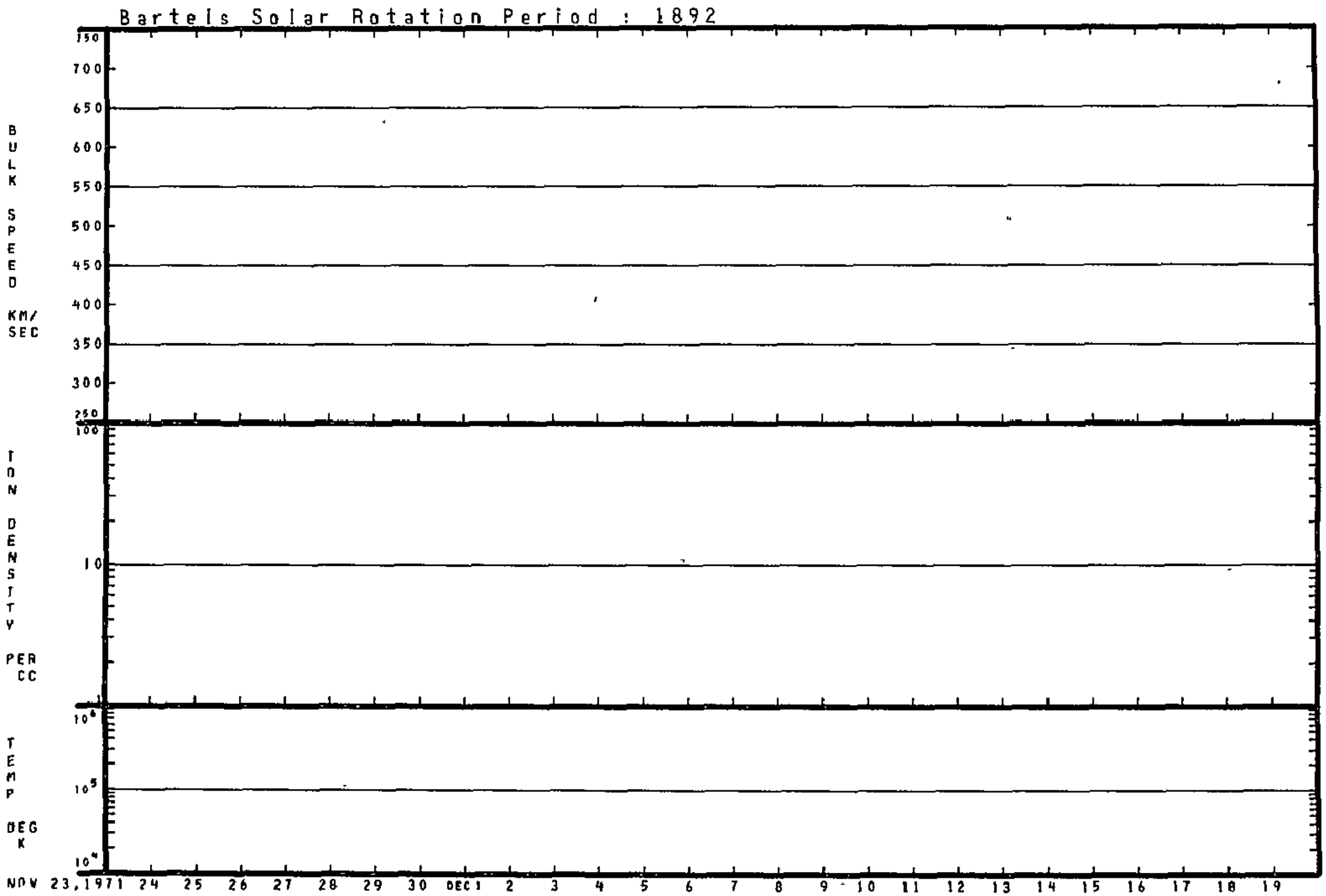
10/27/71 - 11/22/71

Bartels Solar Rotation Period : 1891

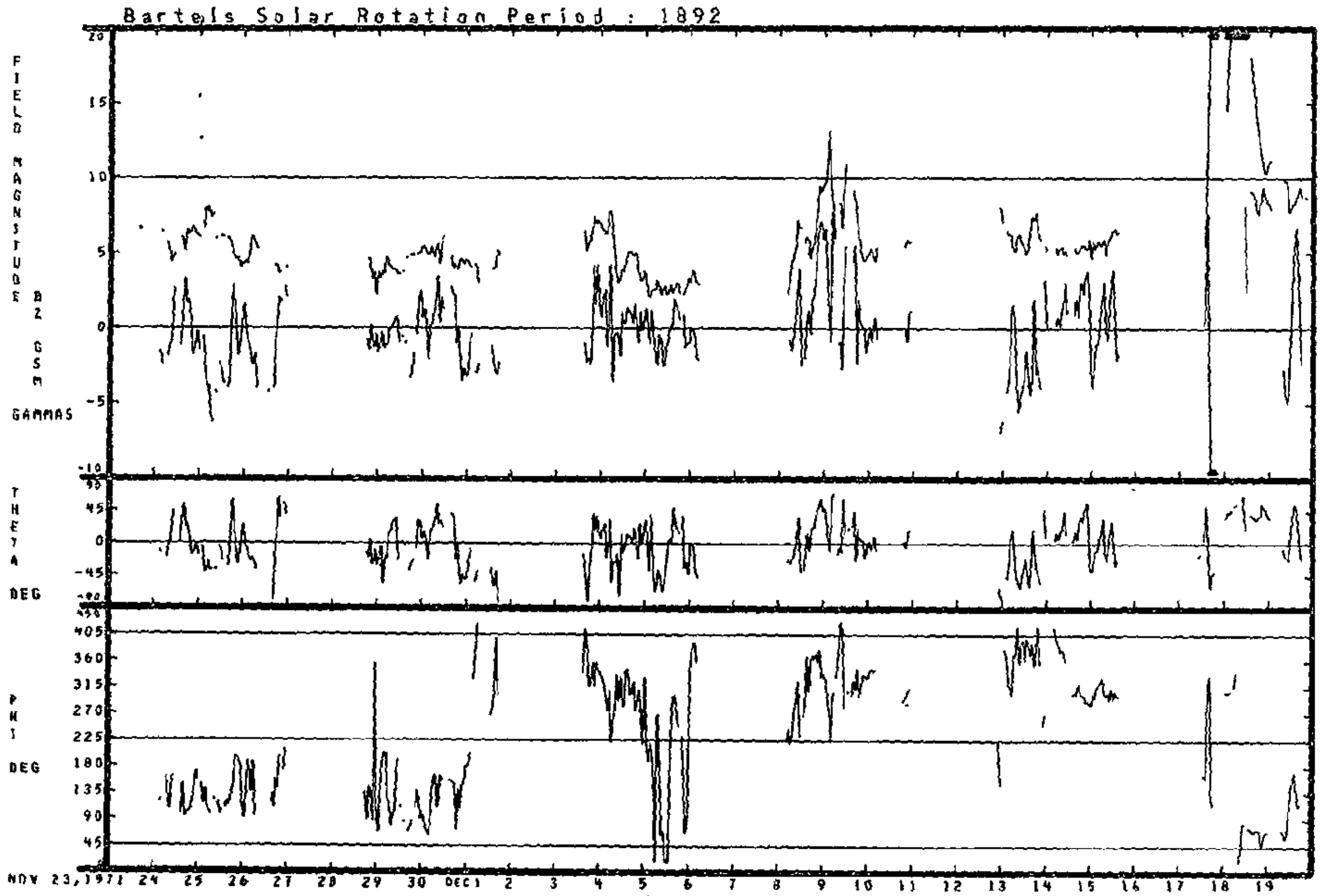




10/27/71 - 11/22/71



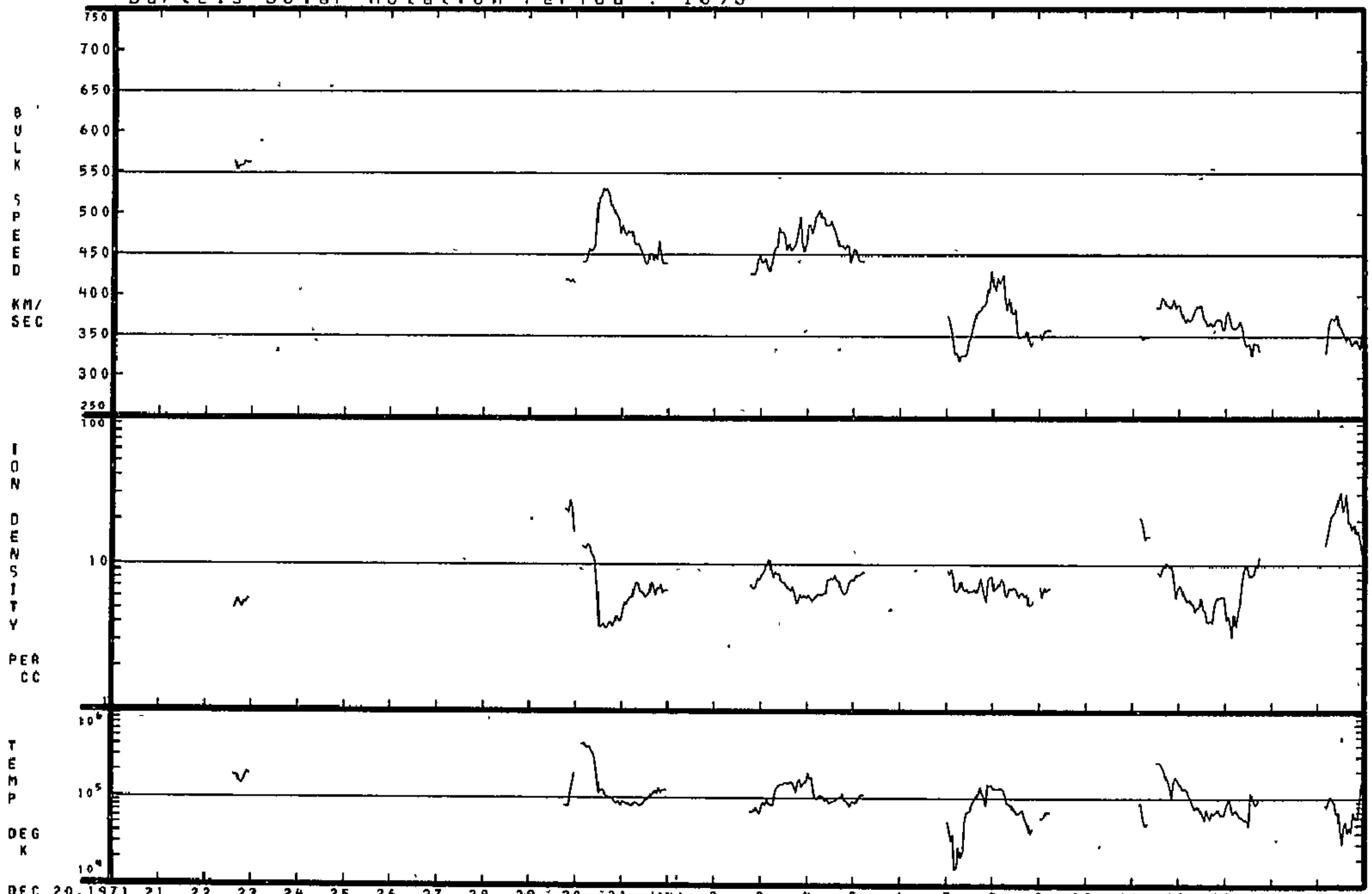
11/23/71 - 12/19/71



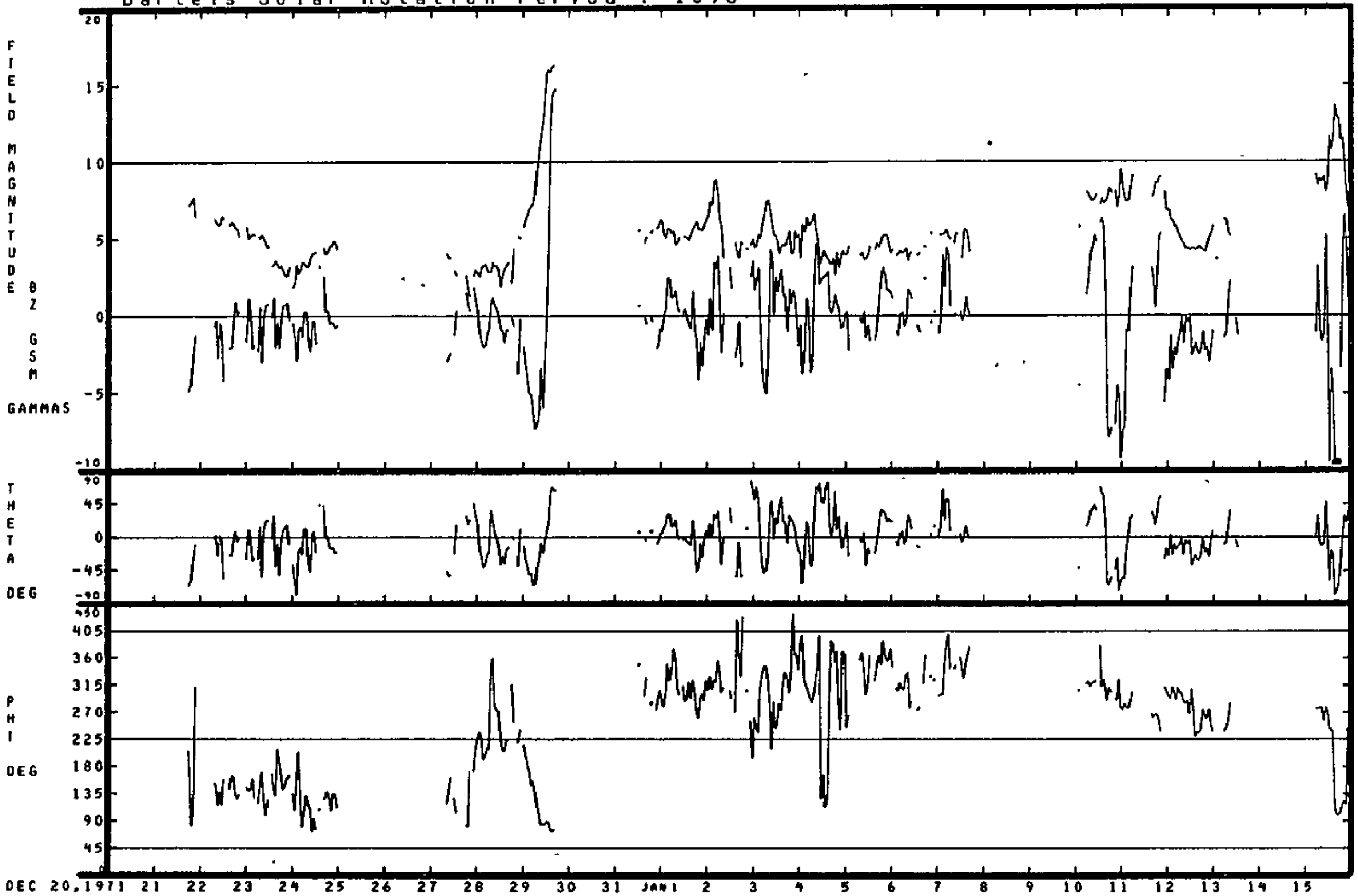
11/23/71 - 12/19/71

12/20/71 - 01/15/72

Bartels Solar Rotation Period : 1893



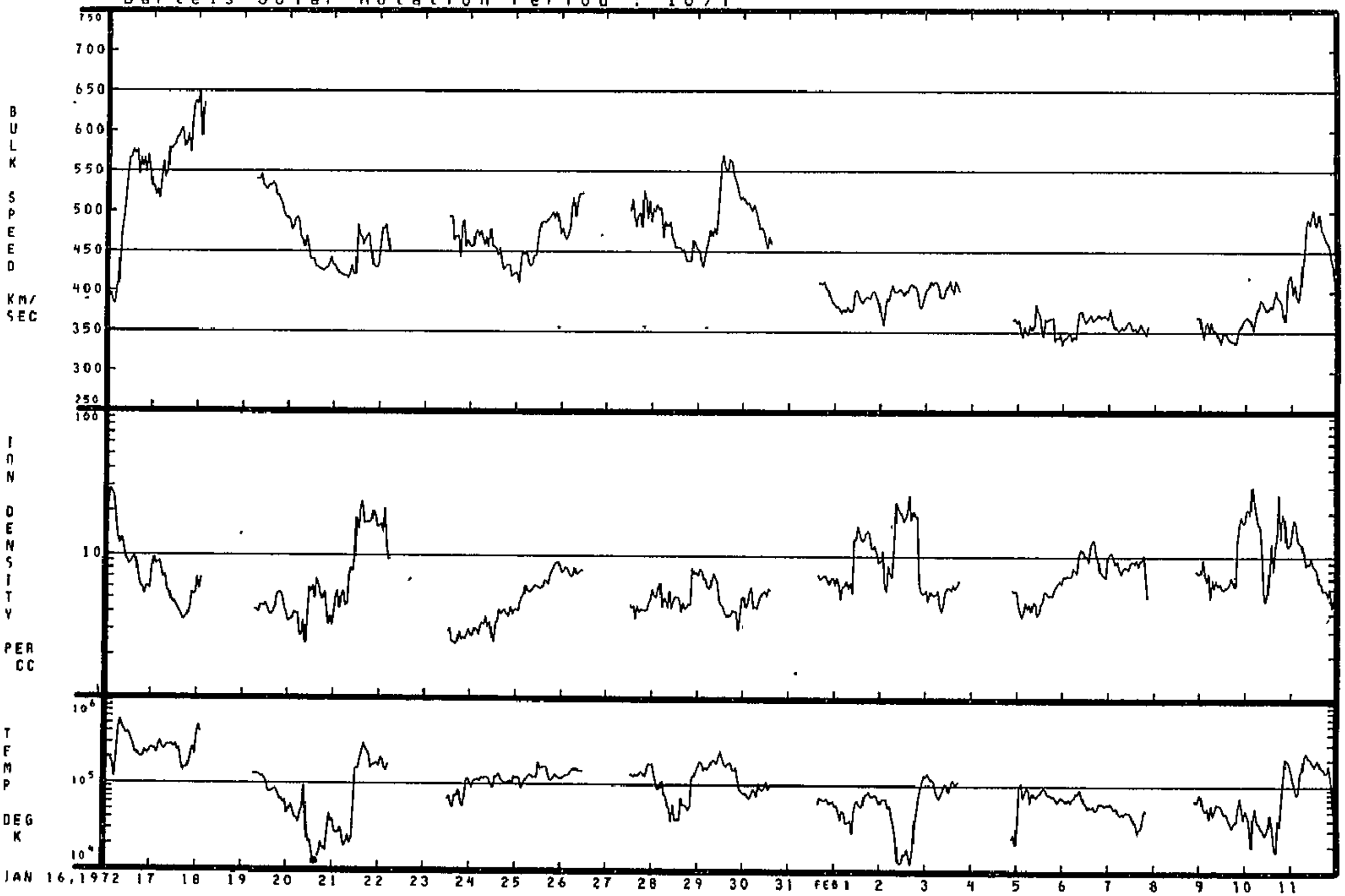
Bartels Solar Rotation Period : 1893



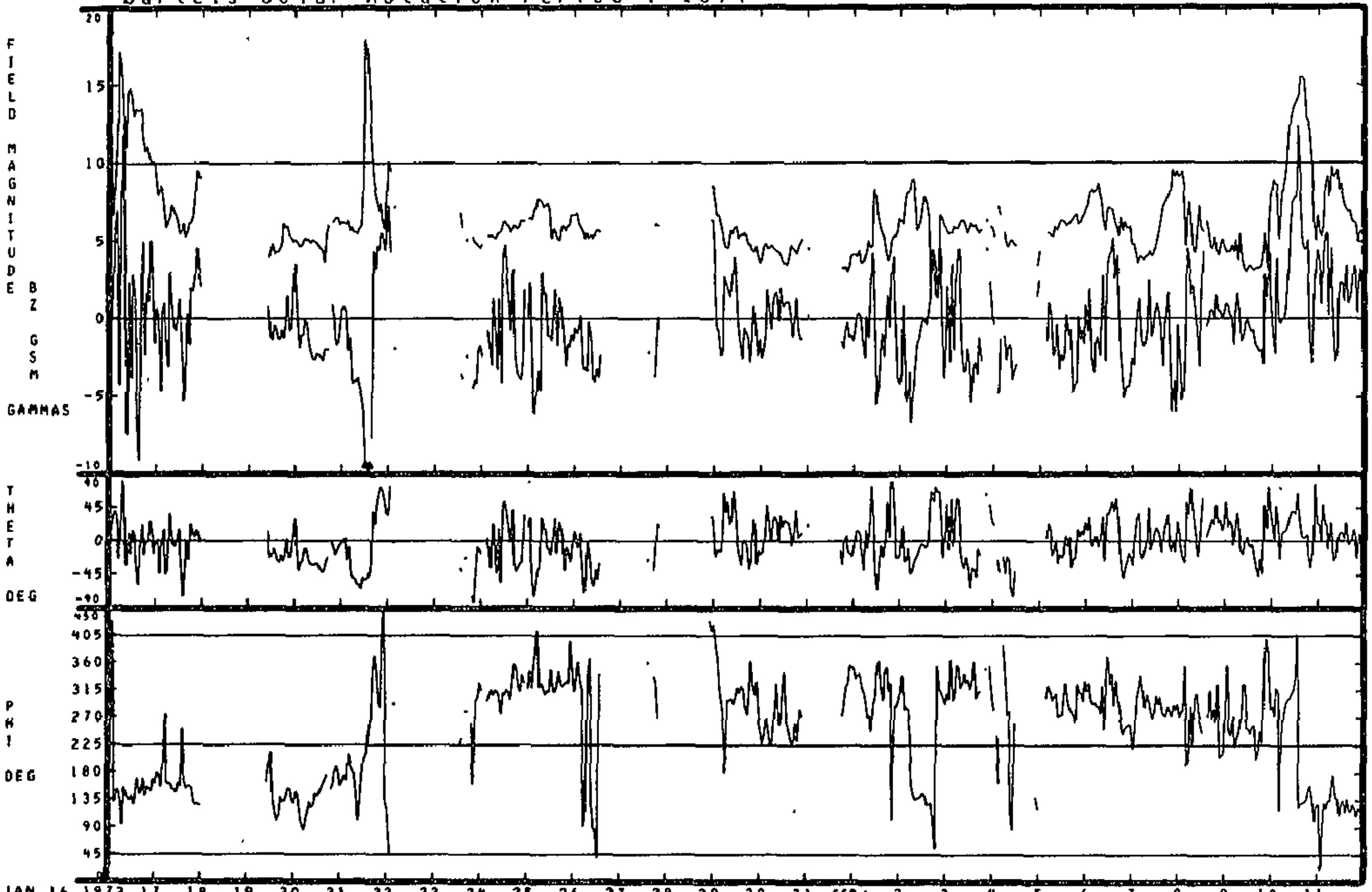
12/20/71 - 01/15/72

01/16/72 - 02/11/72

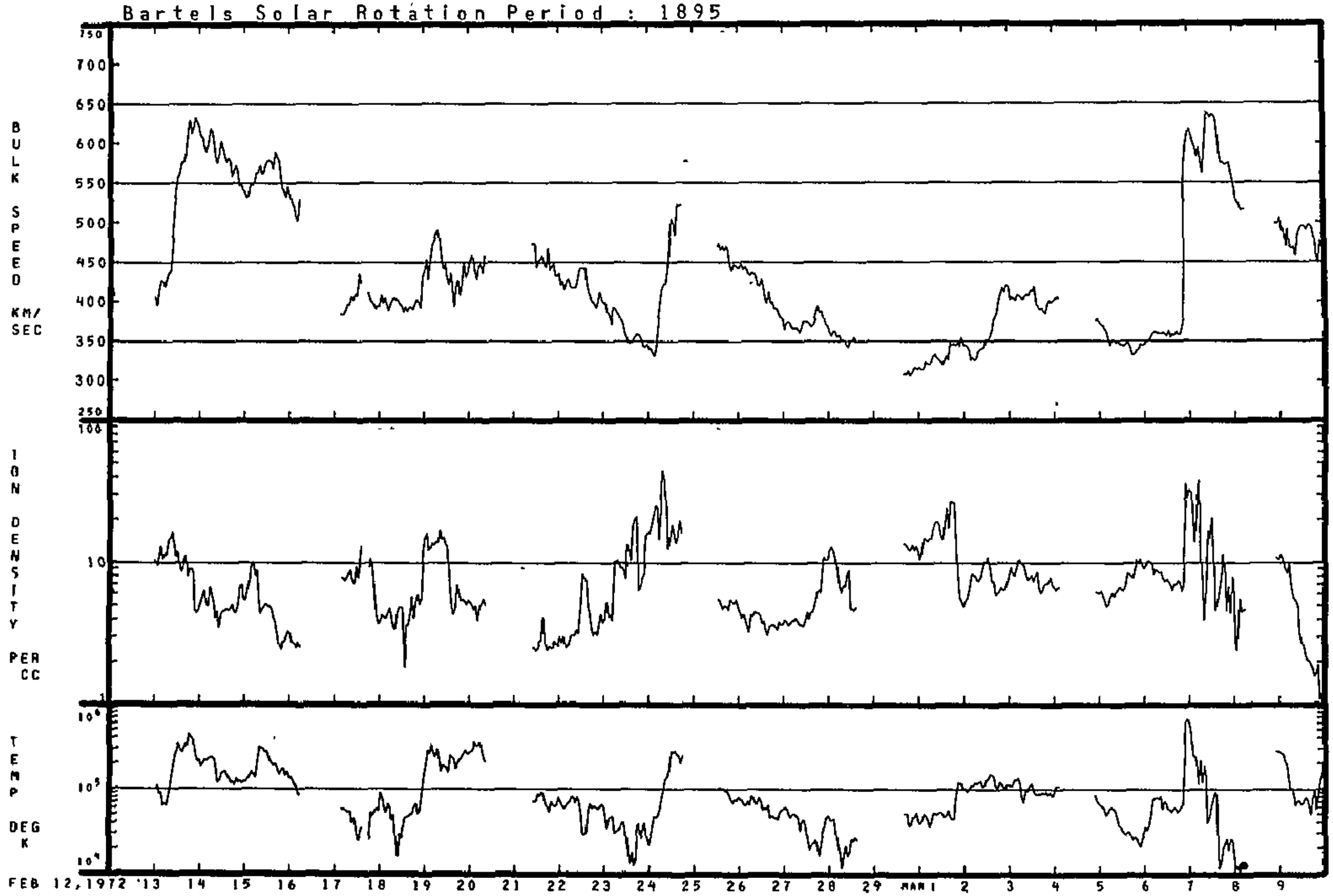
Bartels Solar Rotation Period : 1894



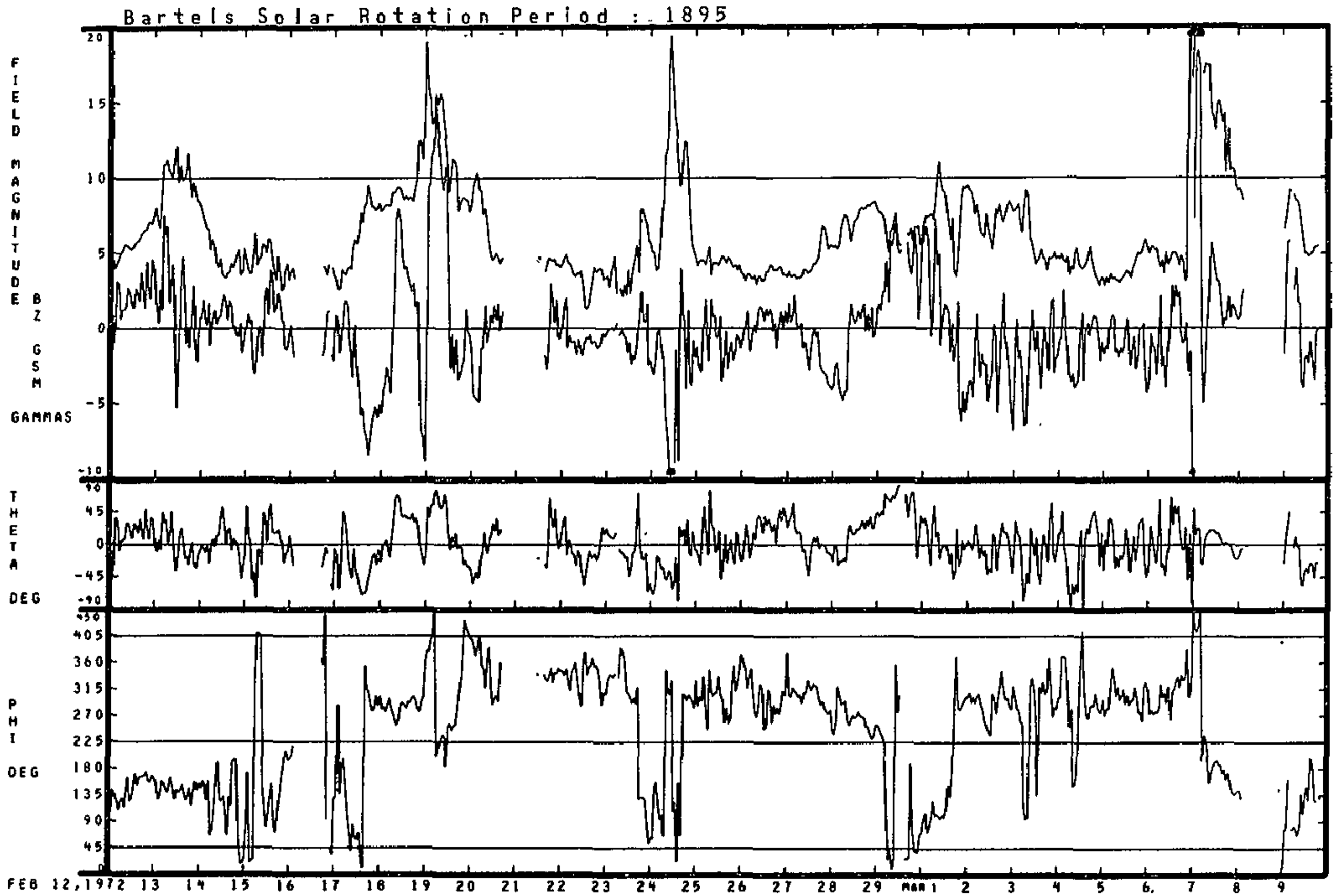
Bartels Solar Rotation Period : 1894



01/16/72 - 02/11/72



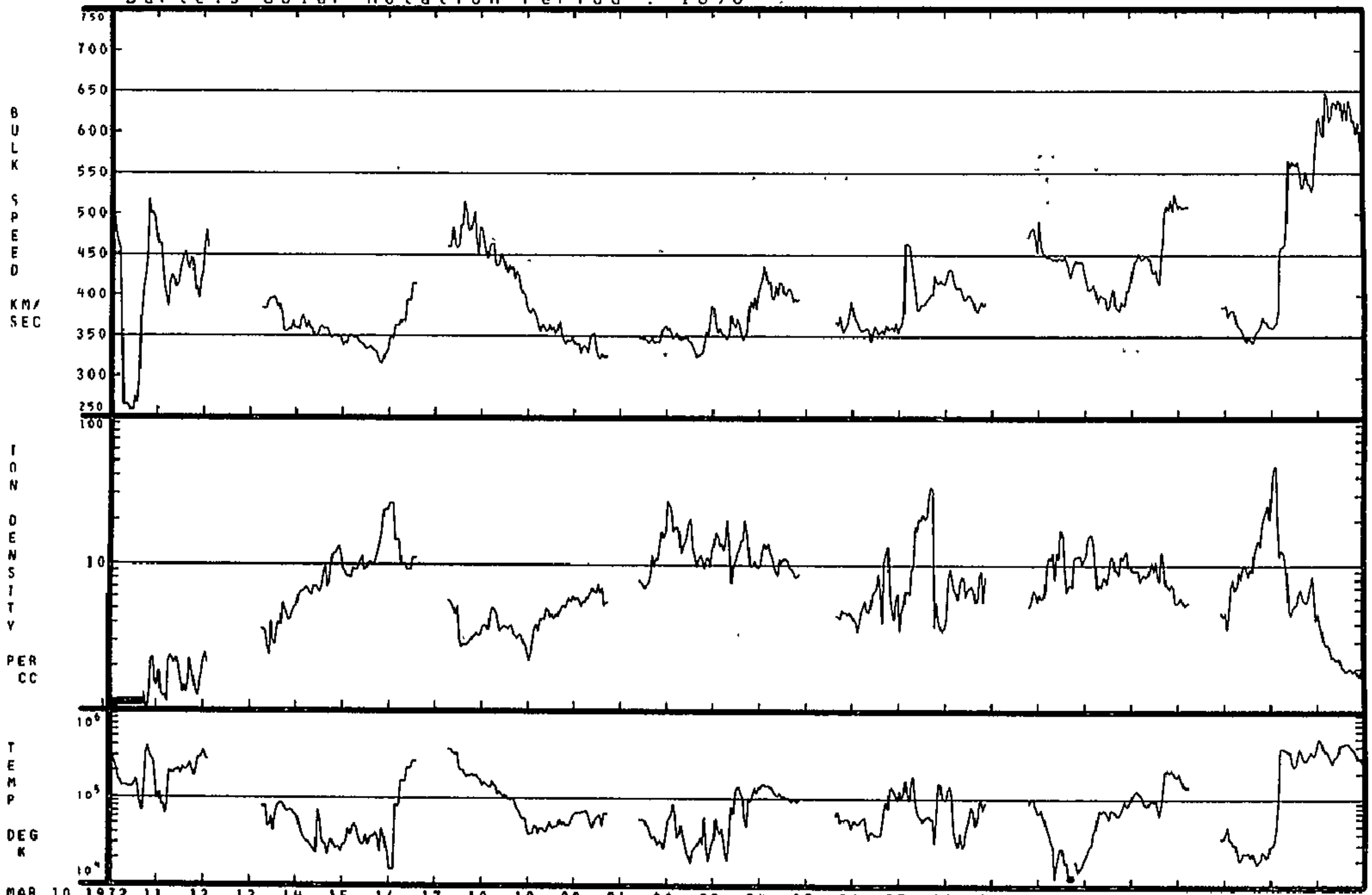
02/12/72 - 03/09/72



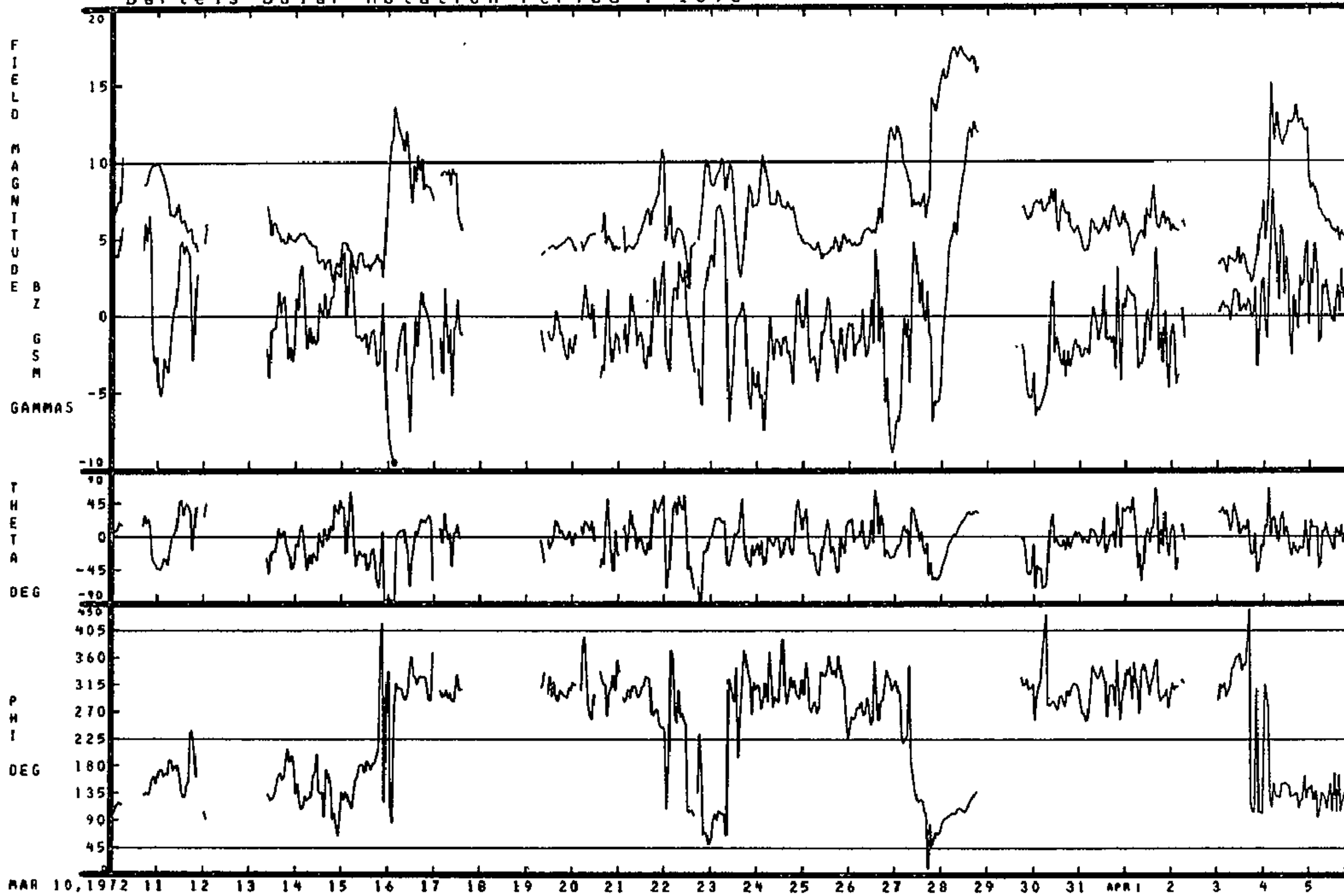
02/12/72 - 03/09/72

03/10/72 - 04/05/72

Bartels Solar Rotation Period : 1896

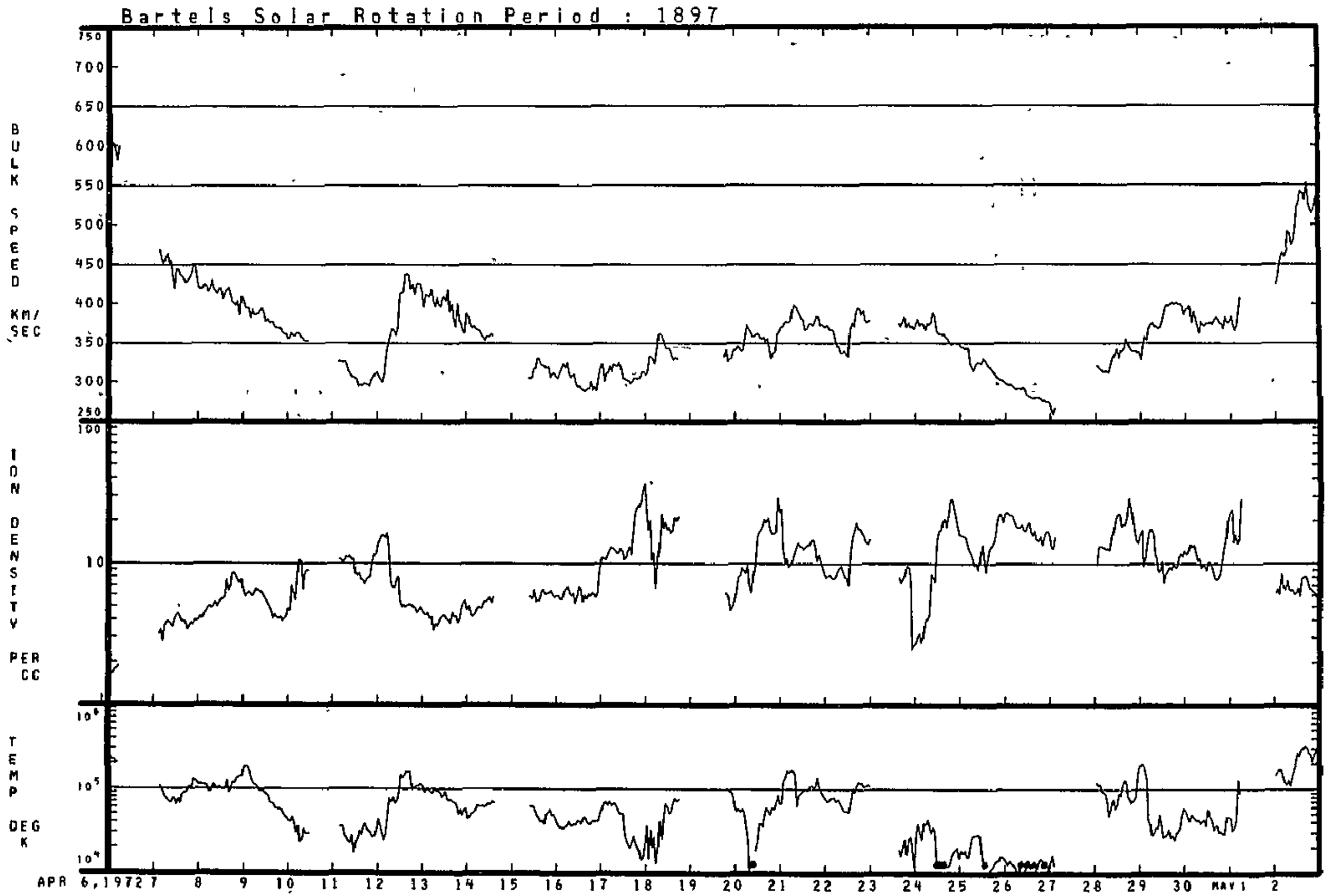


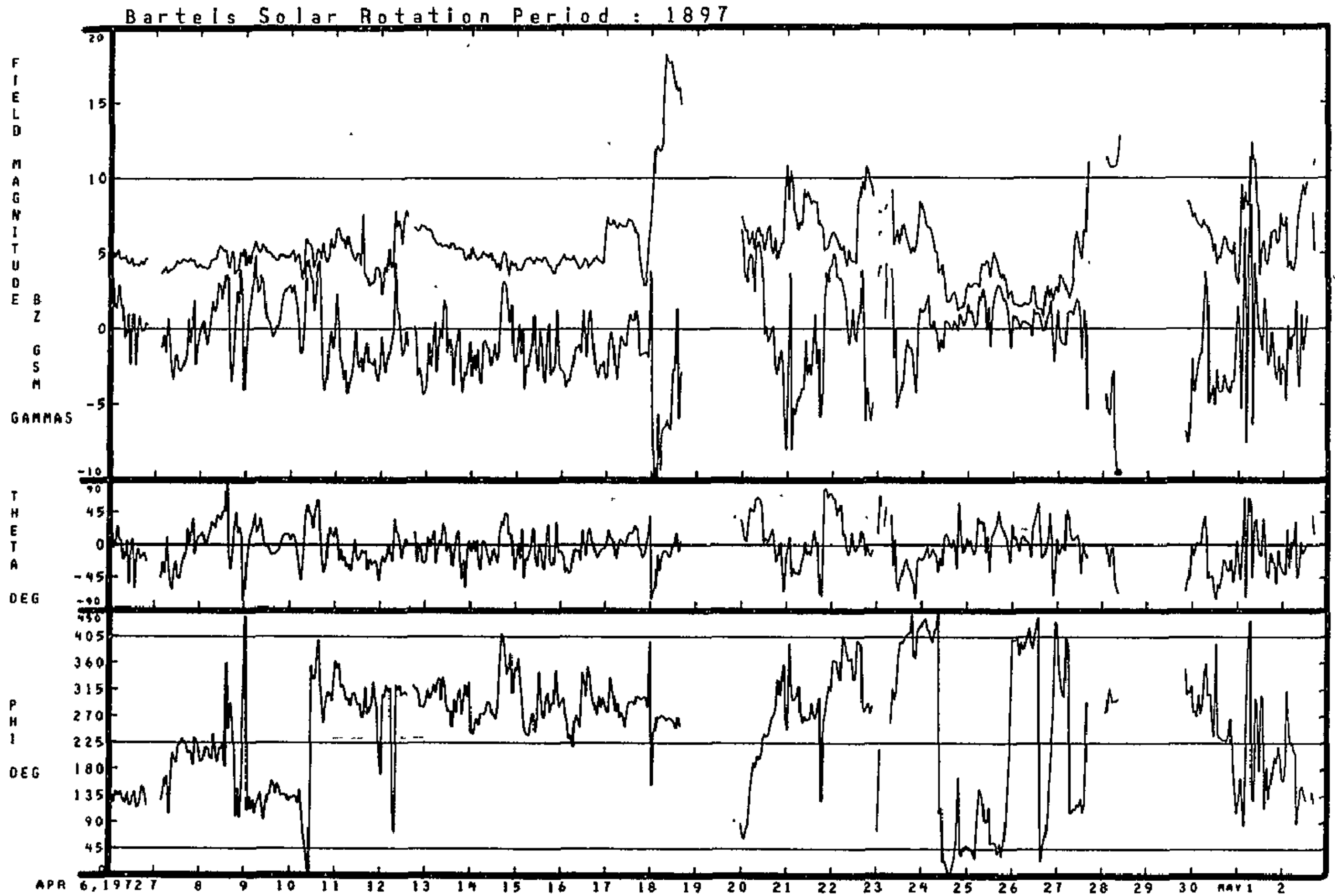
Bartels Solar Rotation Period : 1896



03/10/72 - 04/05/72

04/06/72 - 05/02/72

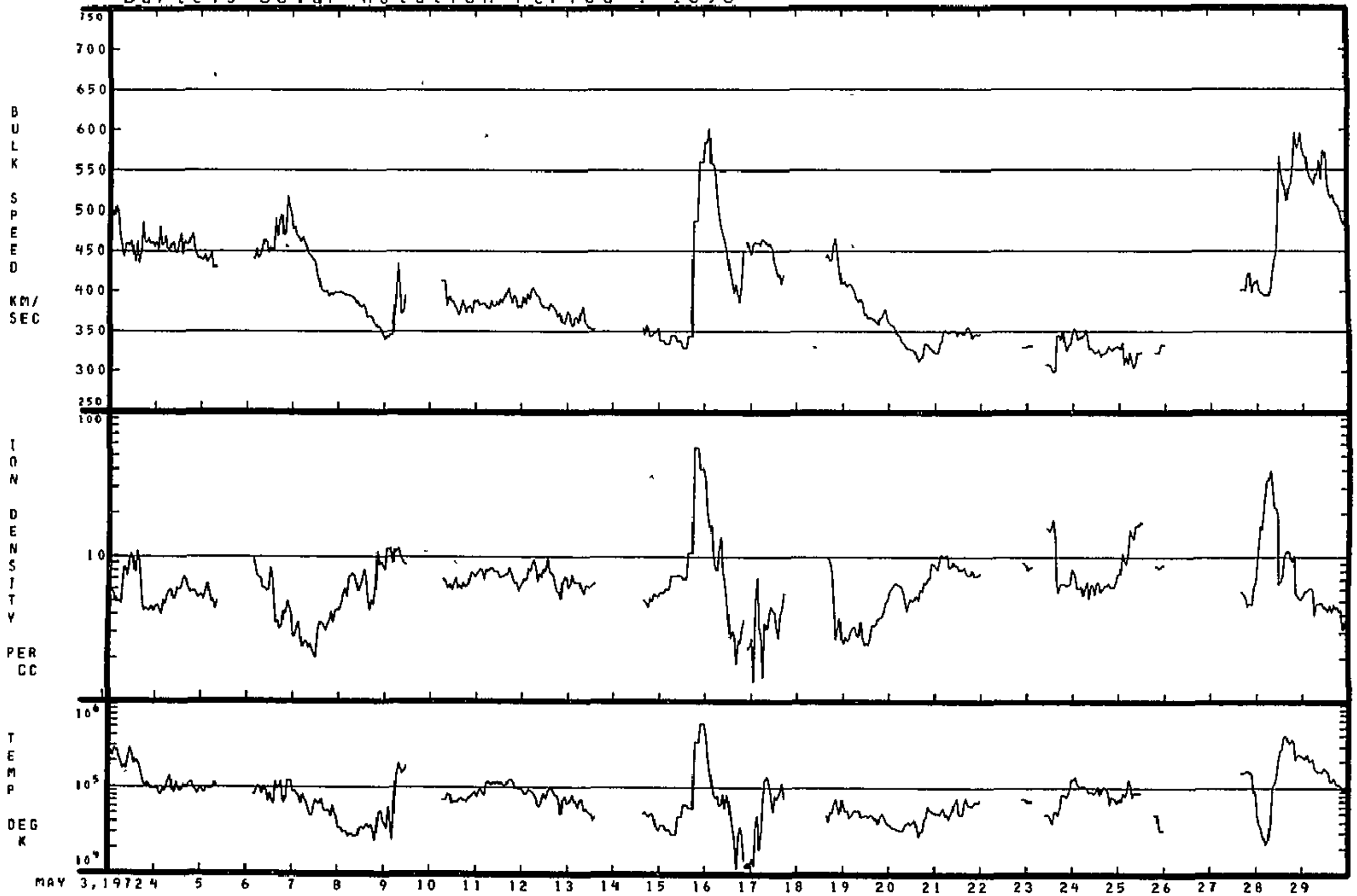




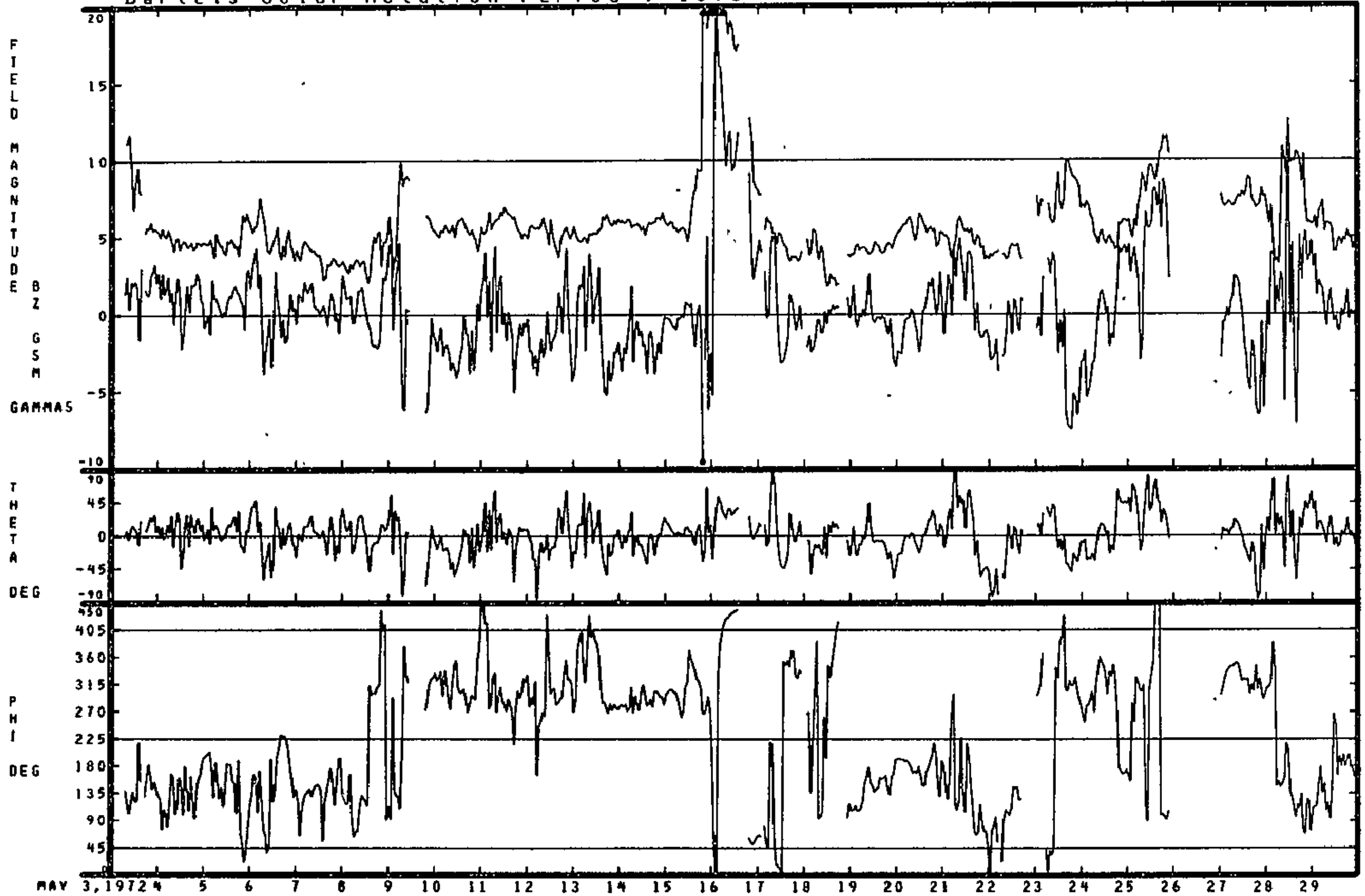
04/06/72 - 05/02/72

05/03/72 - 05/29/72

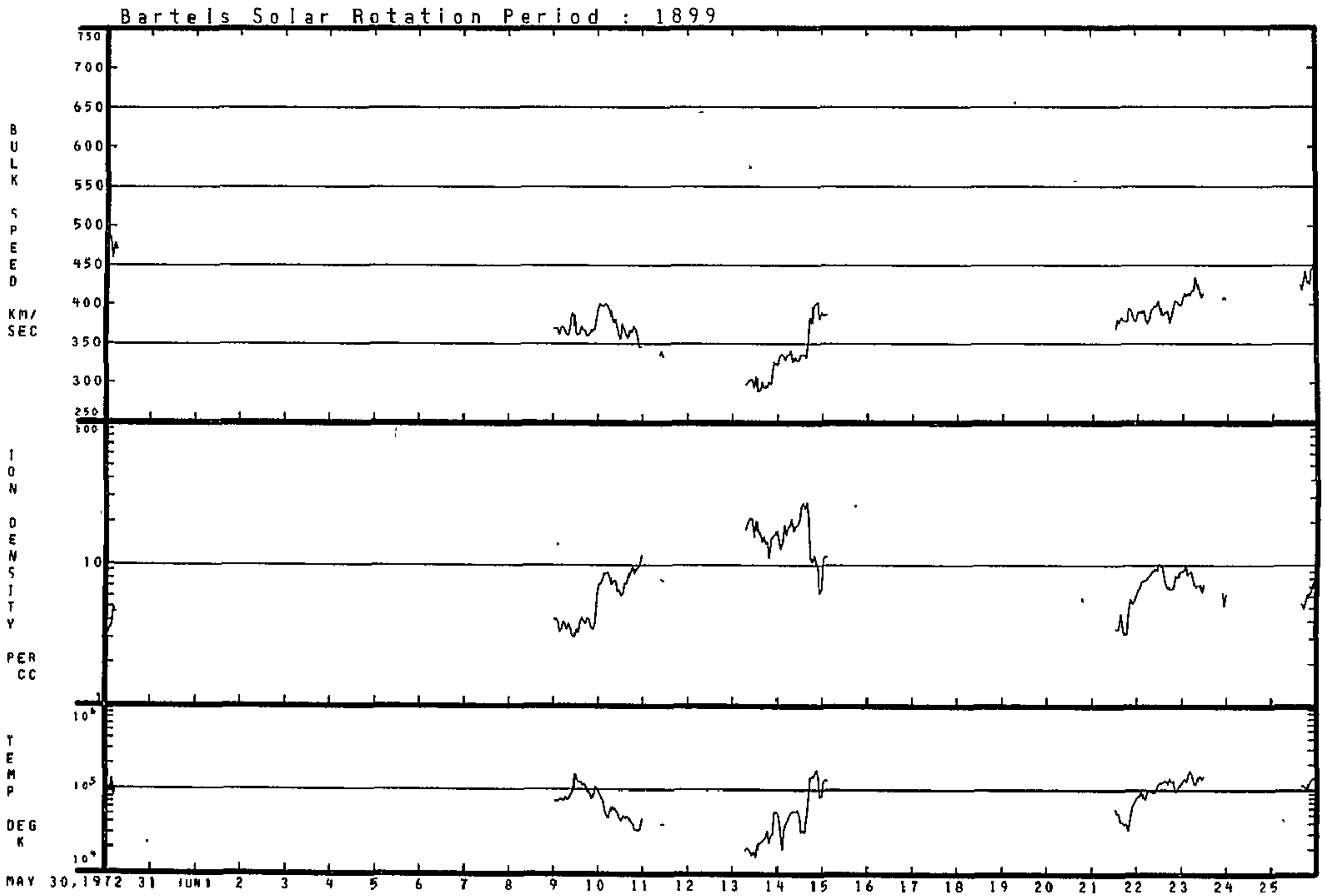
Bartels Solar Rotation Period : 1898



Bartels Solar Rotation Period : 1898



05/03/72 - 05/29/72



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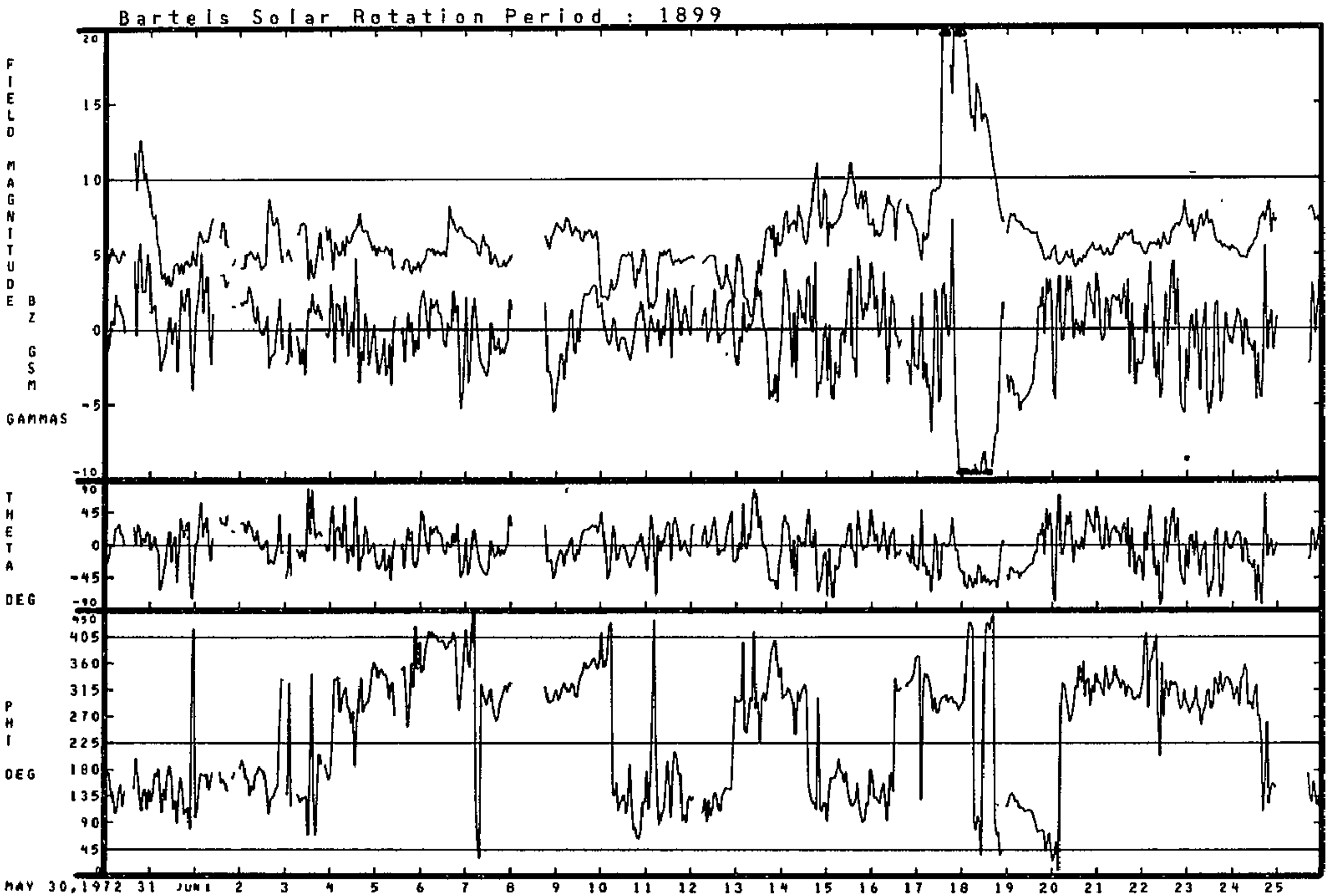
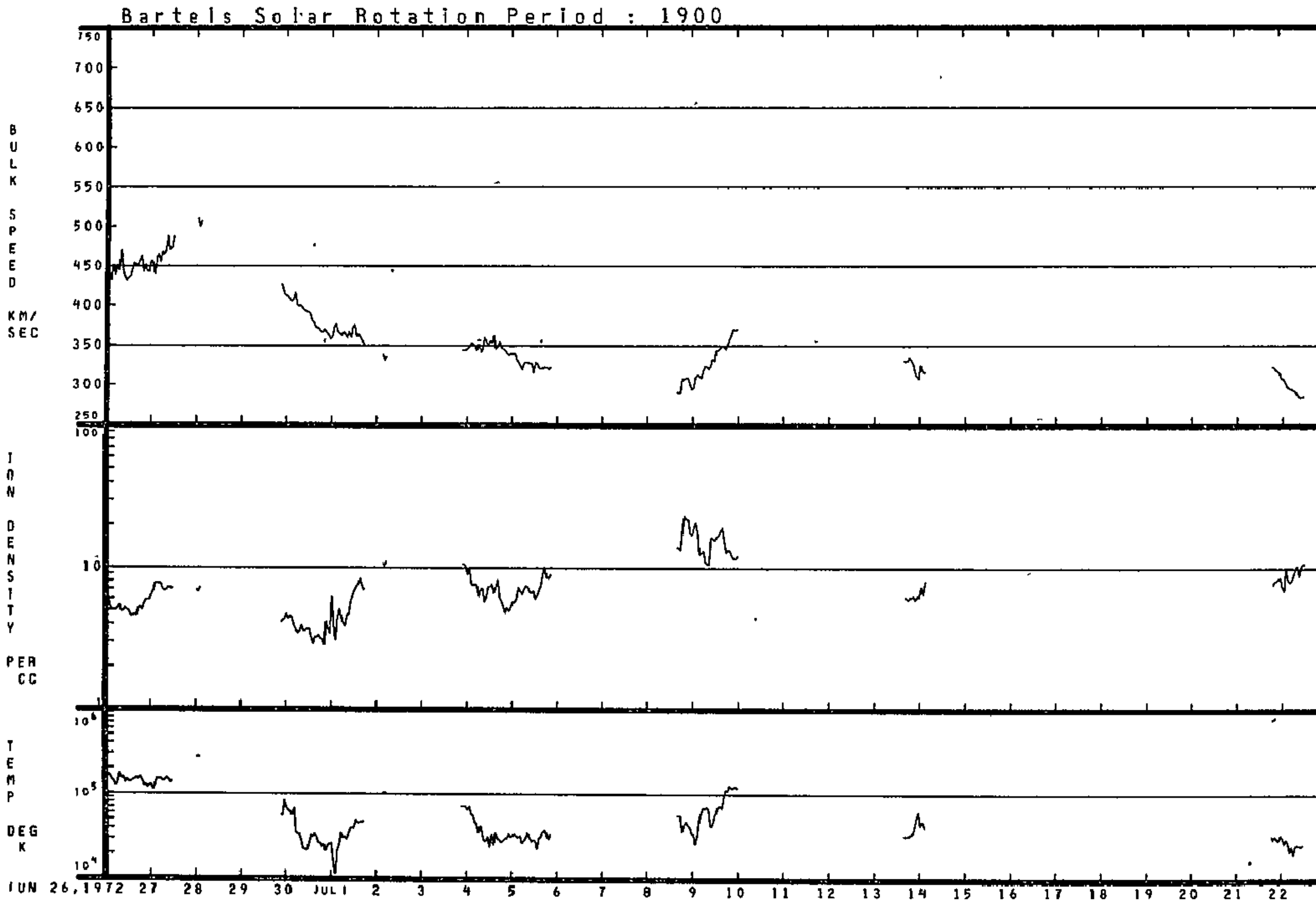
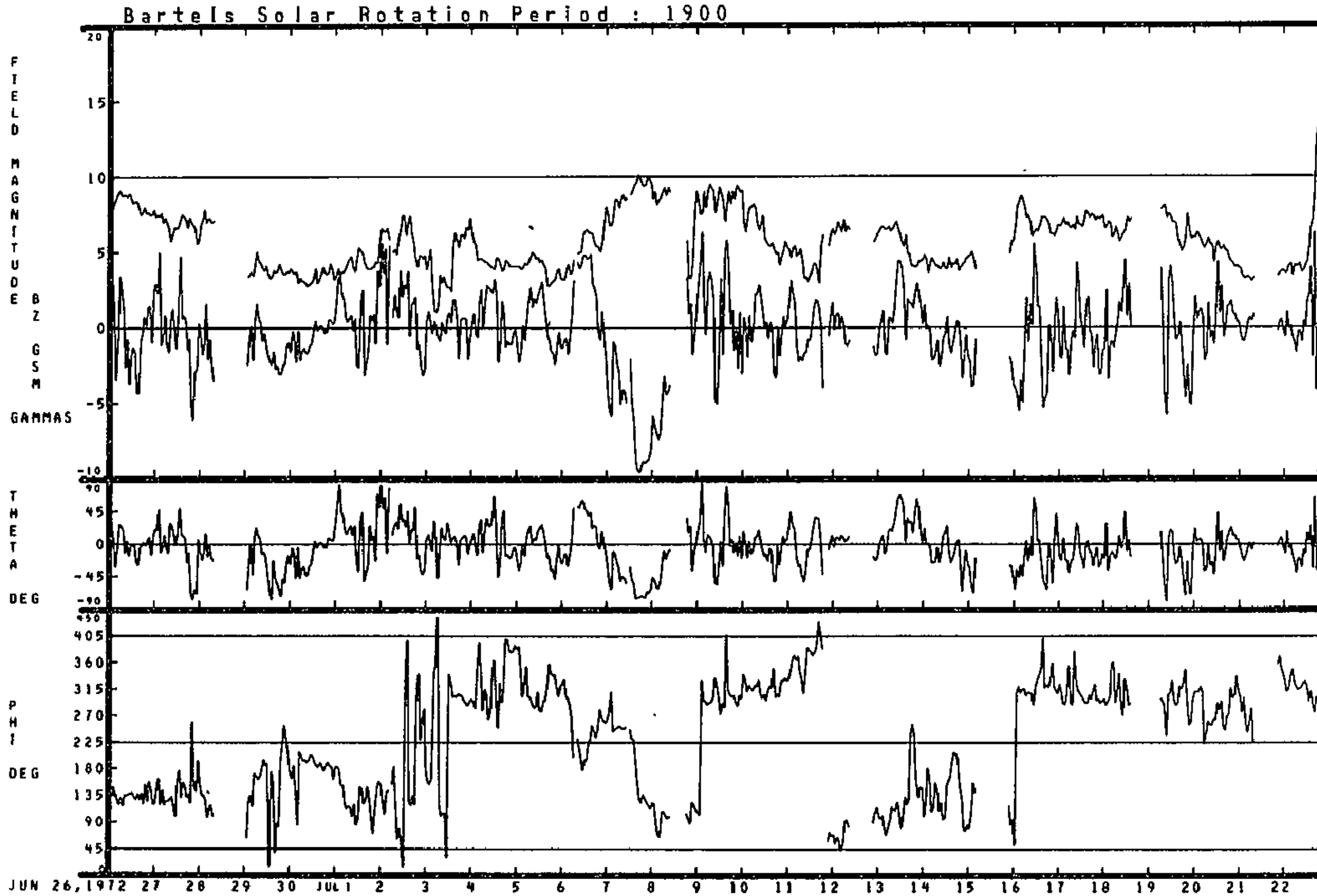


05/30/72 - 06/25/72

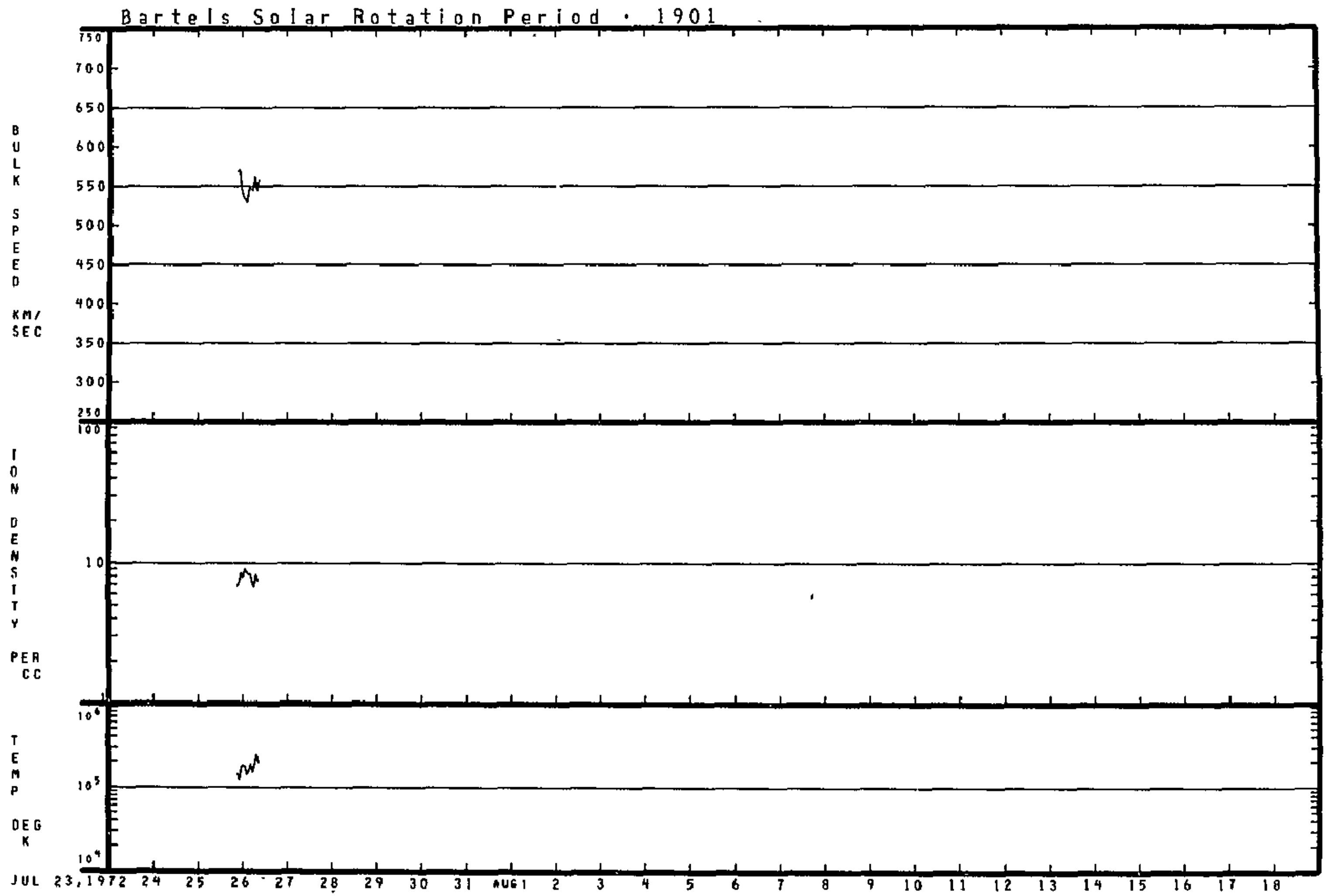
c-4

06/26/72 - 07/22/72



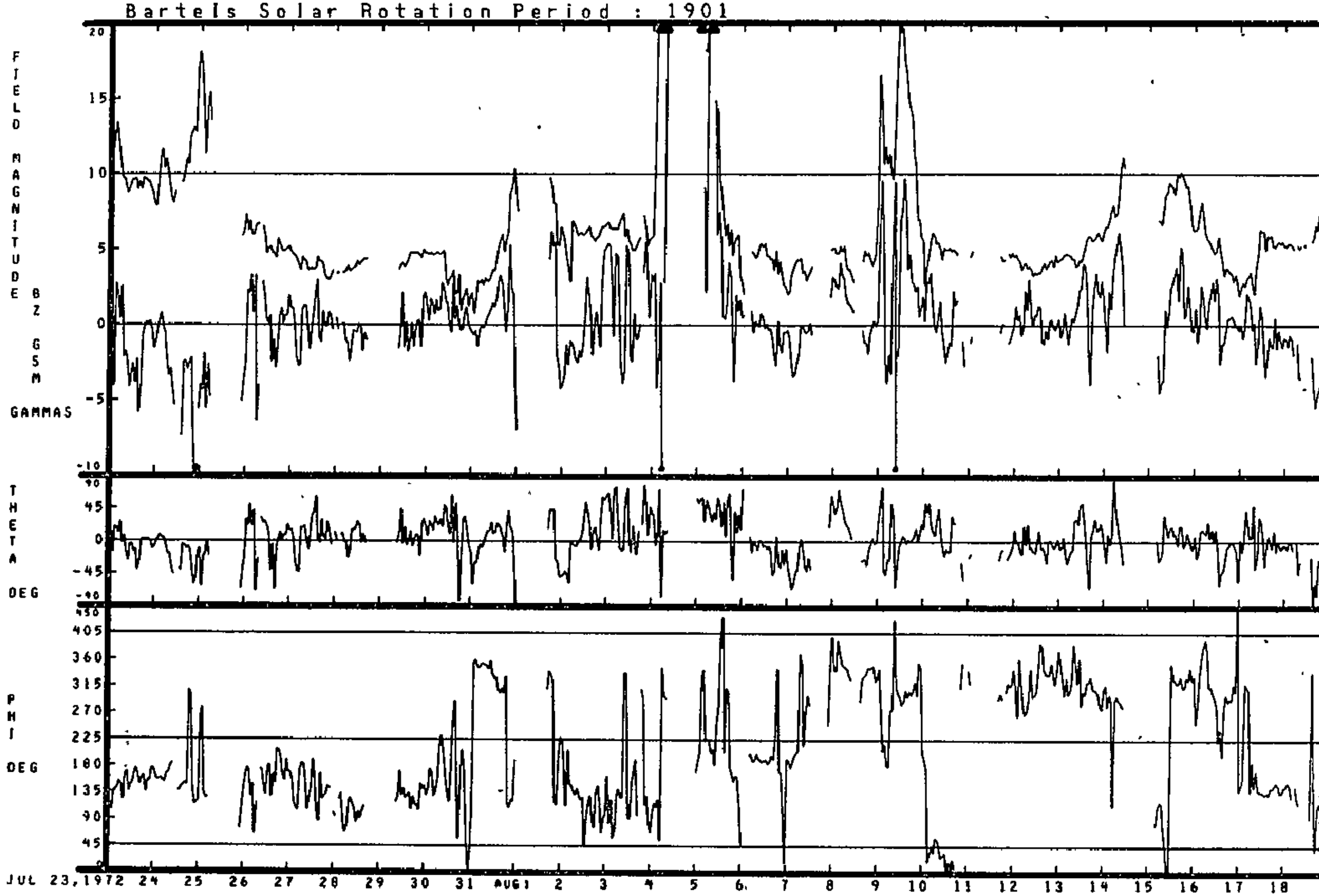


06/26/72 - 07/22/72



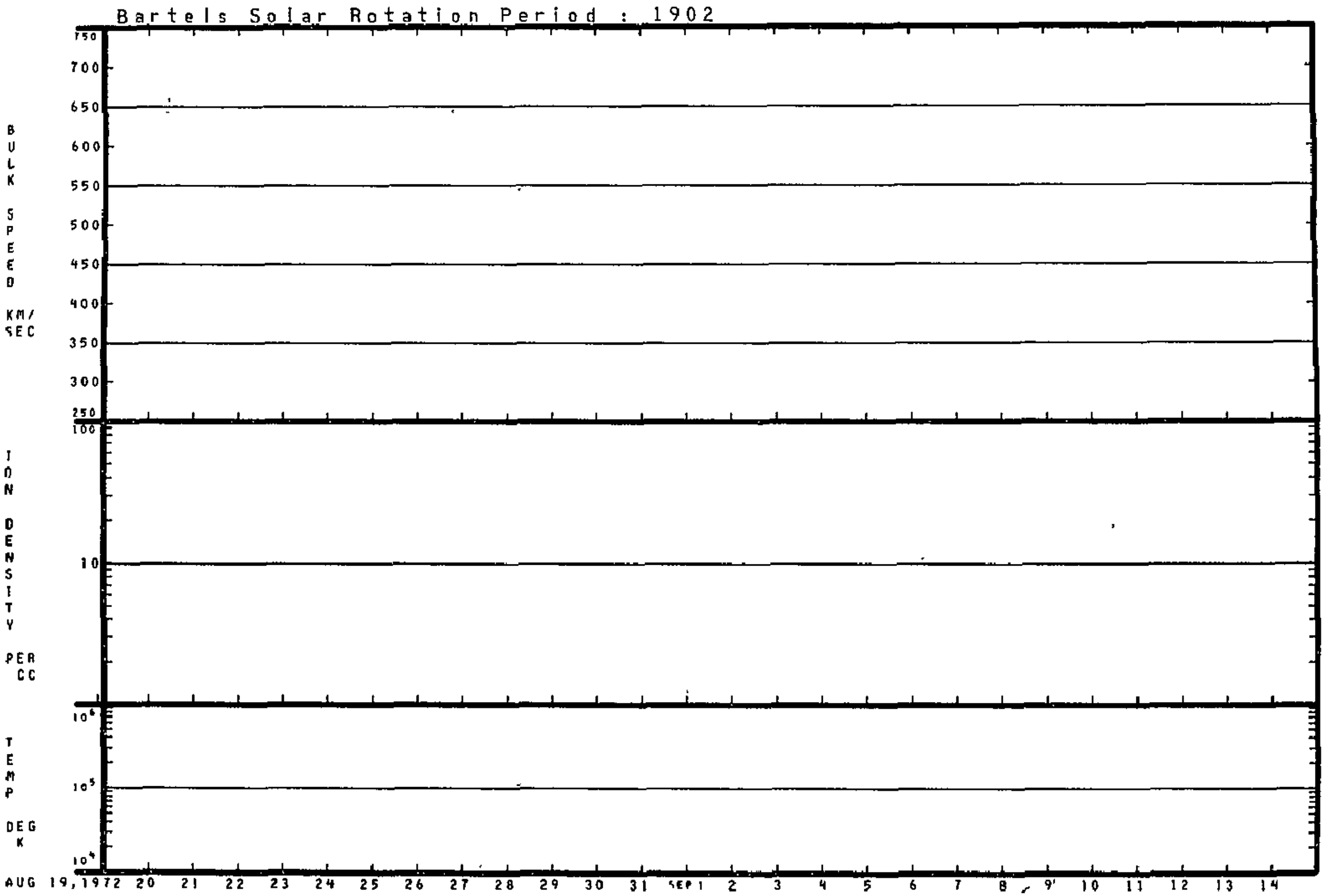
07/23/72 - 08/18/72

Bartels Solar Rotation Period : 1901

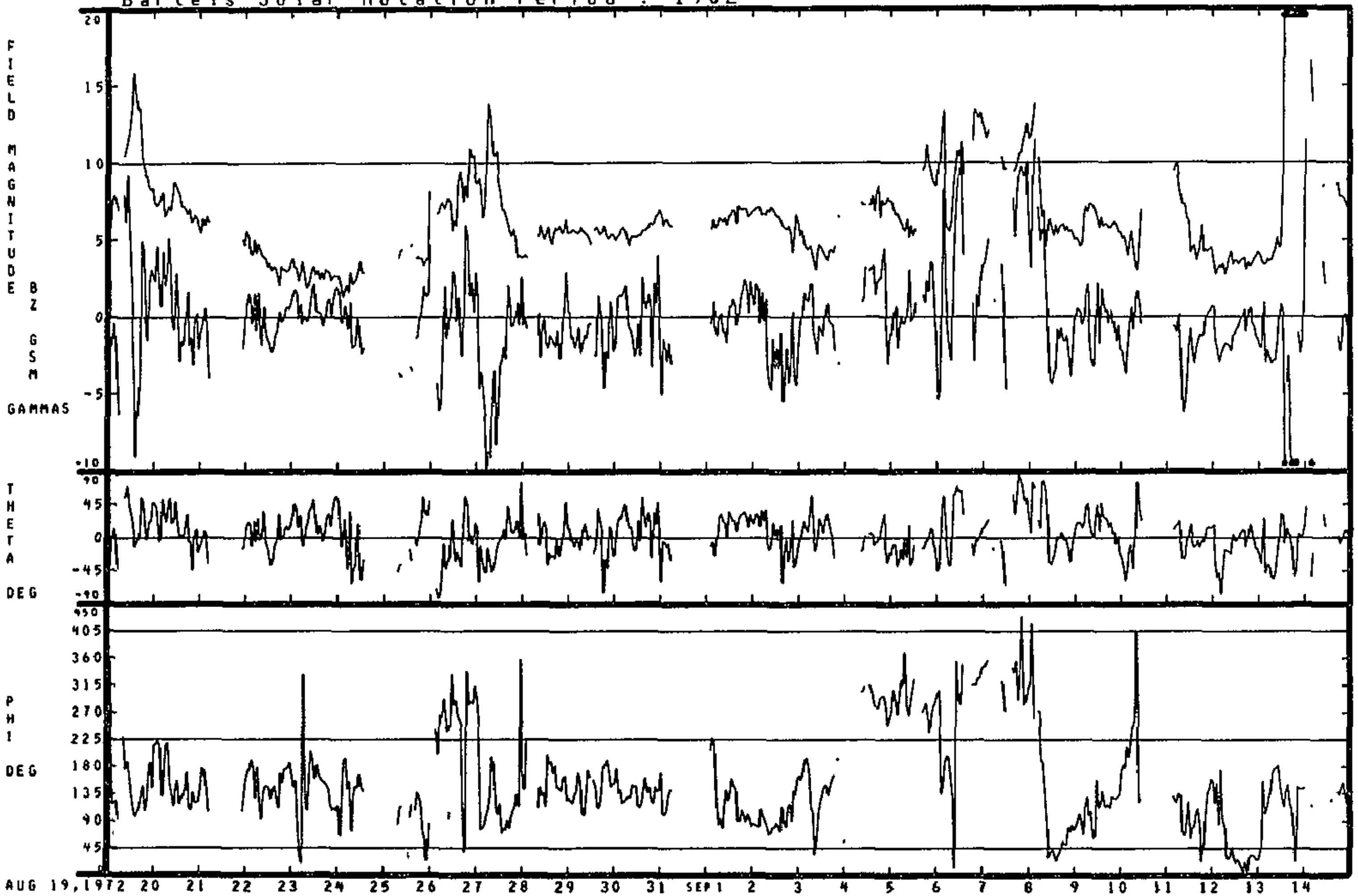


07/23/72 - 08/18/72

08/19/72 - 09/14/72

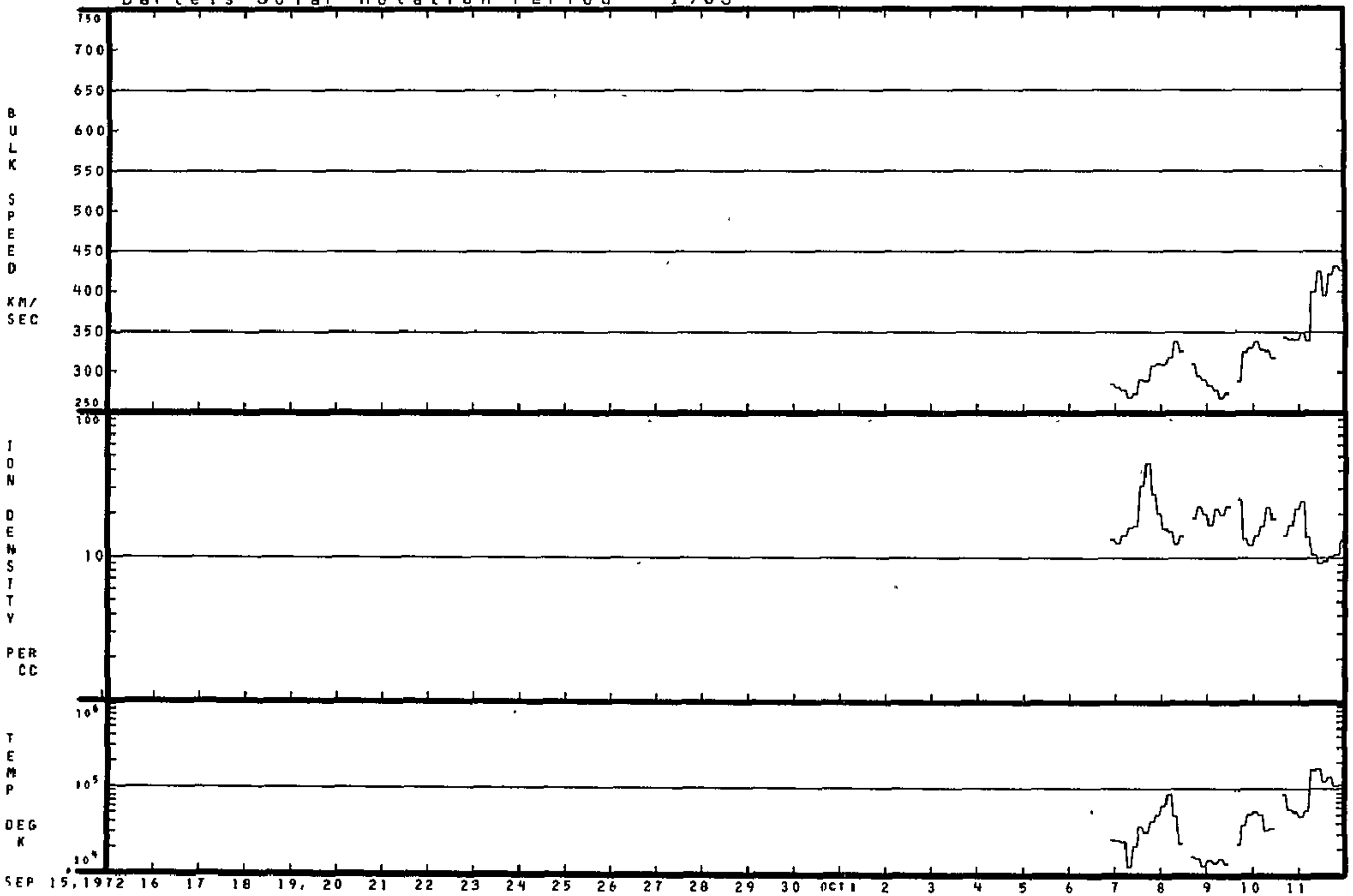


Bartels Solar Rotation Period : 1902



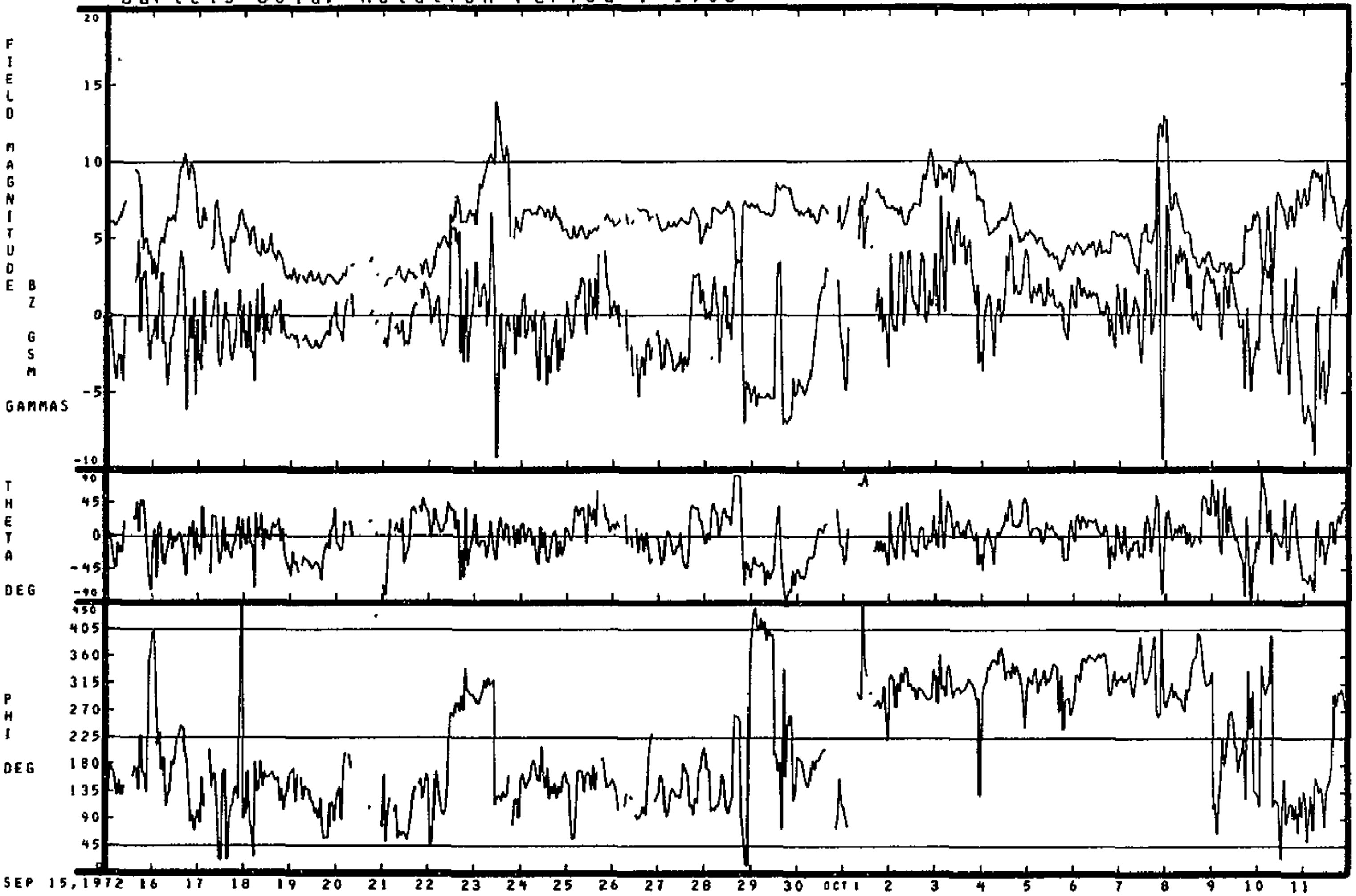
08/19/72 - 09/14/72

Bartels Solar Rotation Period · 1903



09/15/72 - 10/11/72

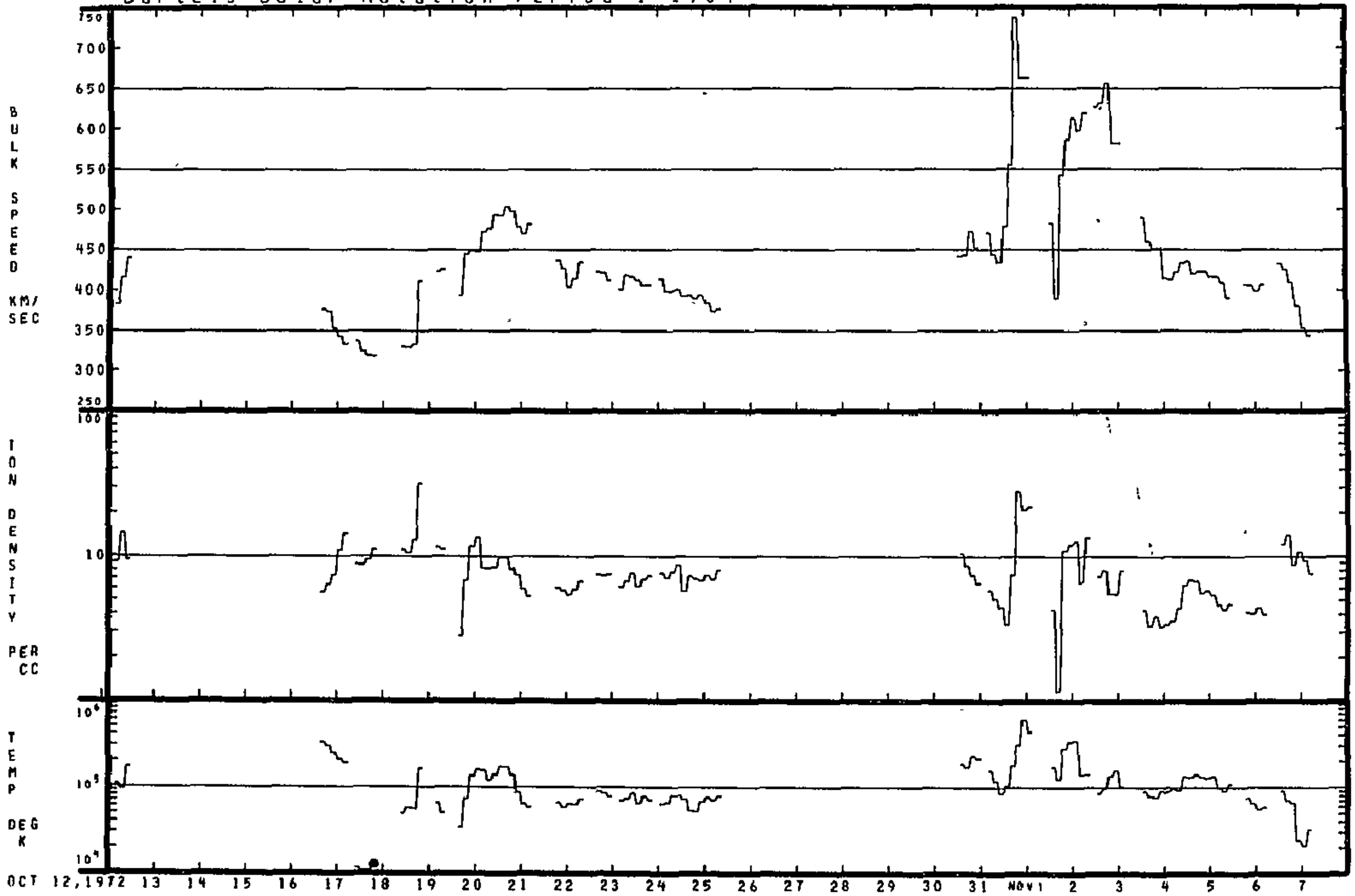
Bartels Solar Rotation Period : 1903



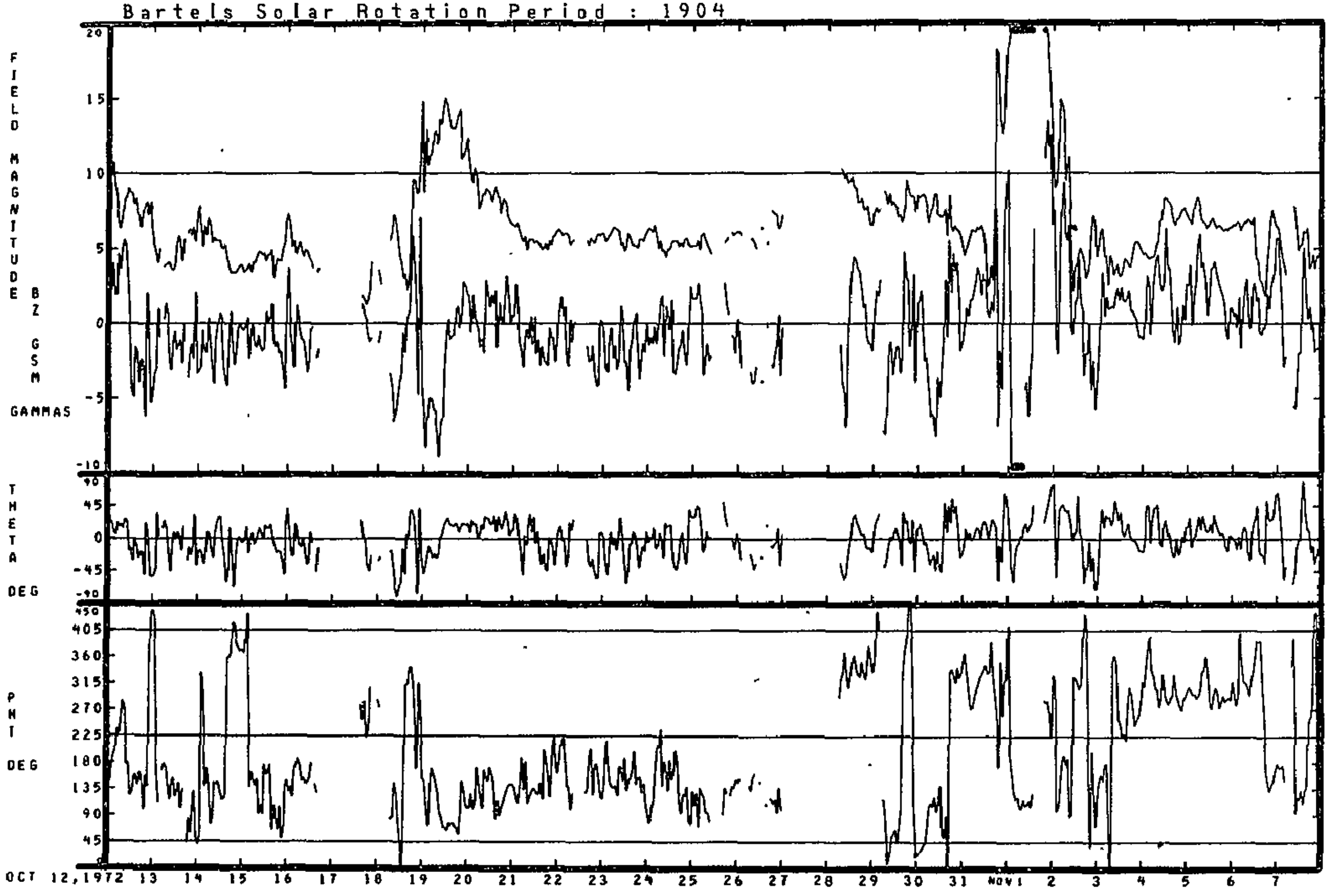
09/15/72 - 10/11/72

10/12/72 - 11/07/72

Bartels Solar Rotation Period : 1904



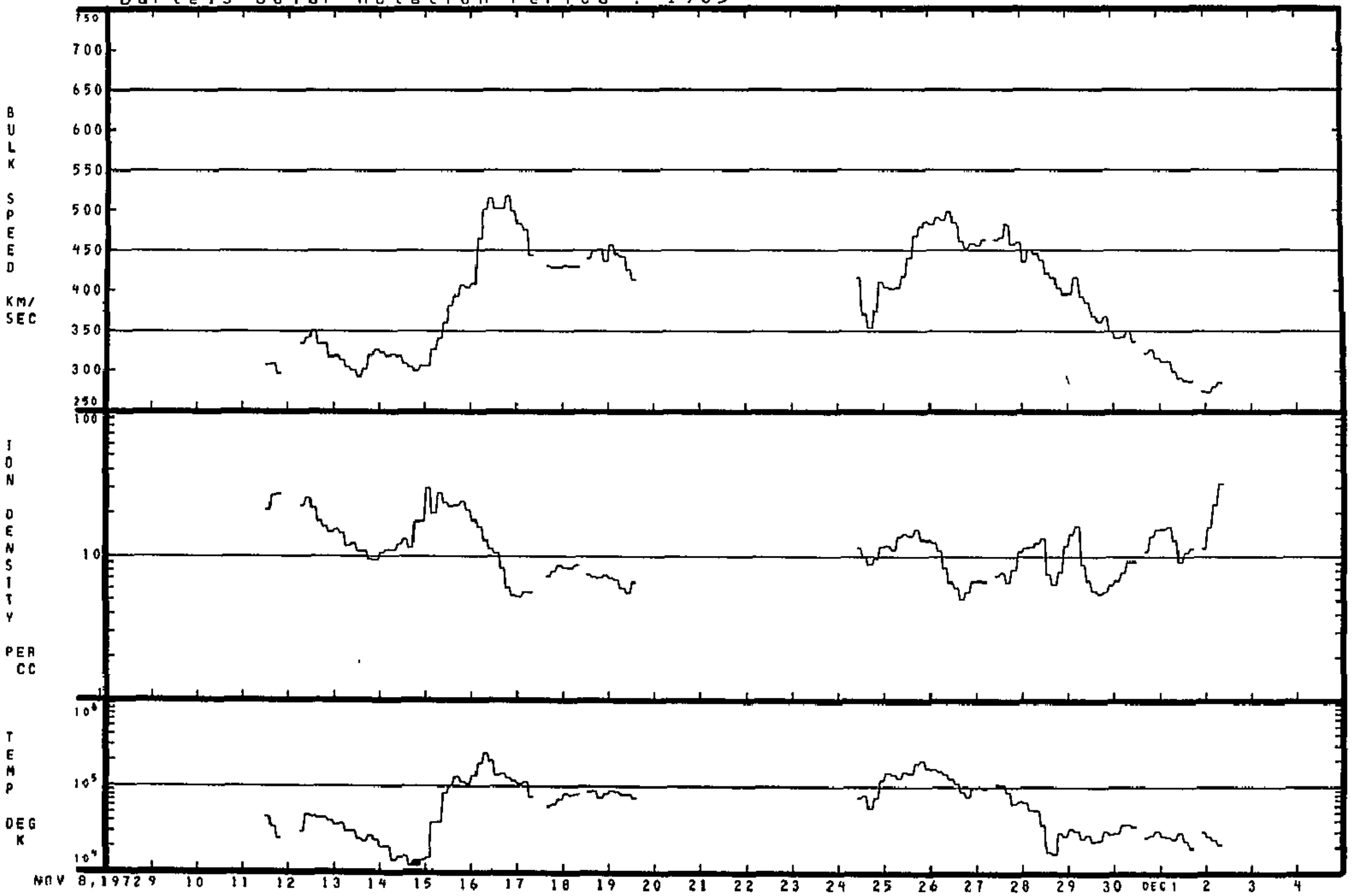
Bartels Solar Rotation Period : 1904



10/12/72 - 11/07/72

11/08/72 - 12/04/72

Bartels Solar Rotation Period : 1905



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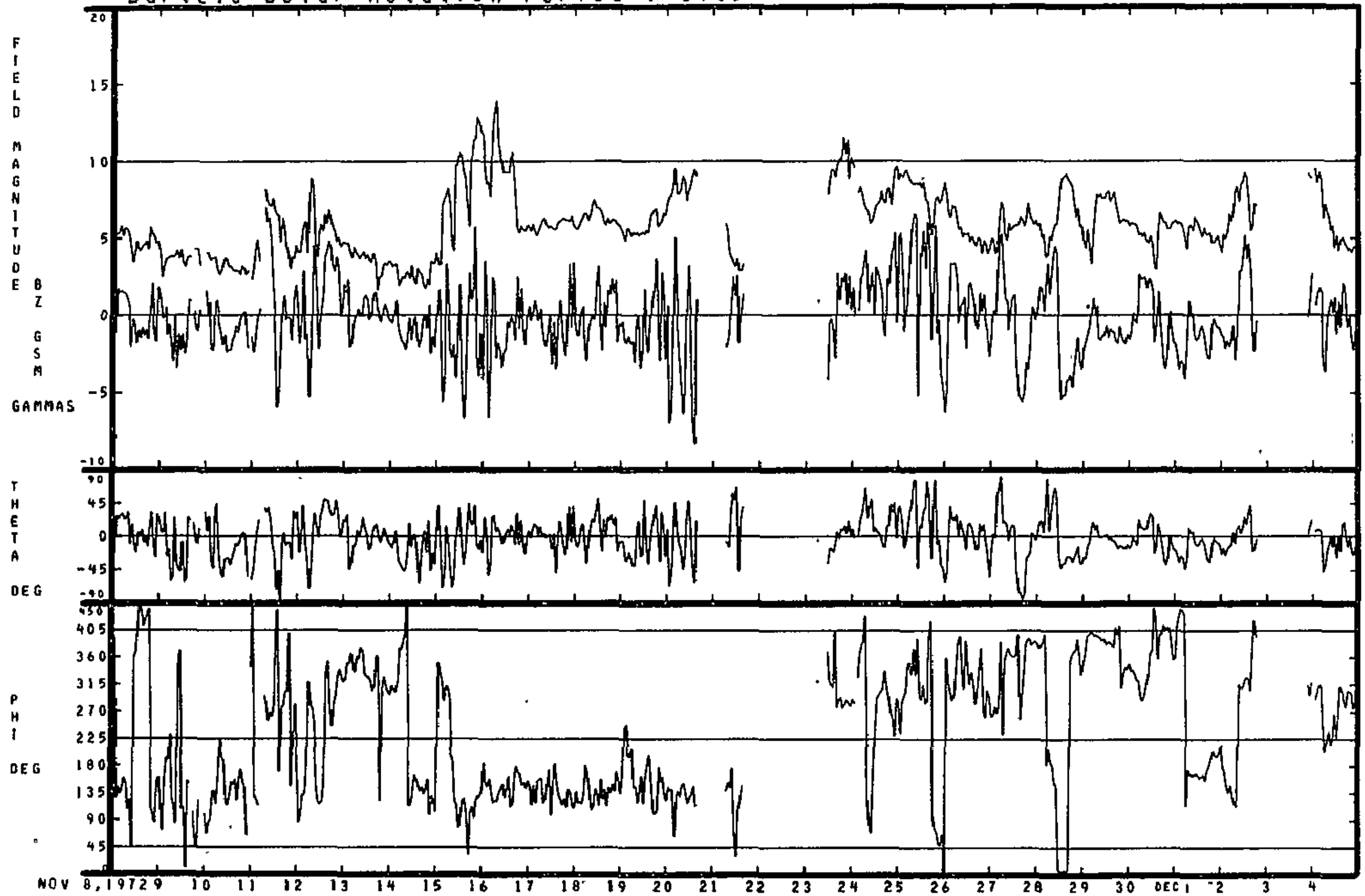
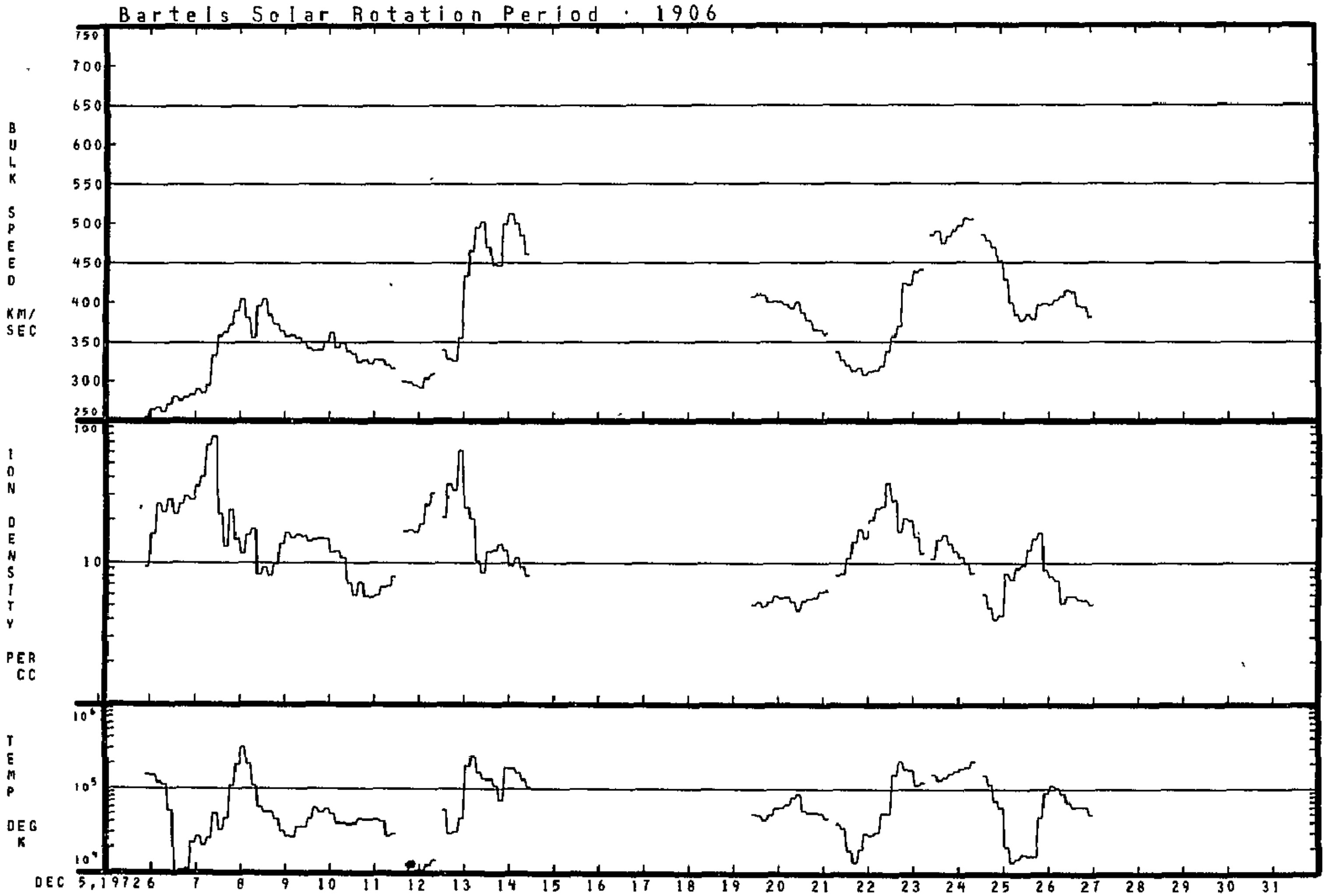
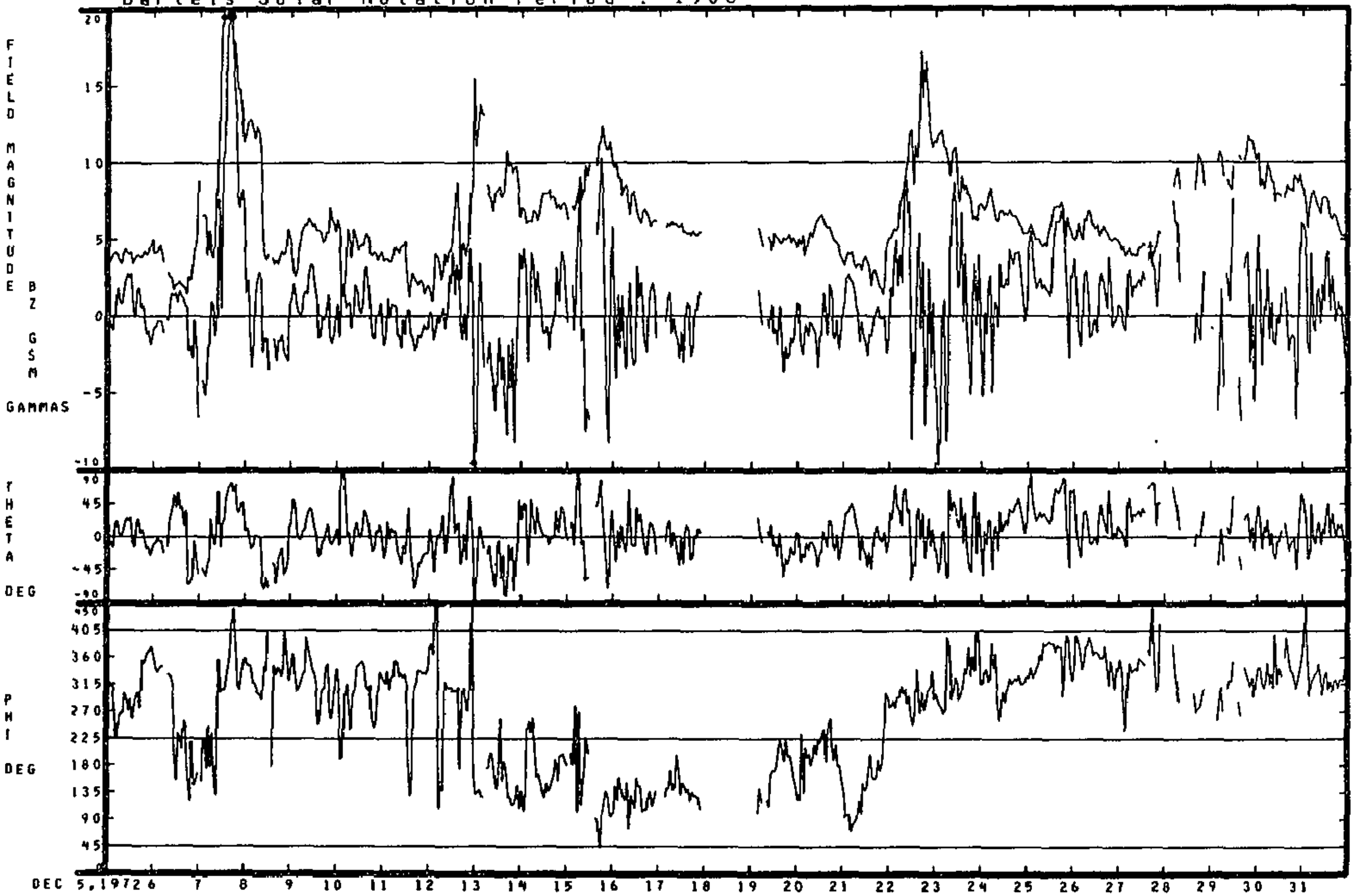


11/08/72 - 12/04/72

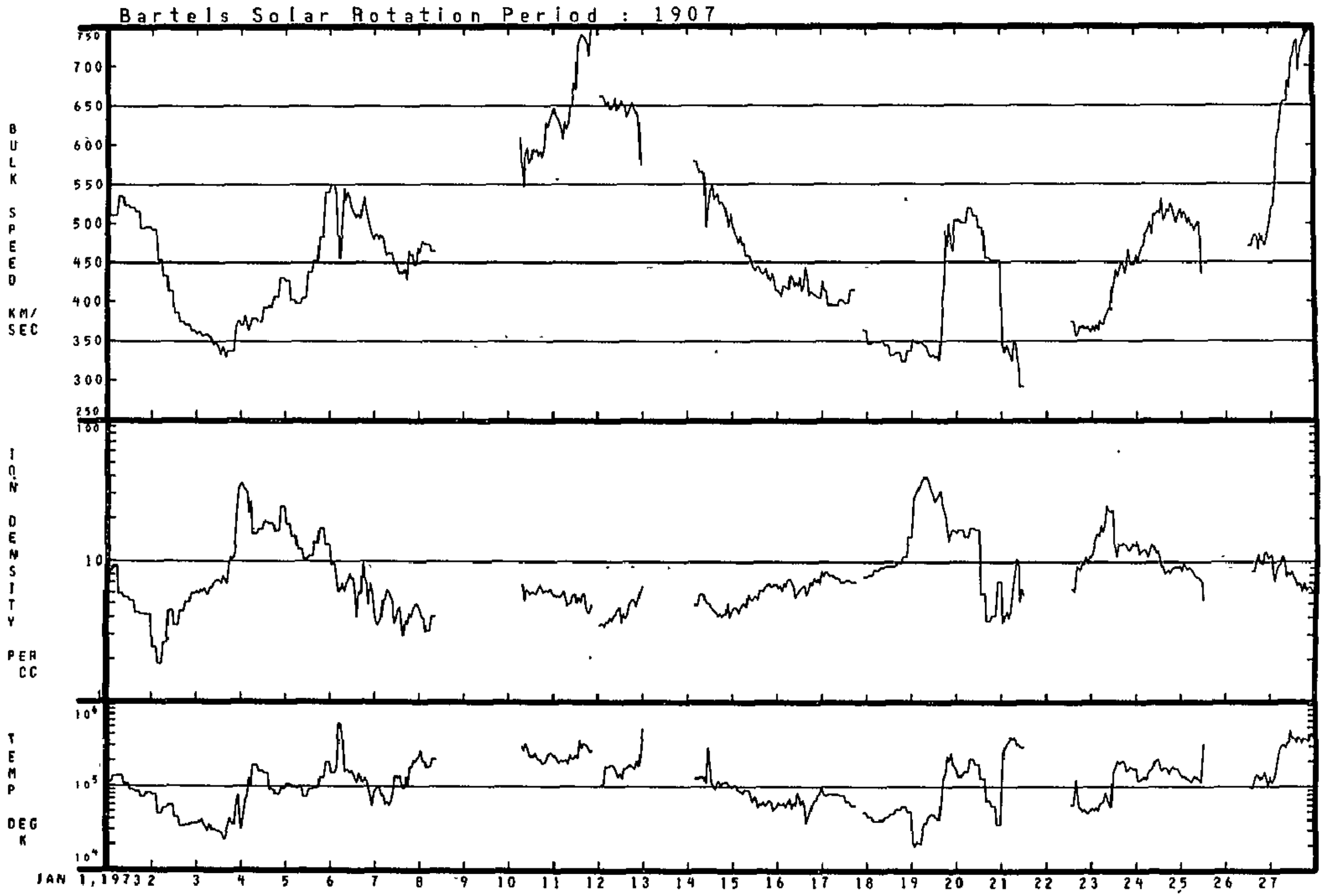
12/05/72 - 12/31/72



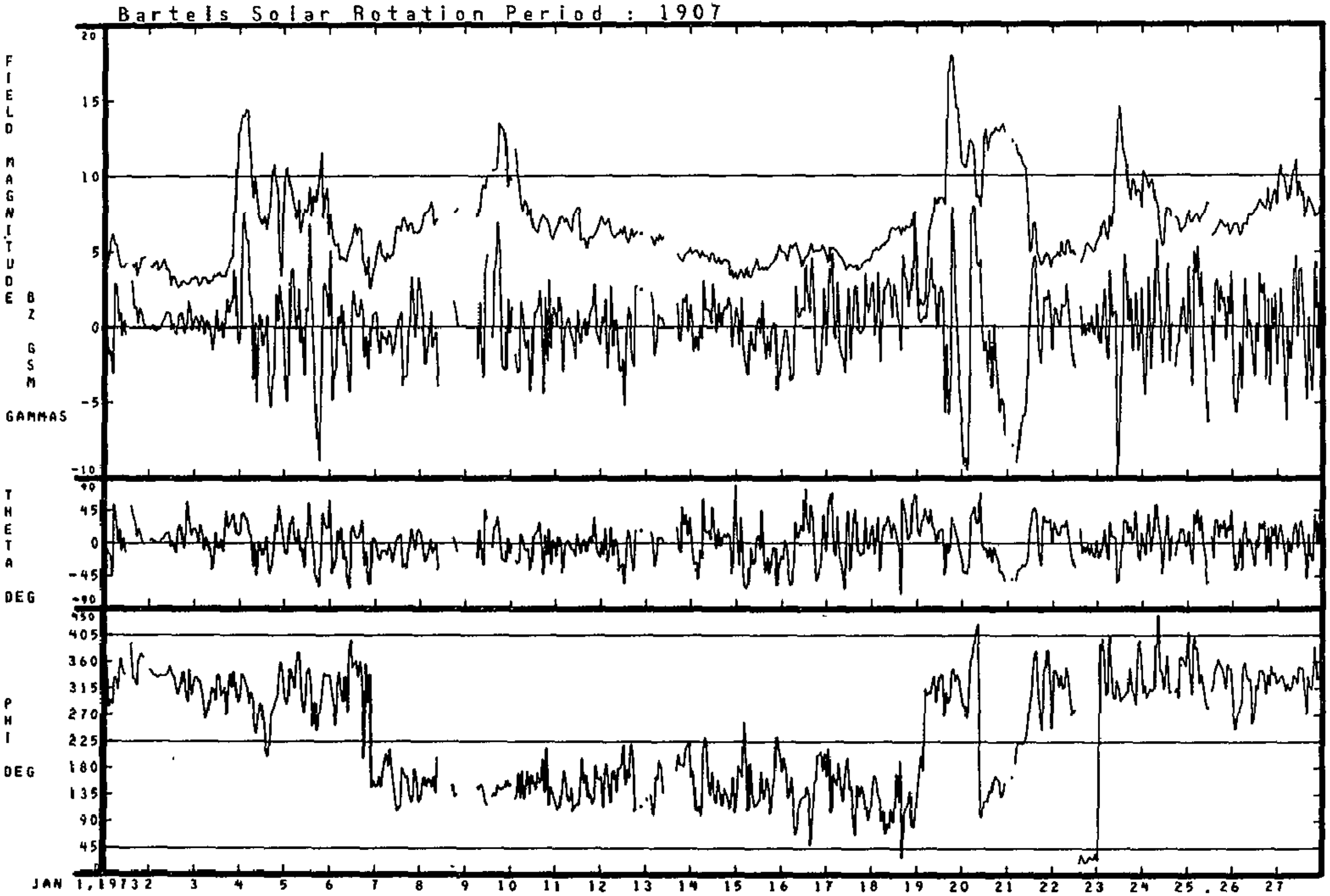
Bartels Solar Rotation Period : 1906



12/05/72 - 12/31/72

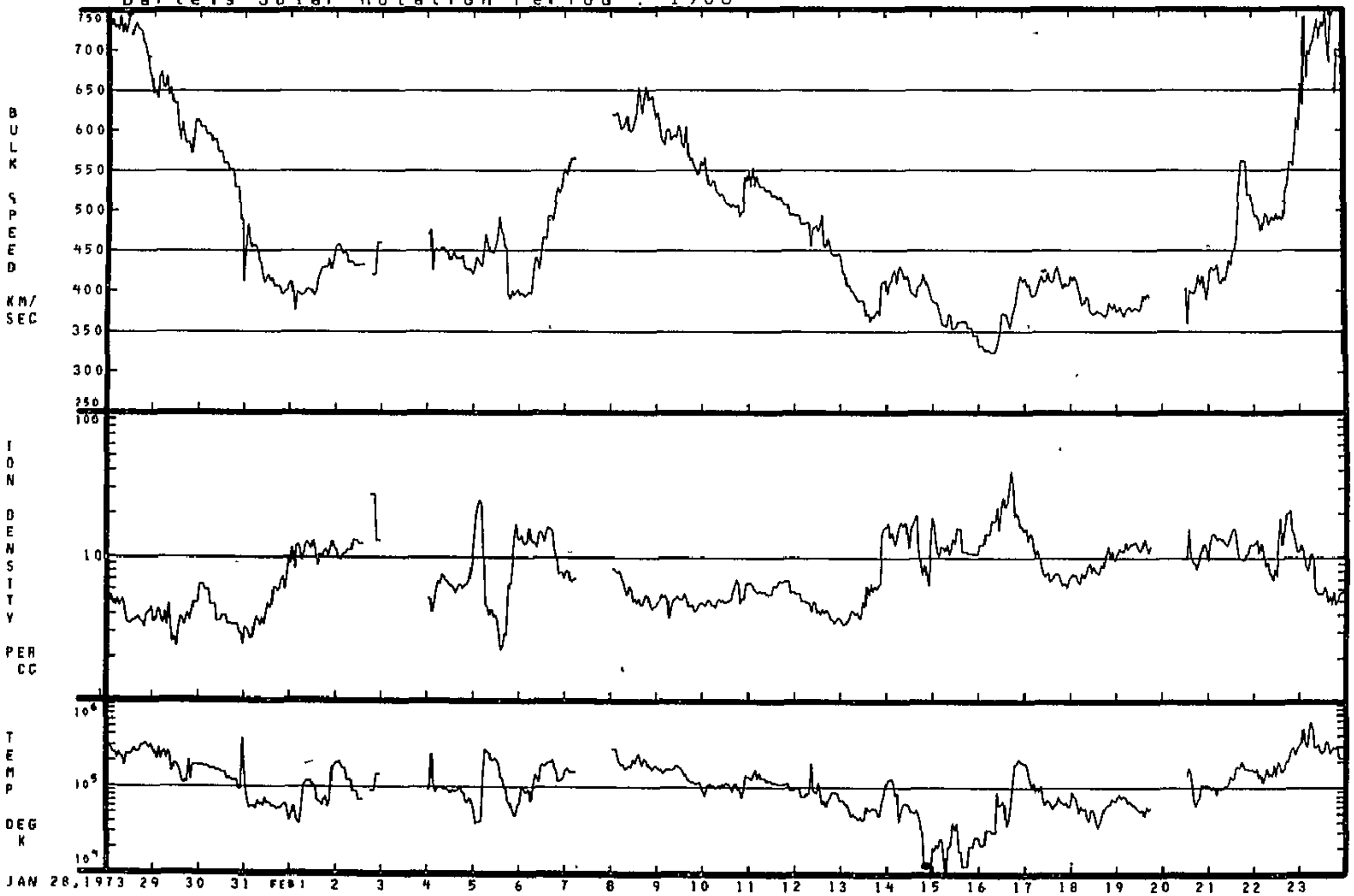


01/01/73 - 01/27/73



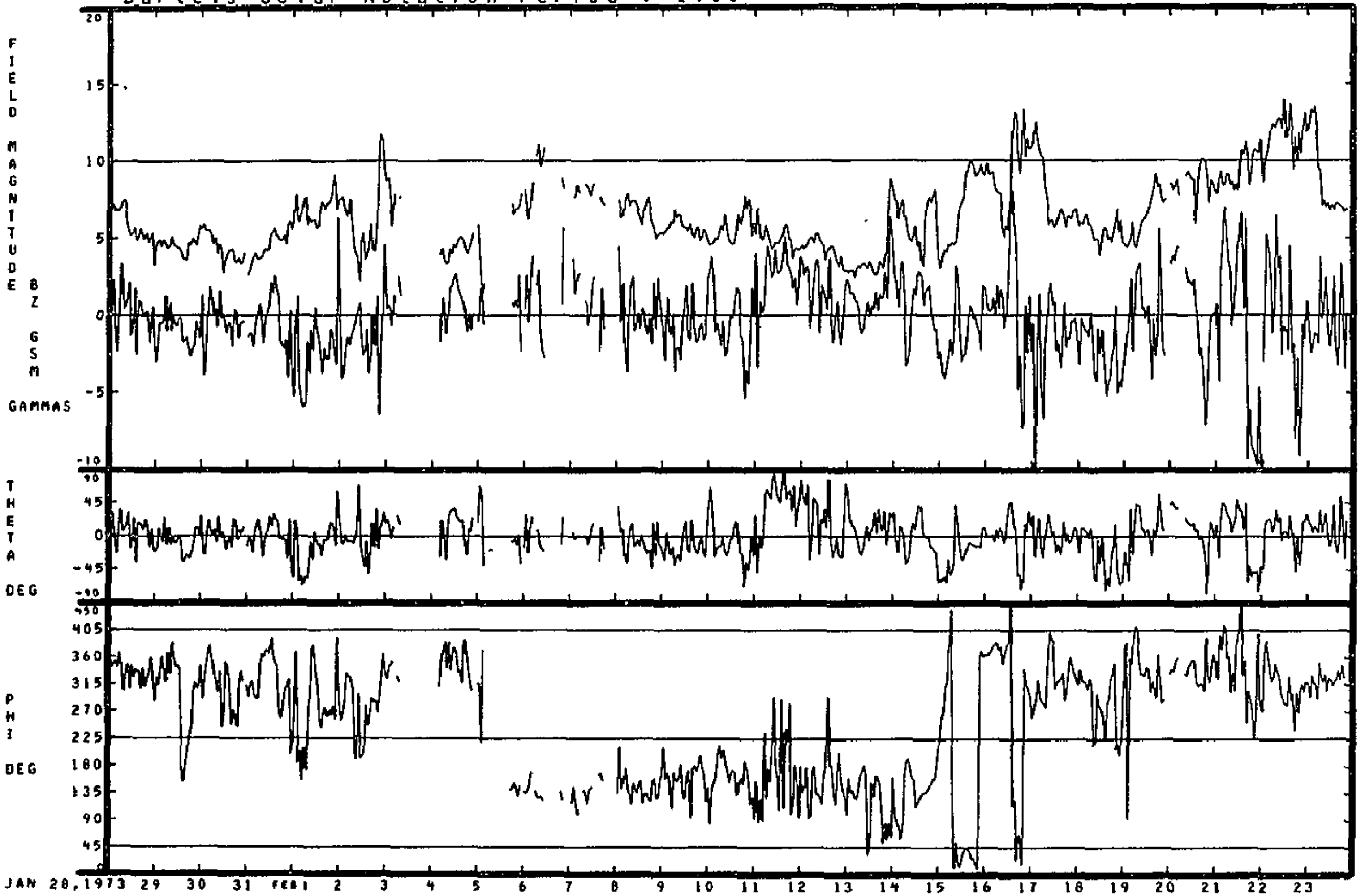
01/01/73 - 01/27/73

Bartels Solar Rotation Period : 1908

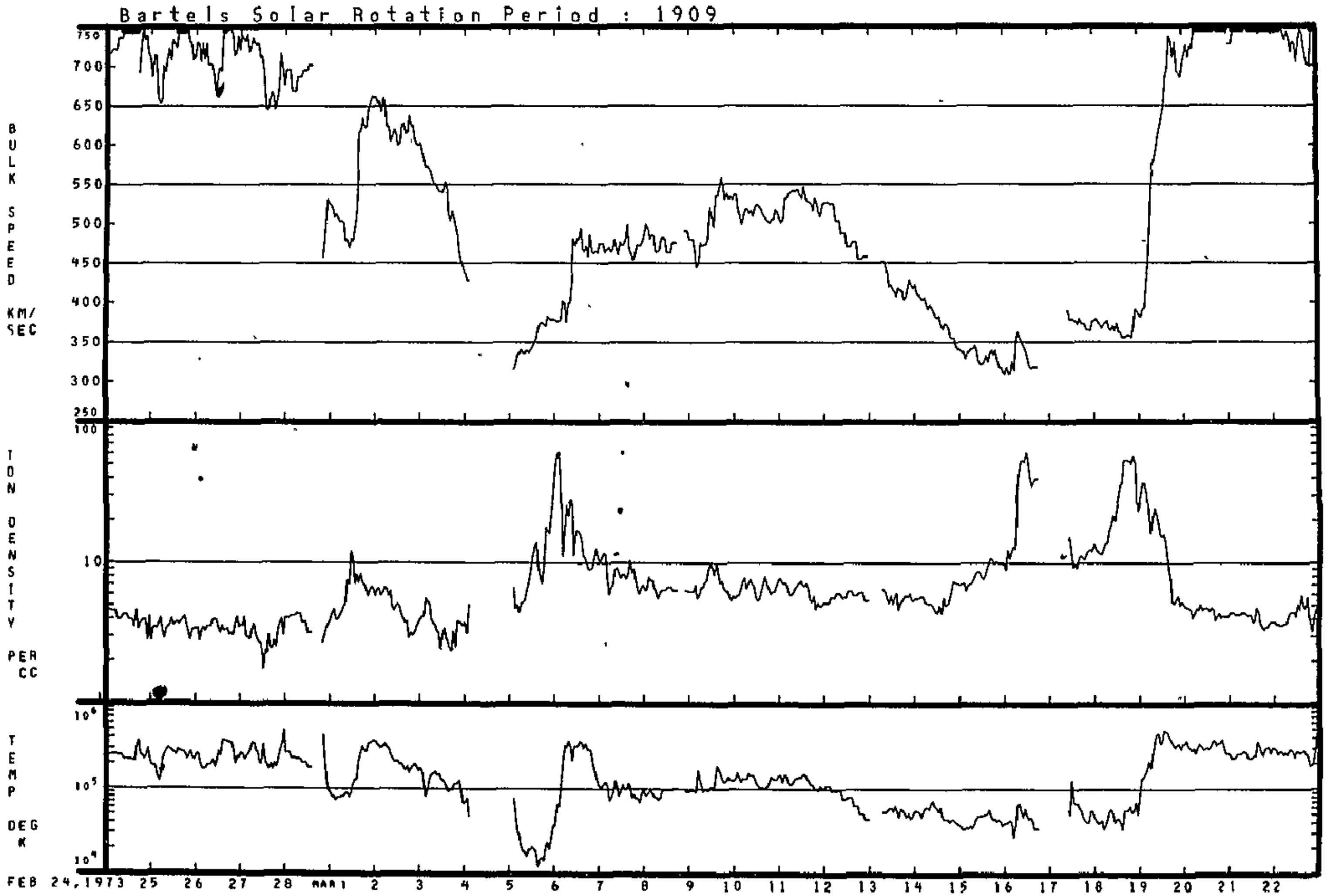


01/28/73 - 02/23/73

Bartels Solar Rotation Period : 1908

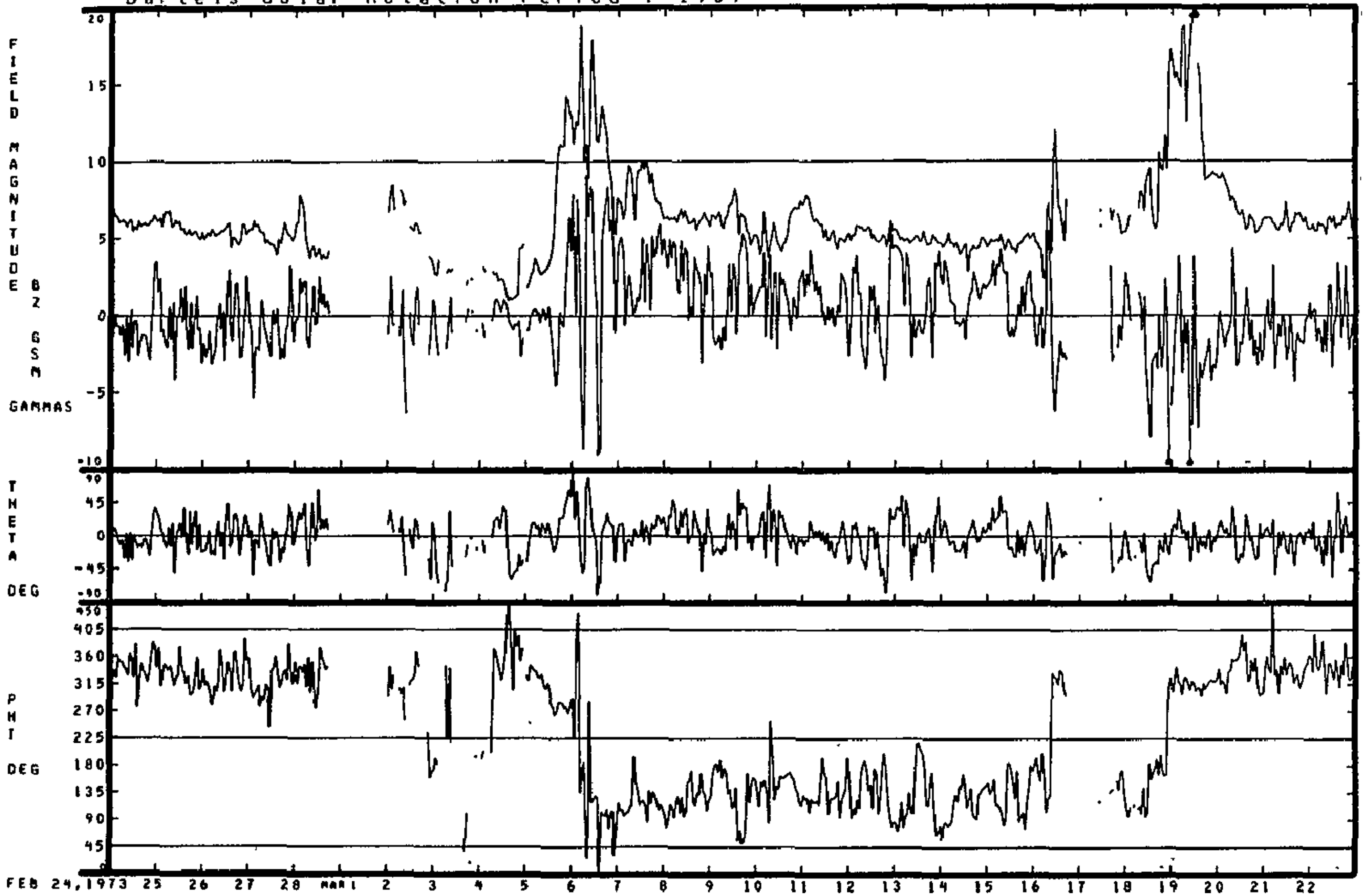


01/28/73 - 02/23/73



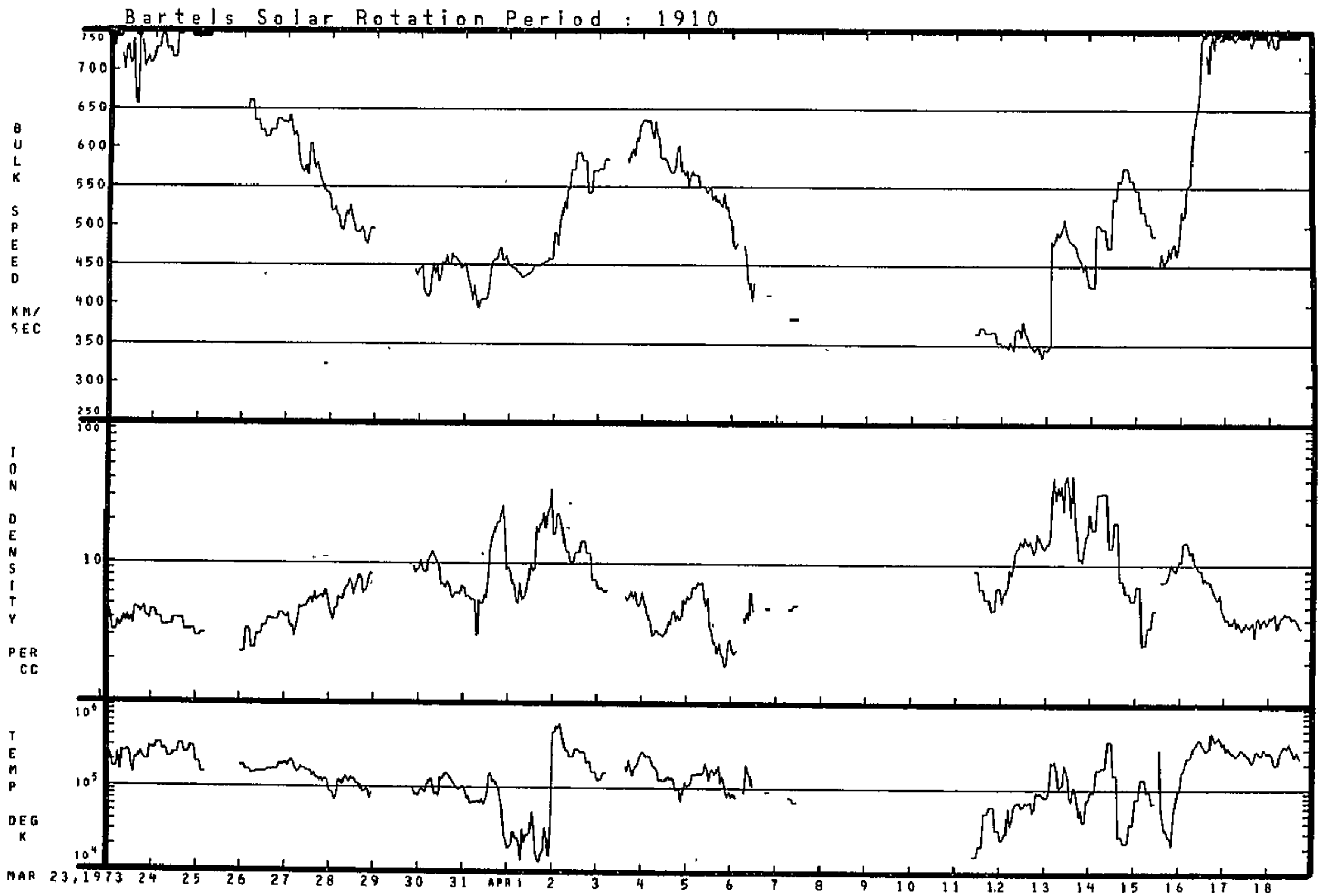
02/24/73 - 03/22/73

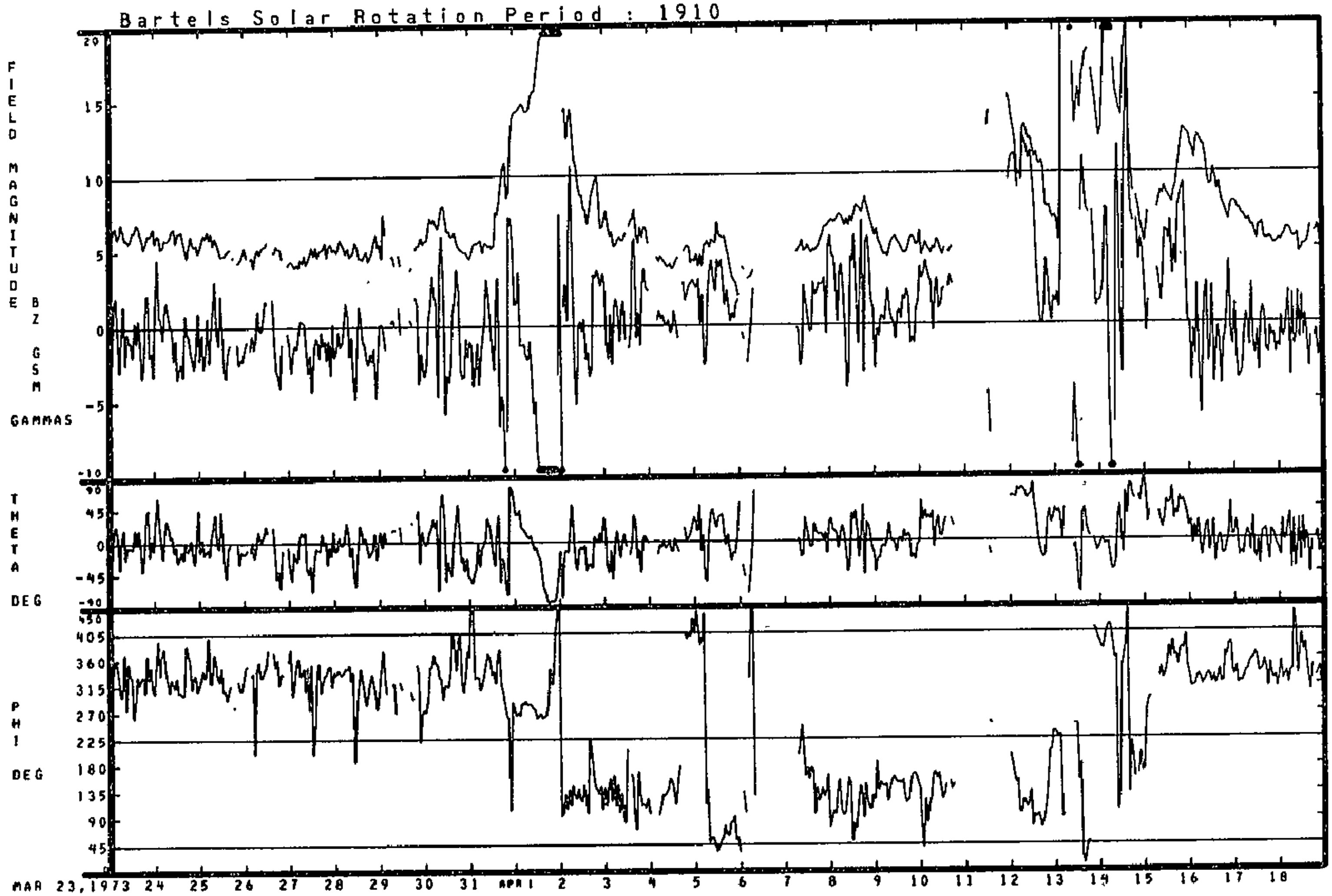
Bartels Solar Rotation Period : 1909



02/24/73 - 03/22/73

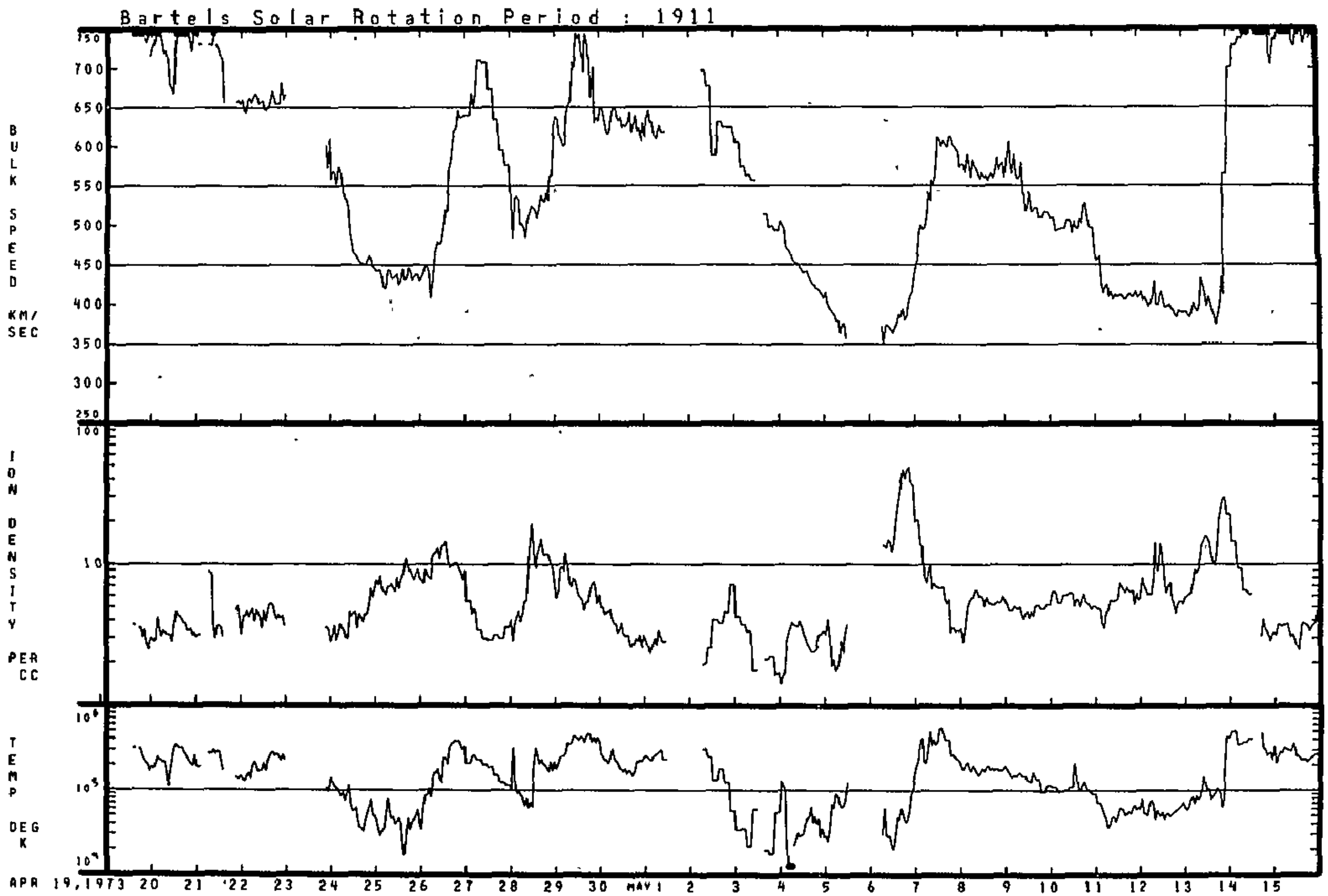
03/23/73 - 04/18/73



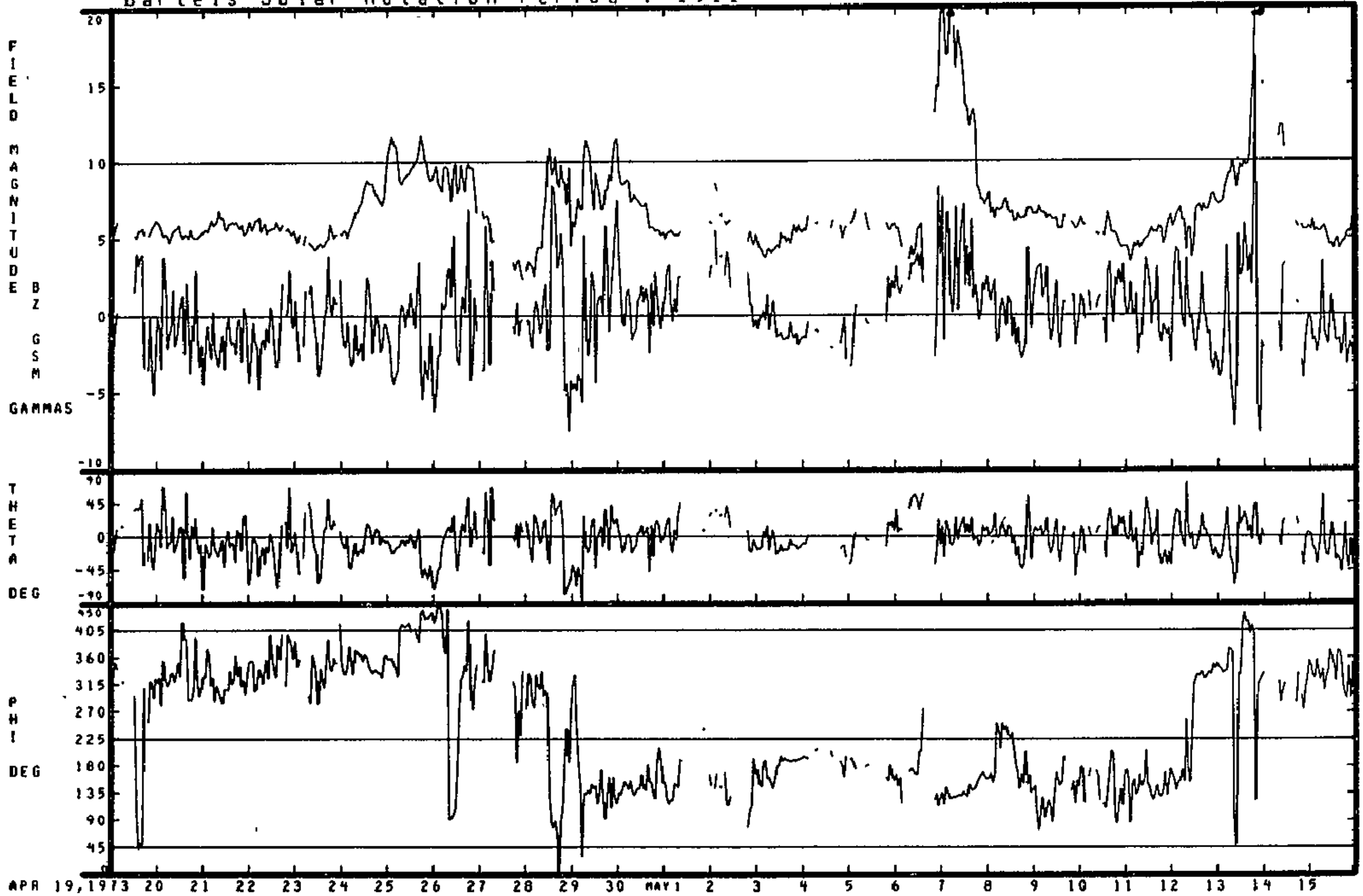


03/23/73 - 04/18/73

04/19/73 - 05/15/73

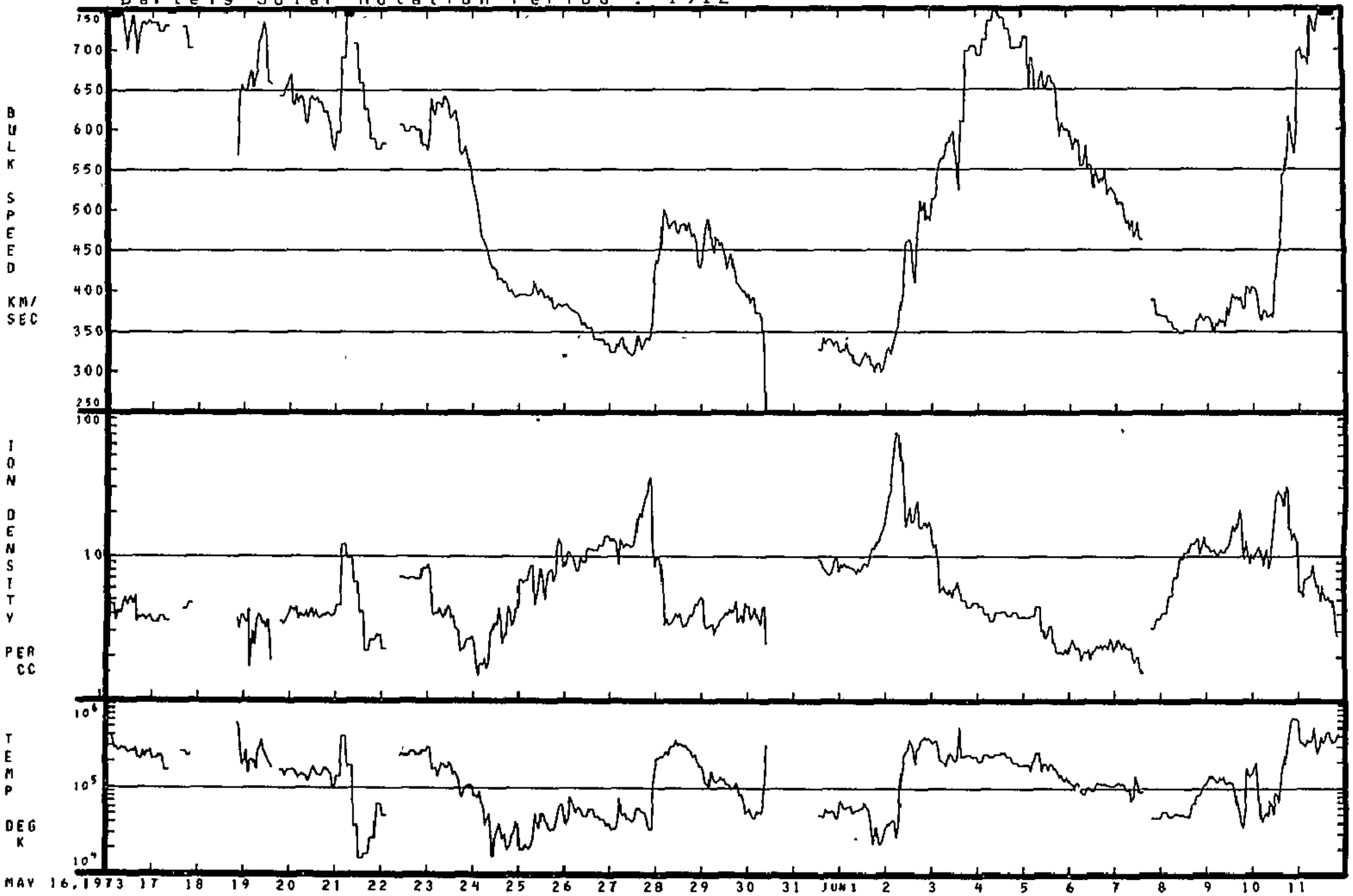


Bartels Solar Rotation Period : 1911

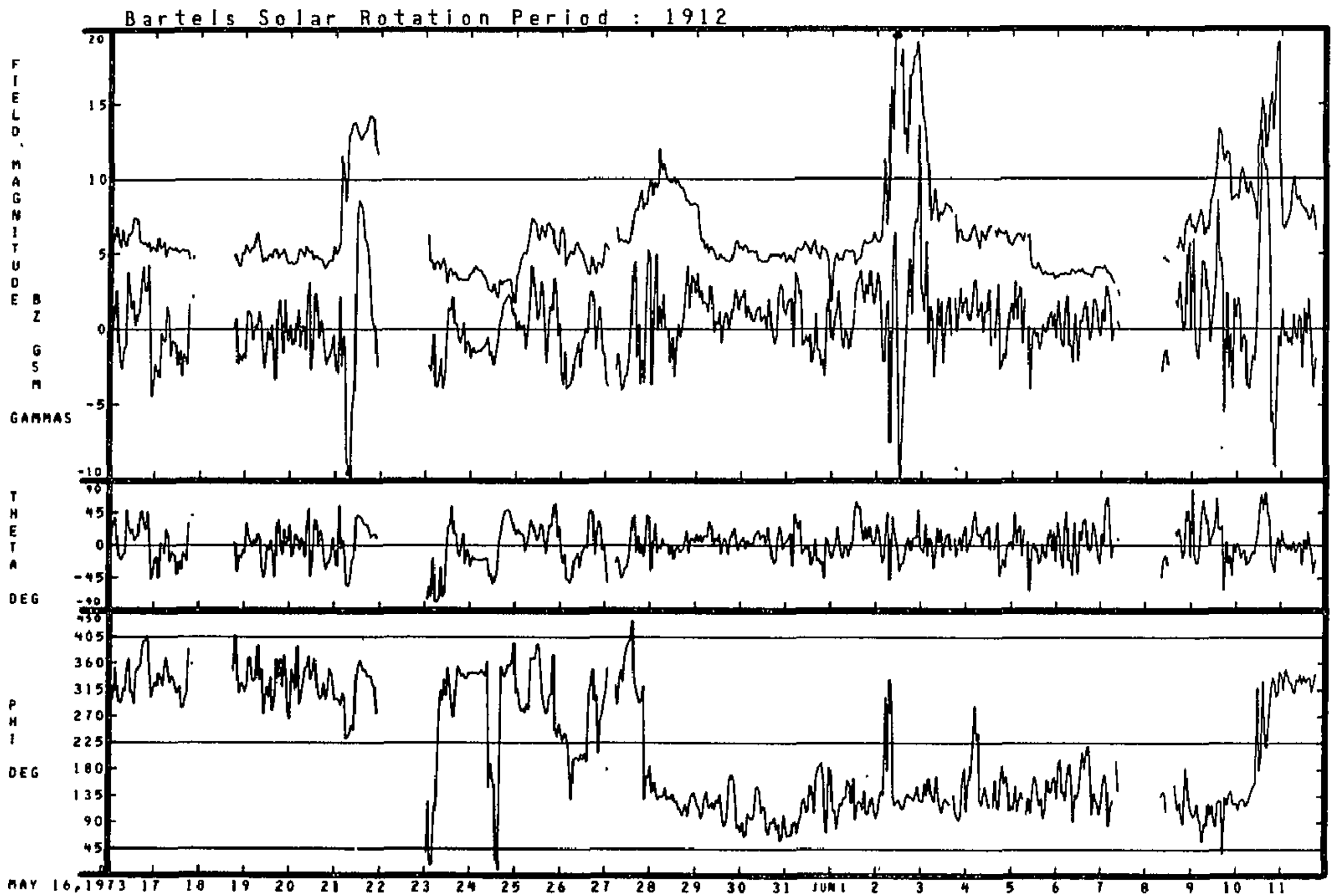


04/19/73 - 05/15/73

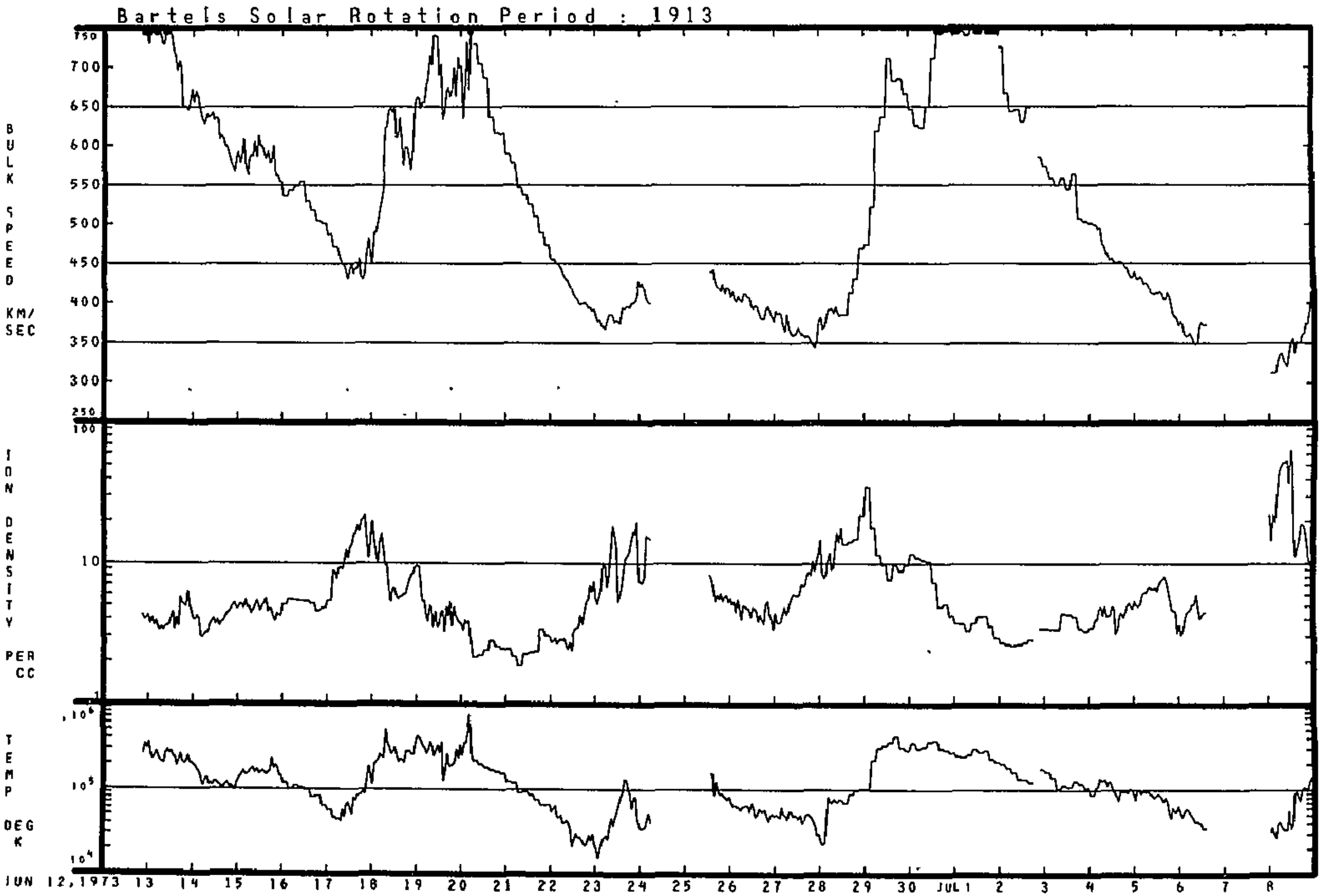
Bartels Solar Rotation Period : 1912



05/16/73 - 06/11/73

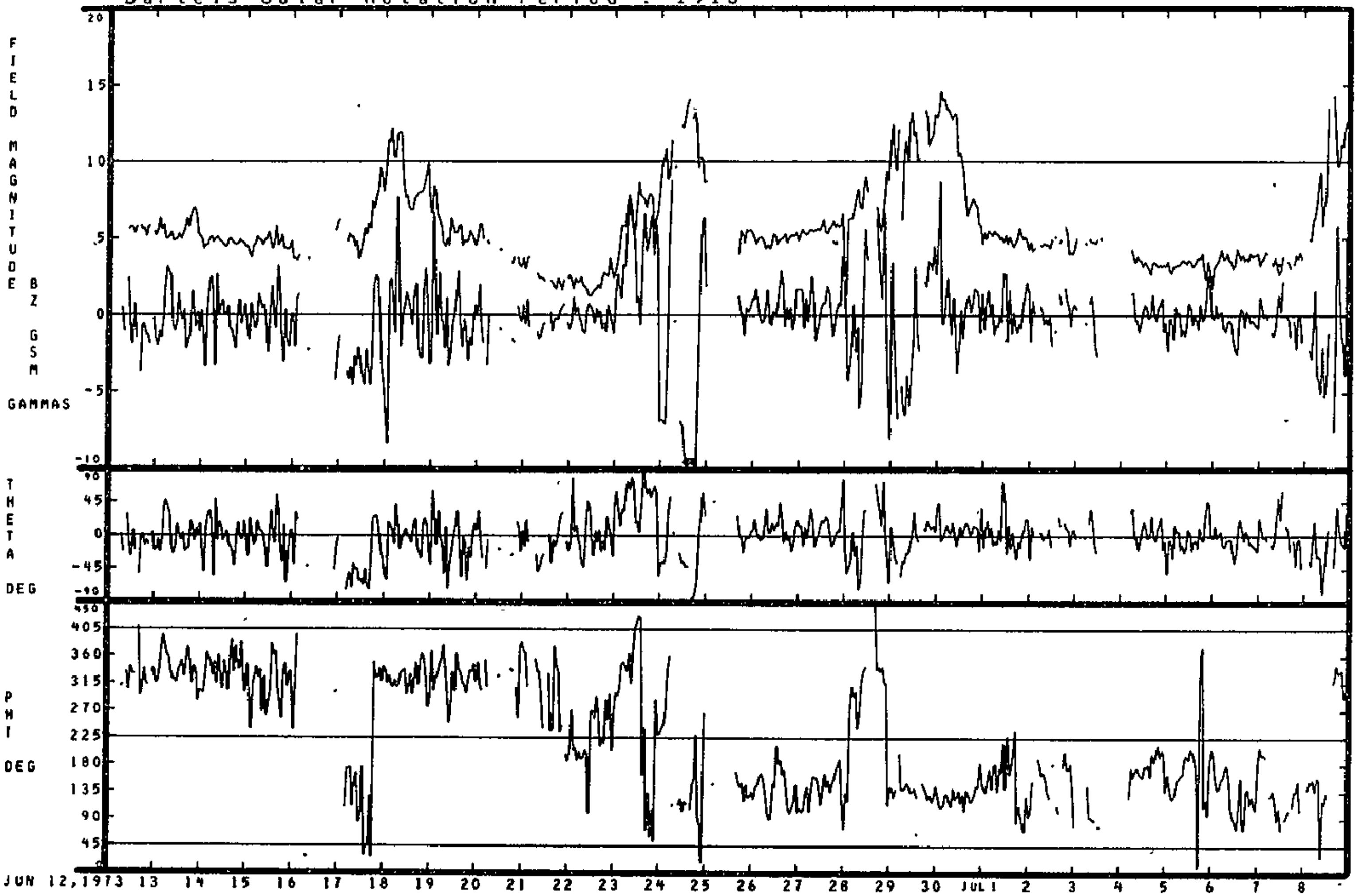


05/16/73 - 06/11/73



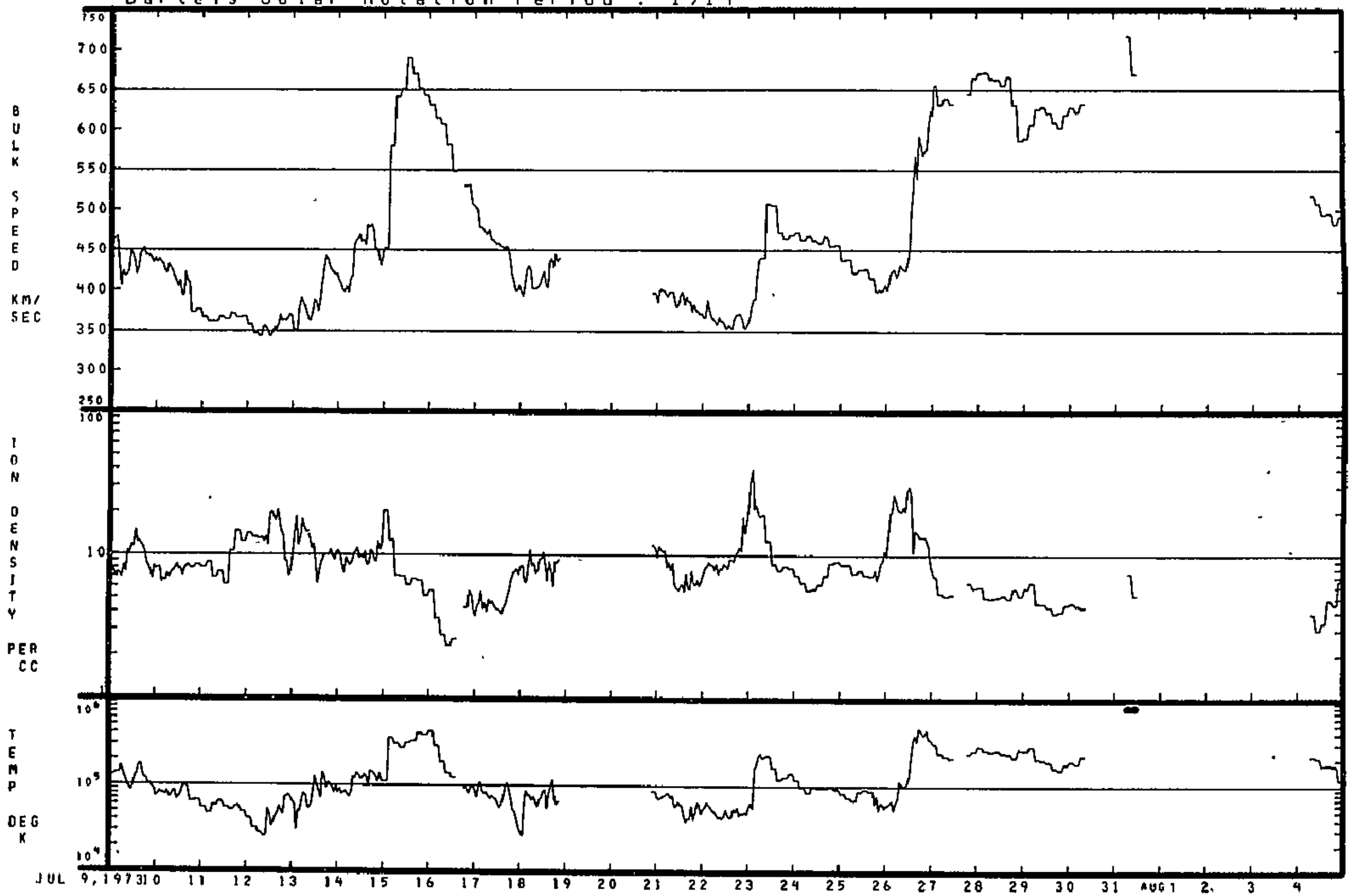
06/12/73 - 07/08/73

Bartels Solar Rotation Period : 1913



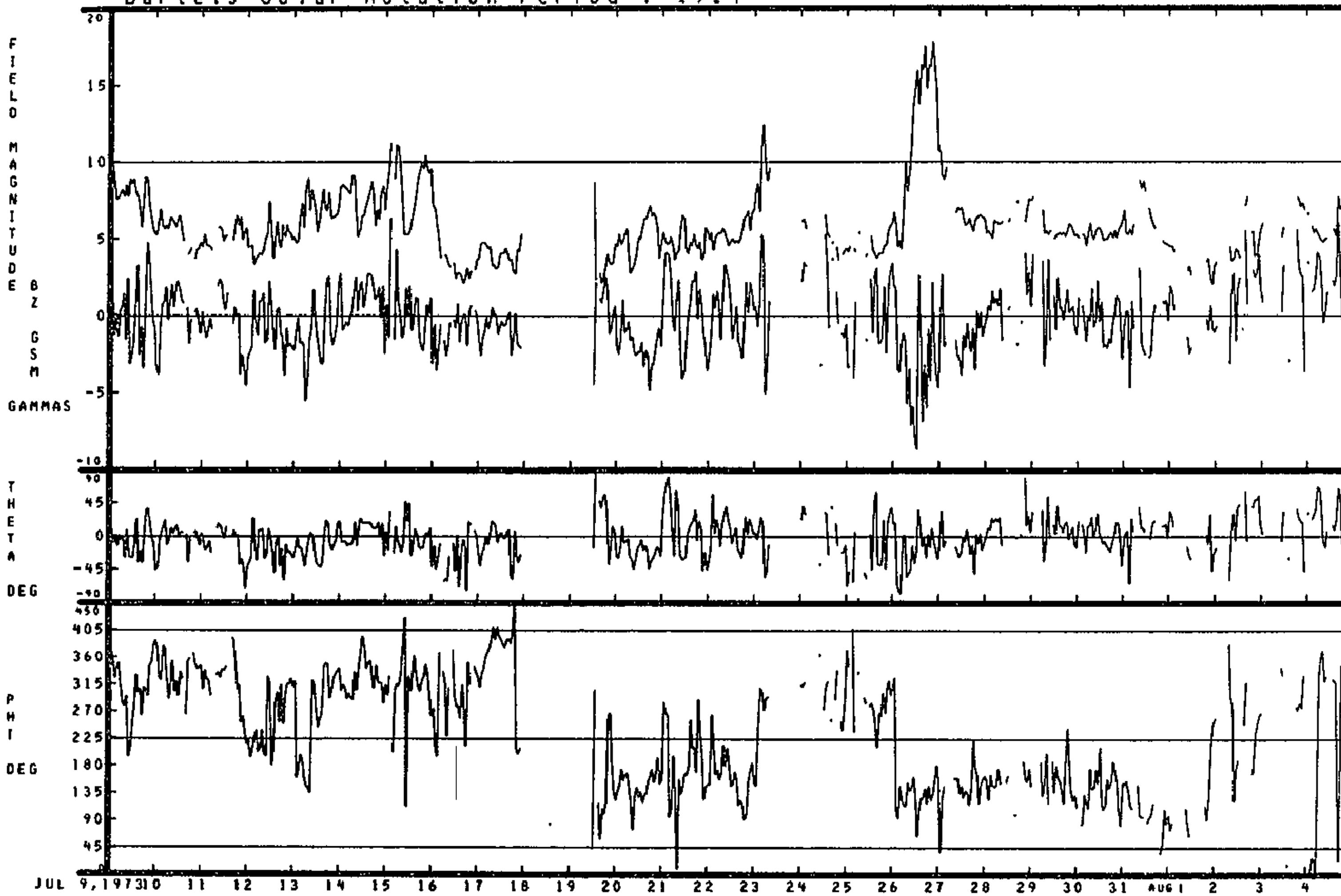
06/12/73 - 07/08/73

Bartels Solar Rotation Period : 1914



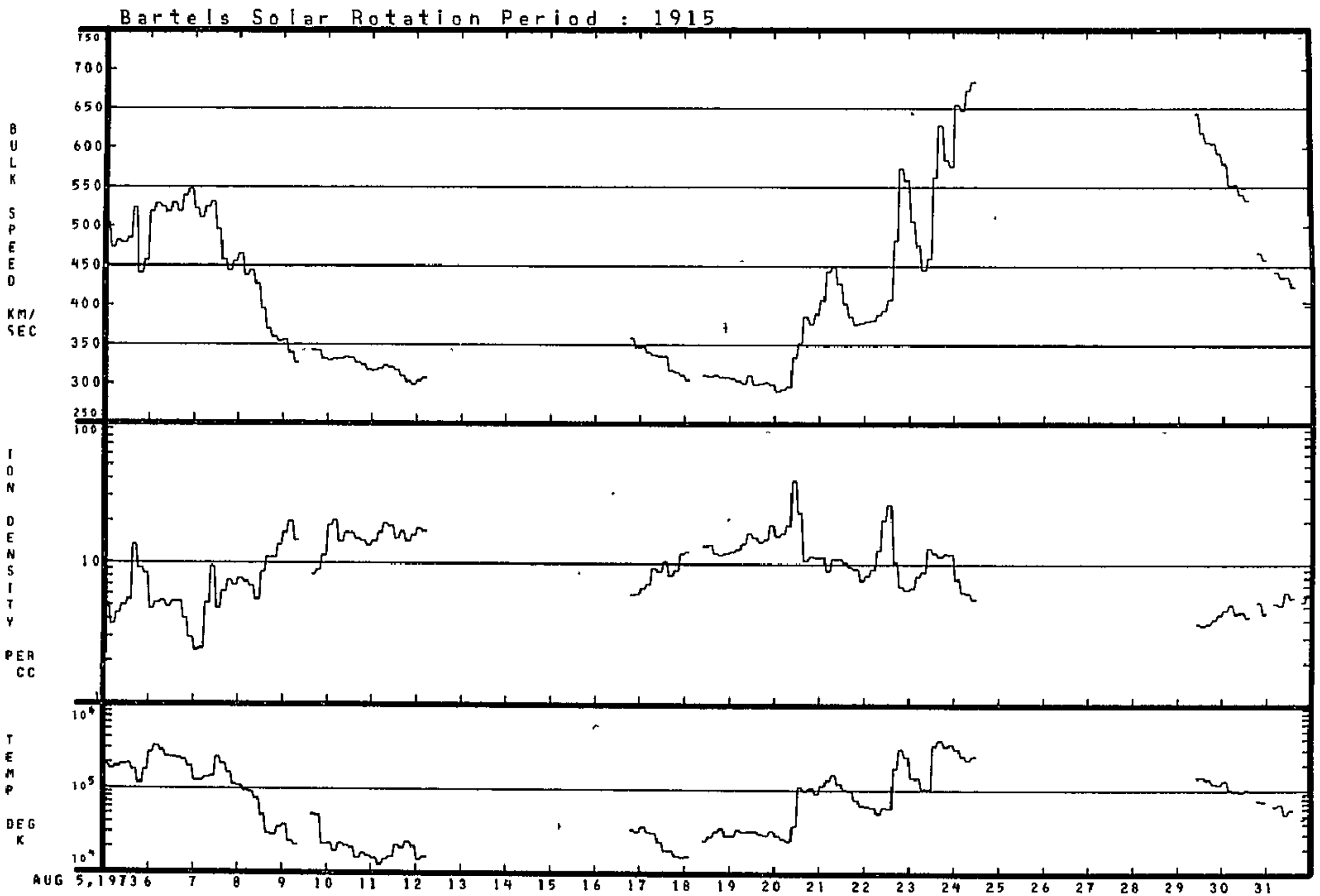
07/09/73 - 08/04/73

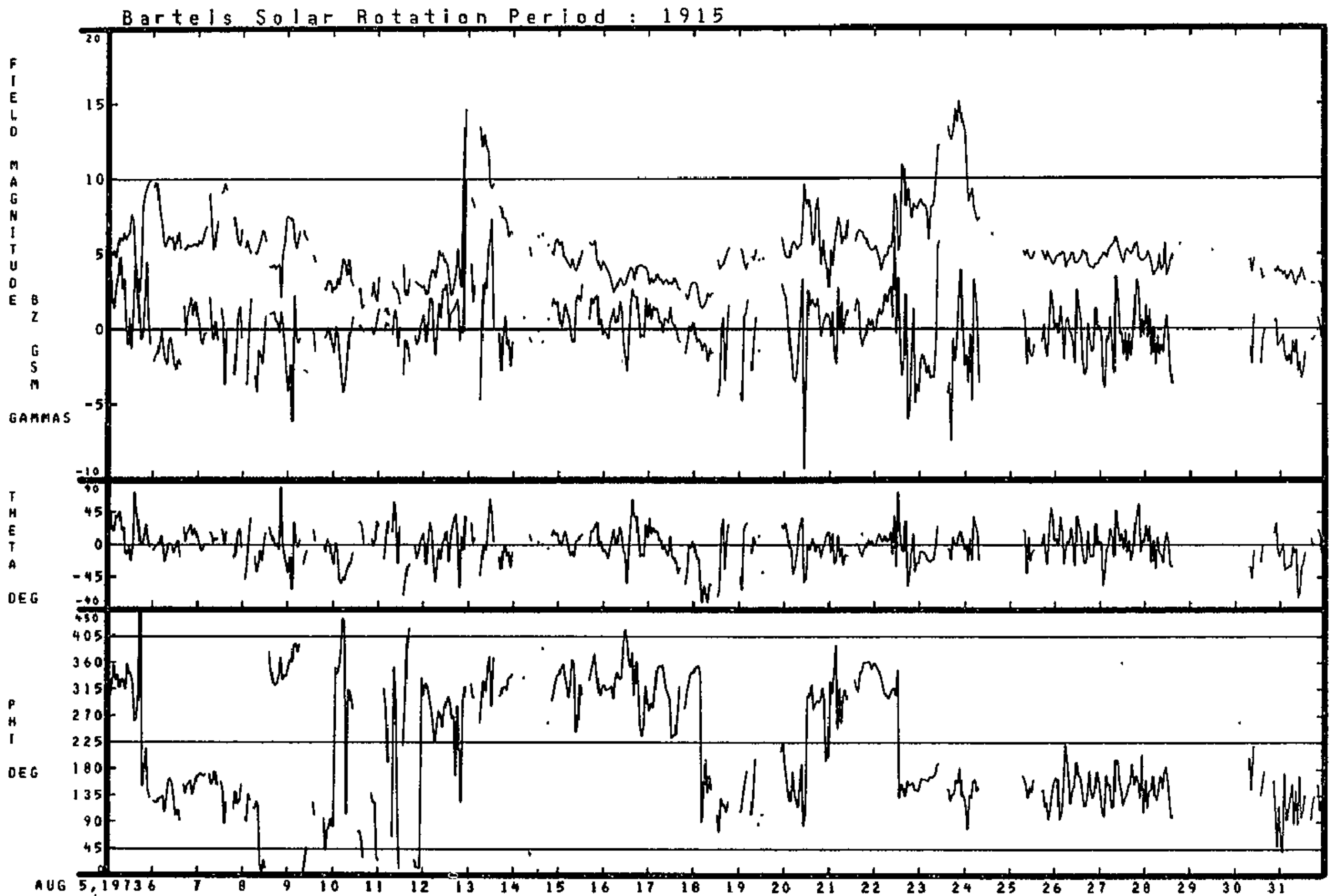
Bartels Solar Rotation Period : 1914



07/09/73 - 08/04/73

08/05/73 - 08/31/73

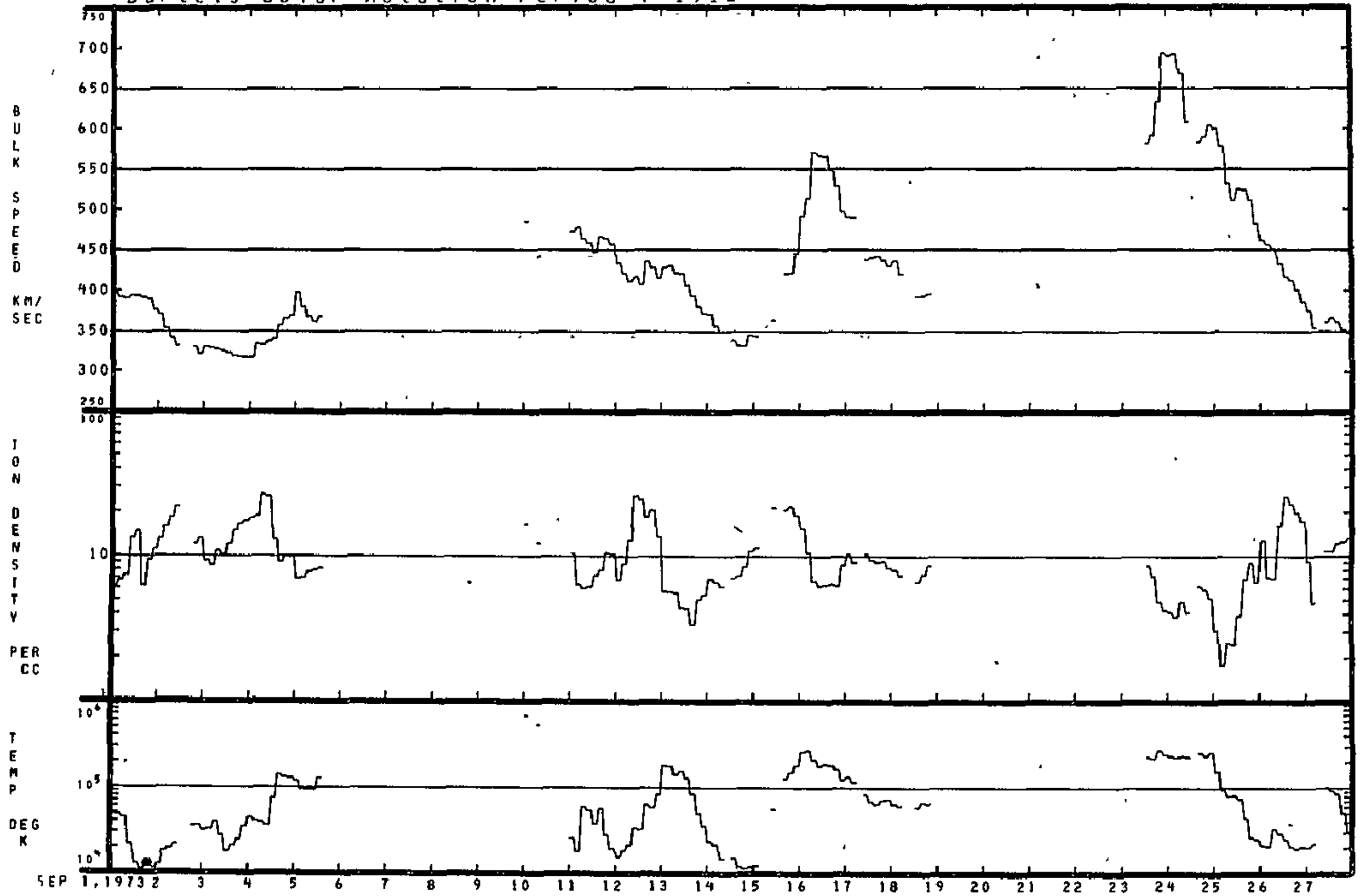




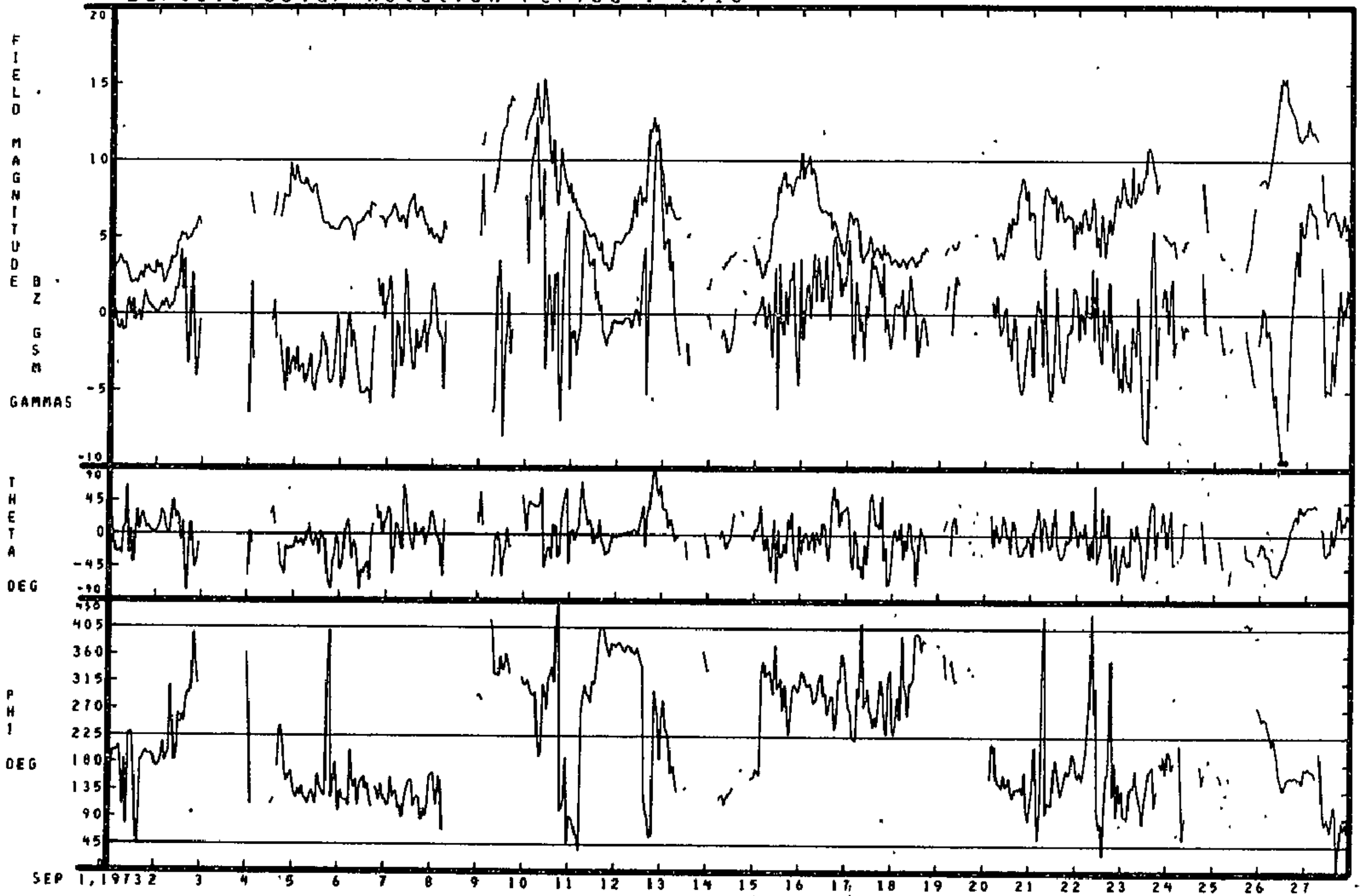
08/05/73 - 08/31/73

09/01/73 - 09/27/73

Bartels Solar Rotation Period : 1916



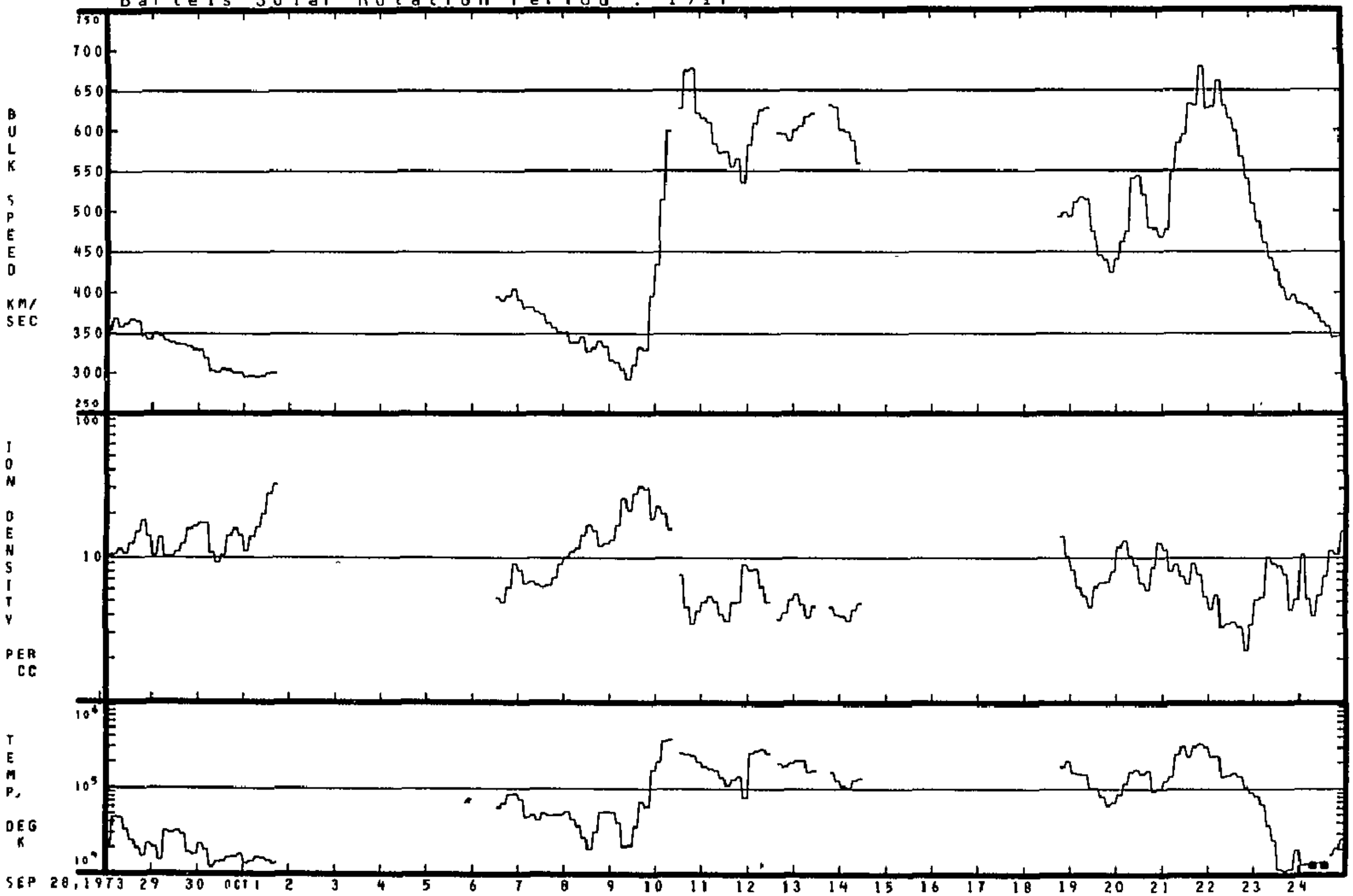
Bartels Solar Rotation Period : 1916



09/01/73 - 09/27/73

09/28/73 - 10/24/73

Bartels Solar Rotation Period : 1917



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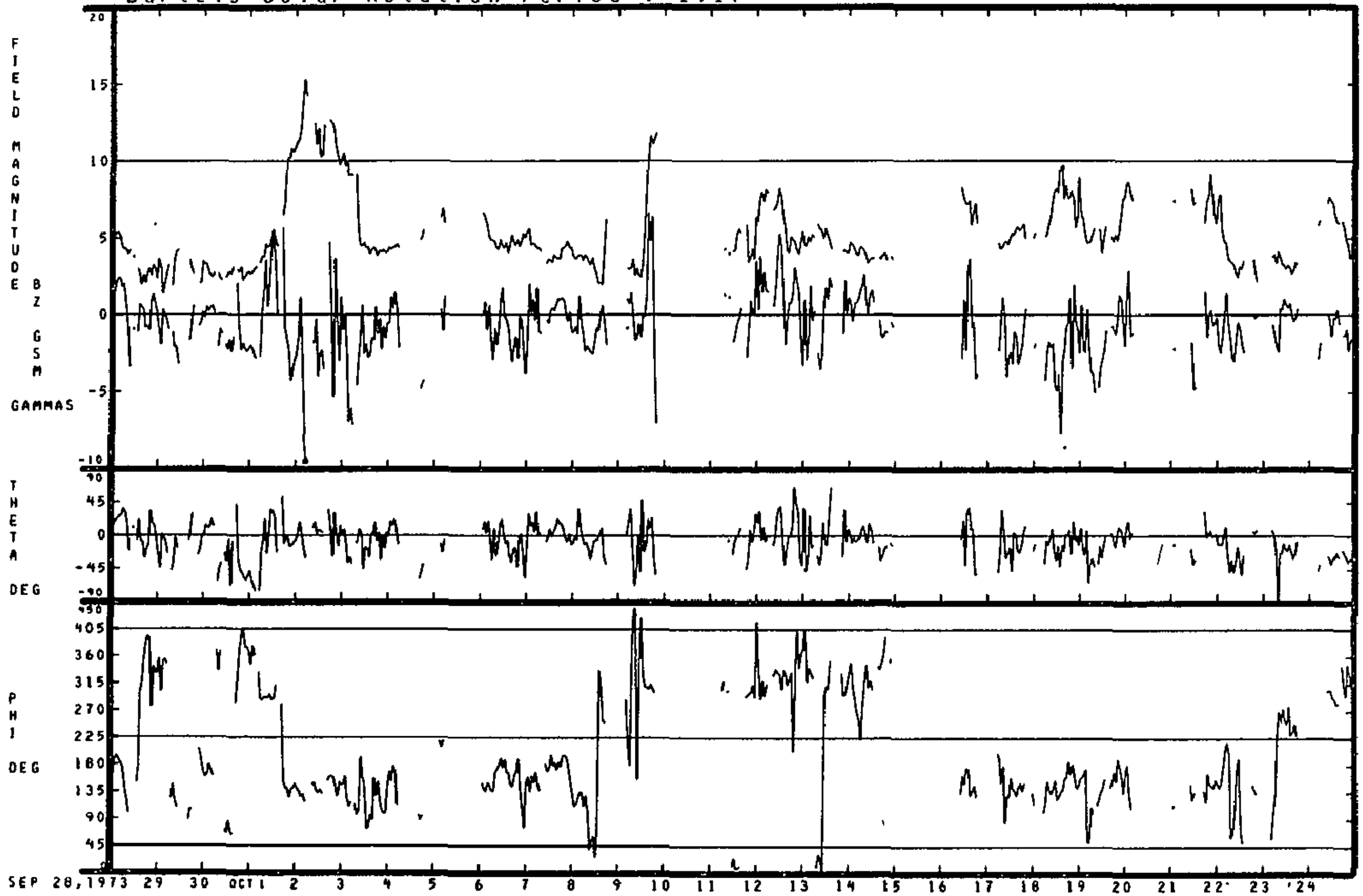
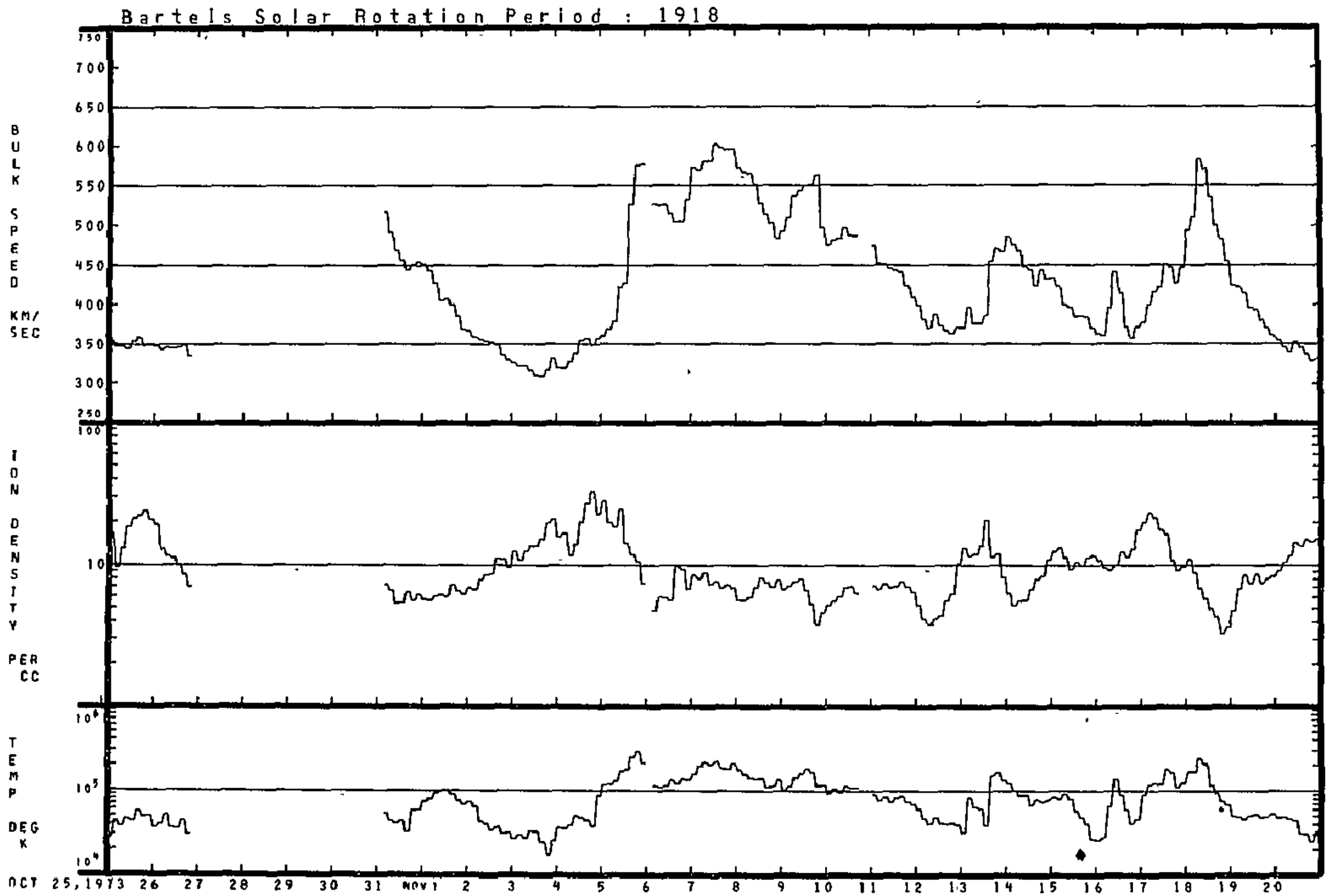
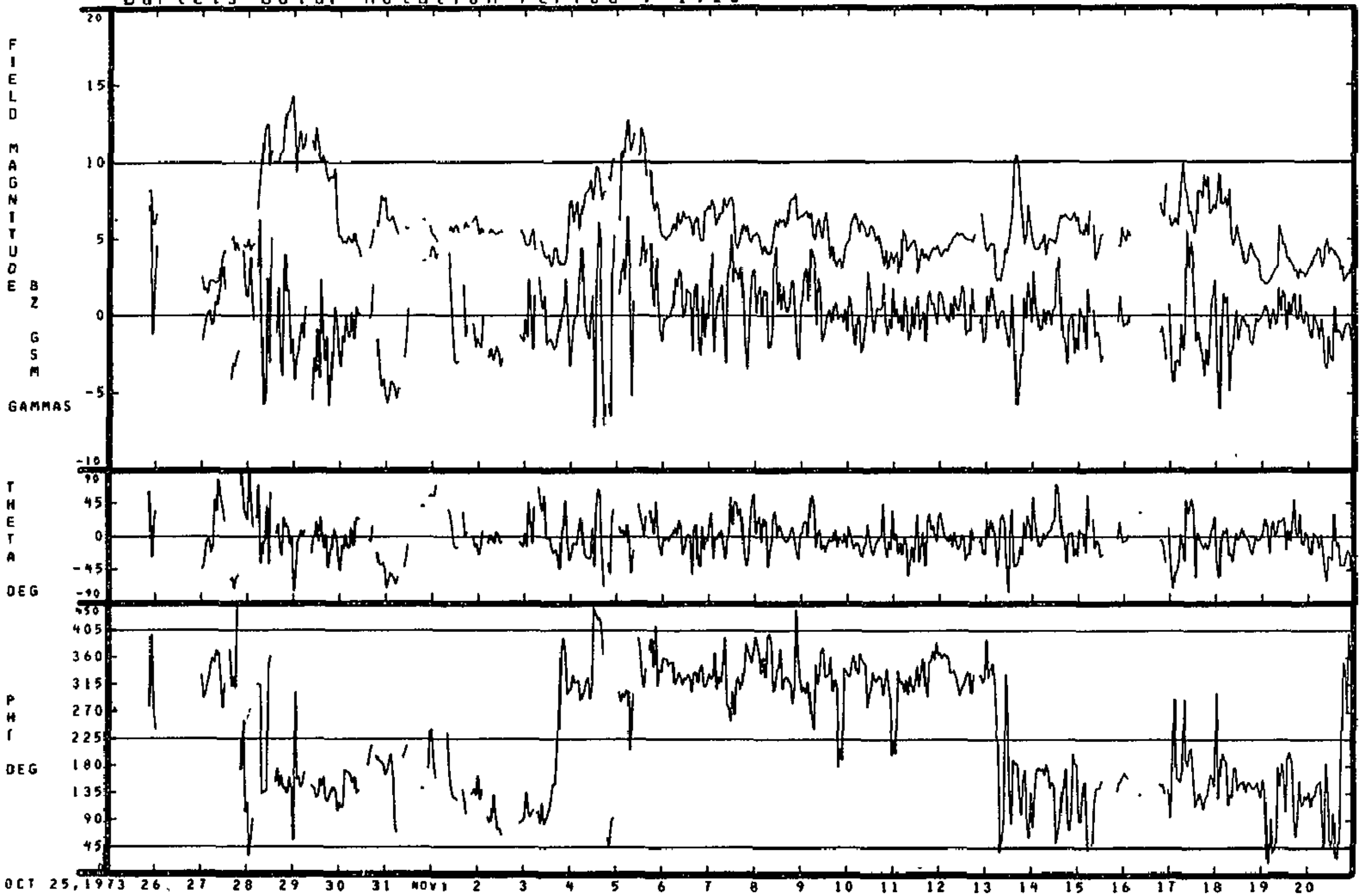


09/28/73 - 10/24/73

10/25/73 - 11/20/73



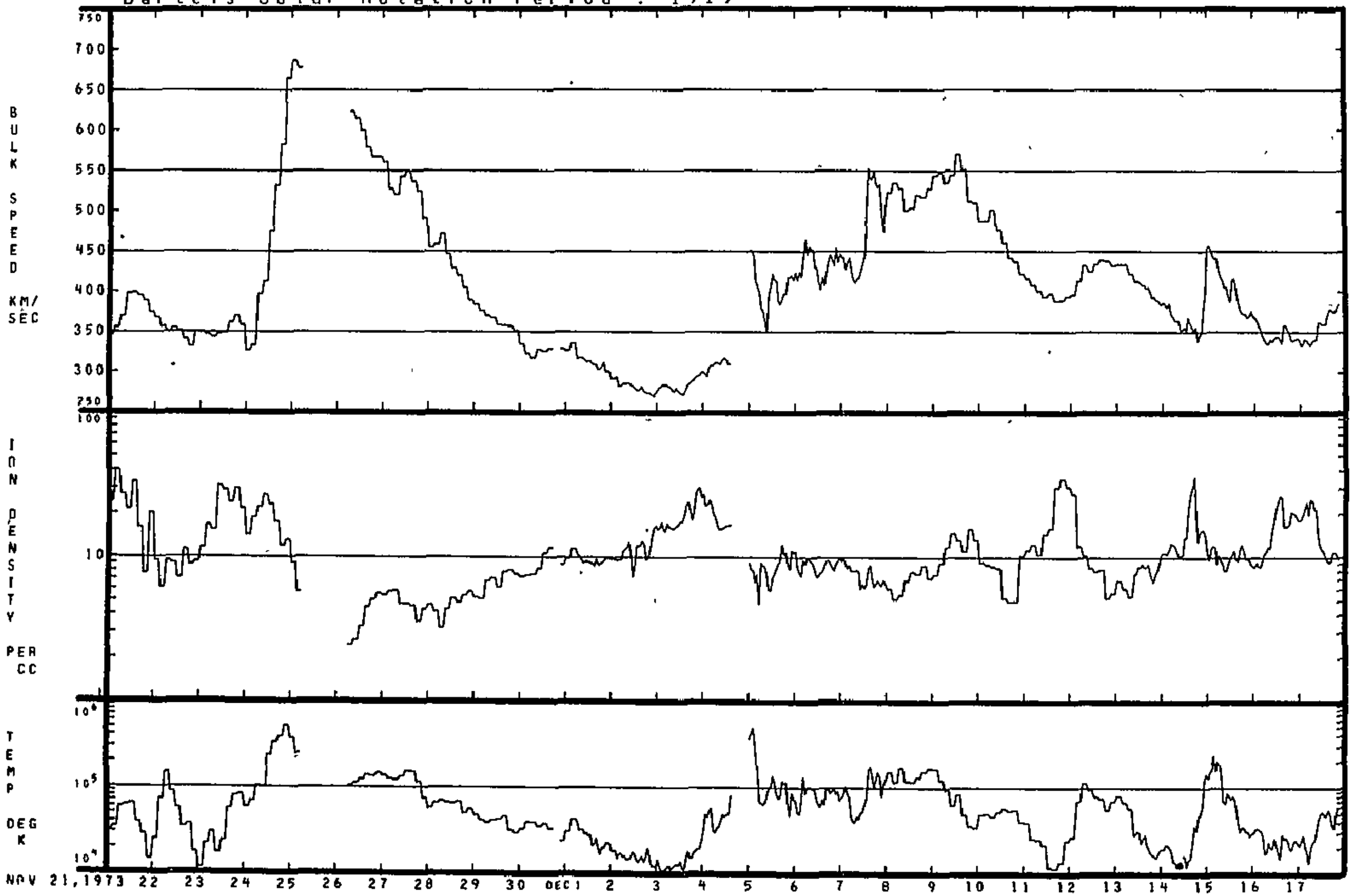
Bartels Solar Rotation Period : 1918



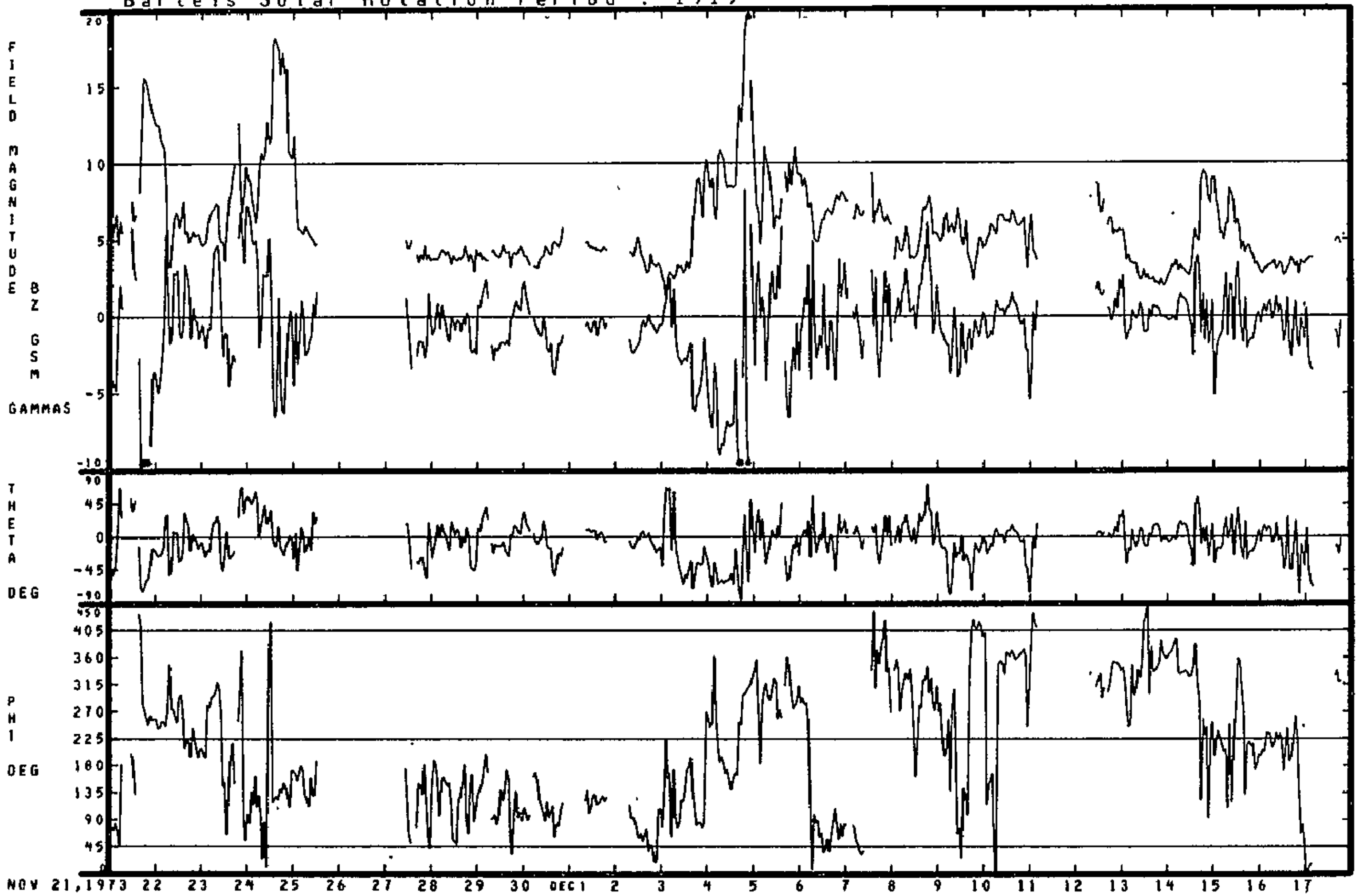
10/25/73 - 11/20/73

11/21/73 - 12/17/73

Bartels Solar Rotation Period : 1919

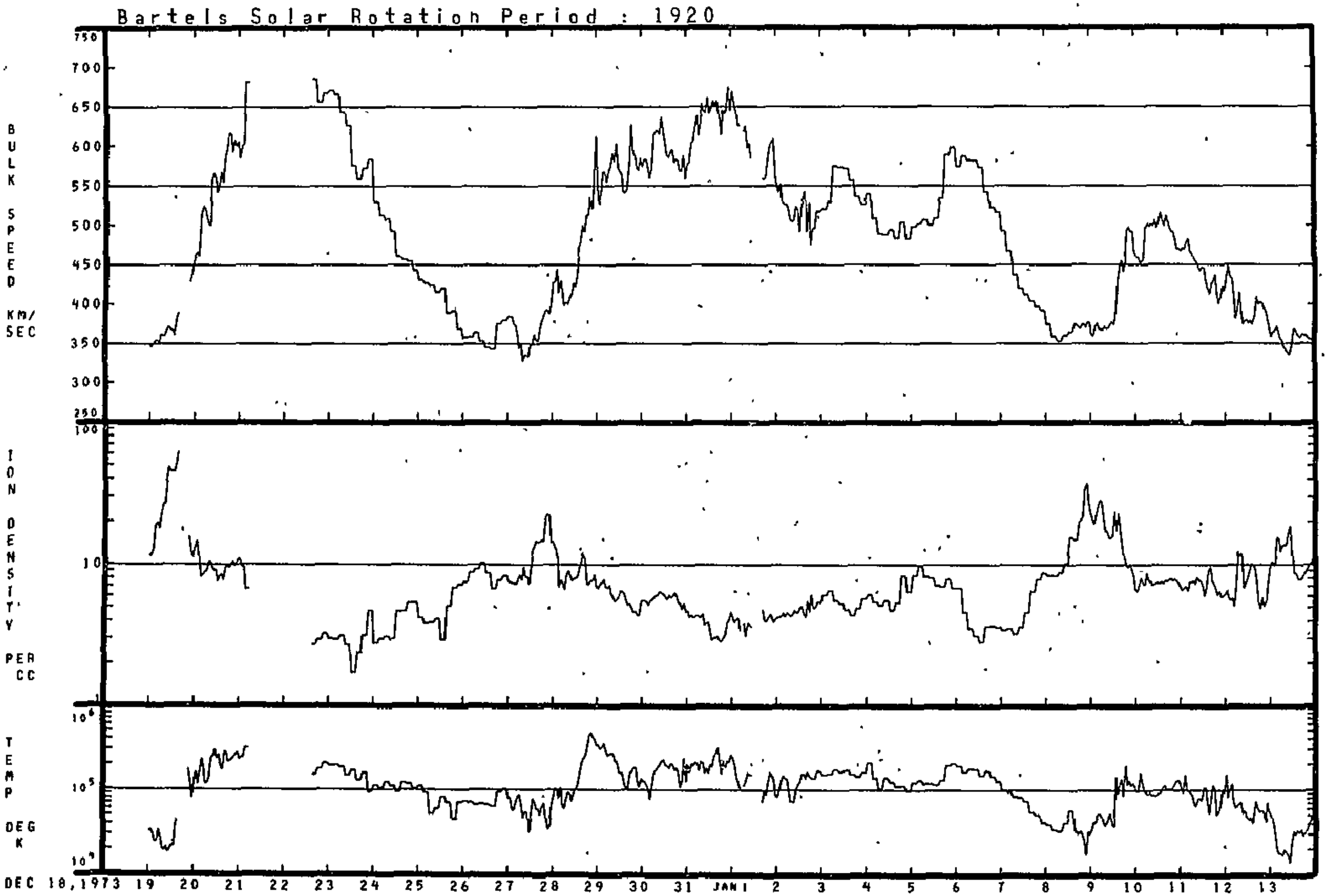


Bartels Solar Rotation Period : 1919

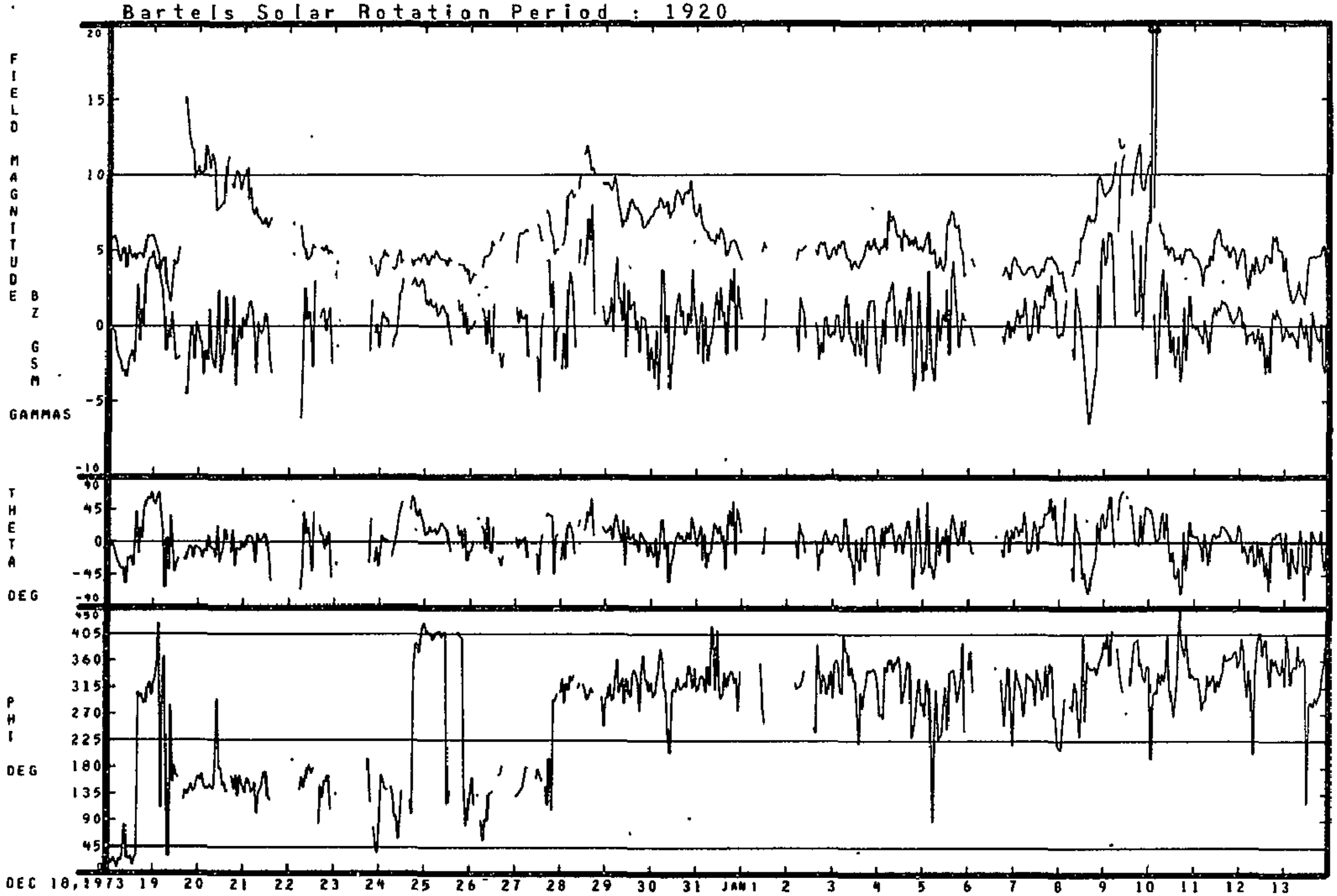


11/21/73 - 12/17/73

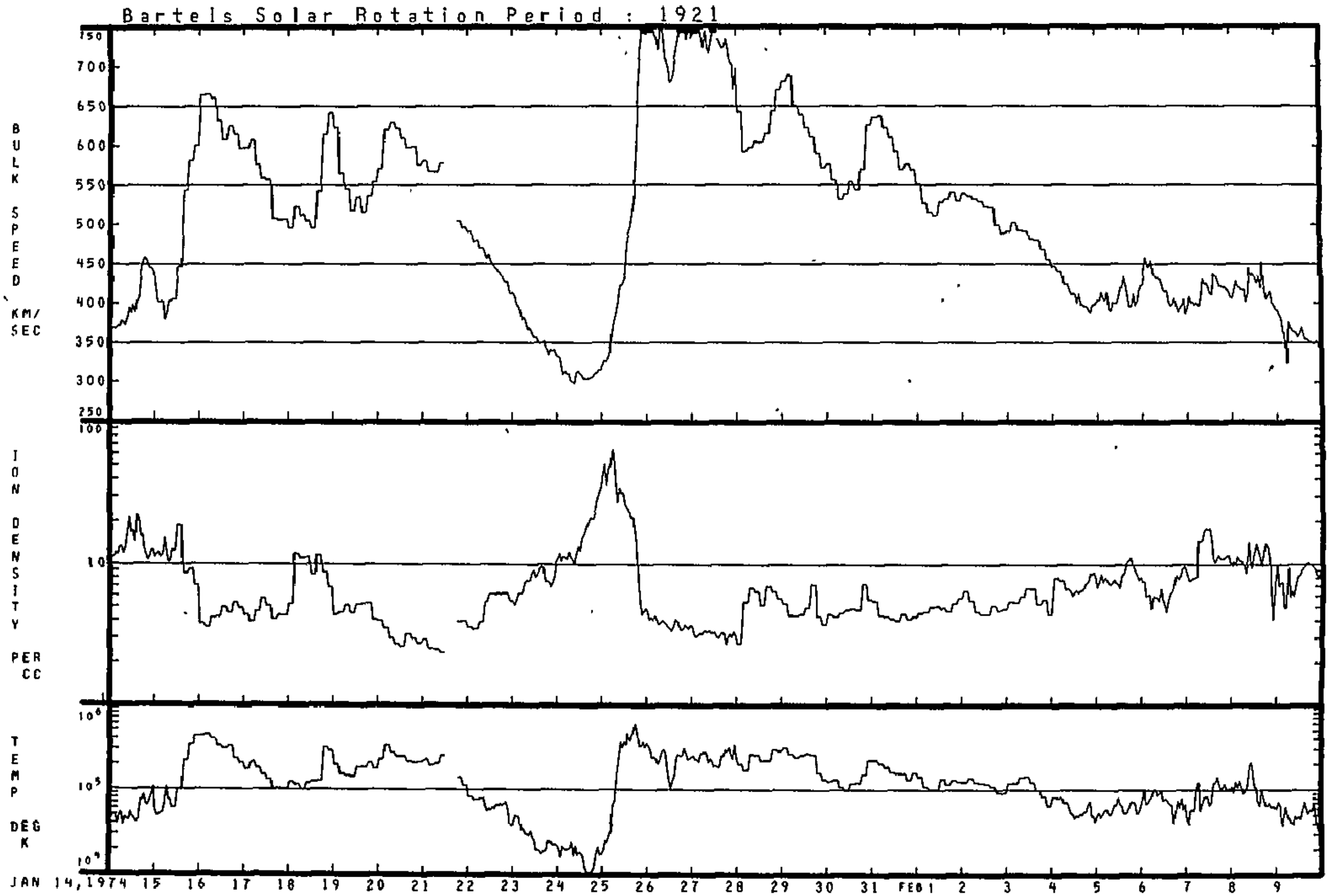
12/18/73 - 01/13/74



Bartels Solar Rotation Period : 1920

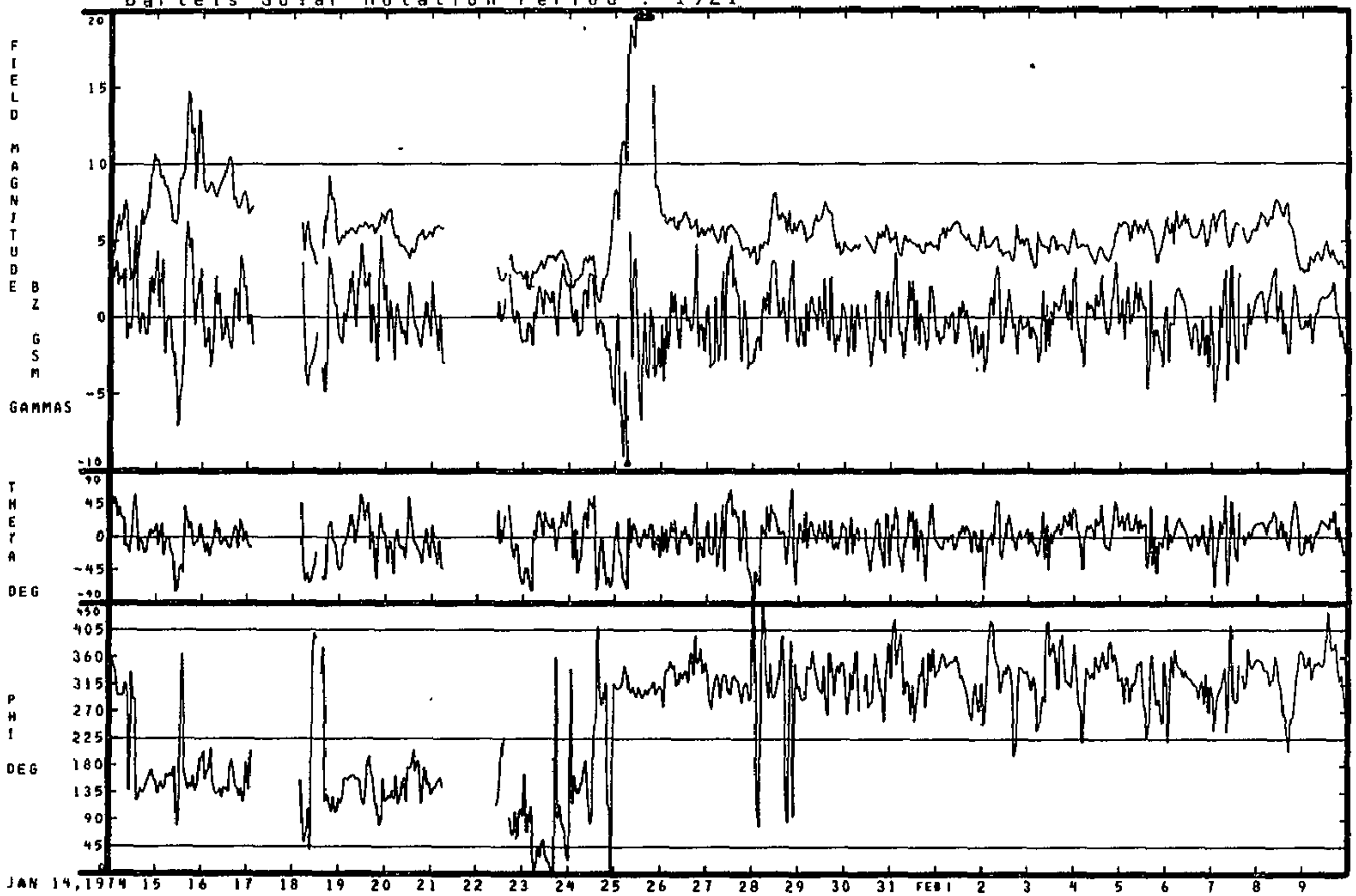


12/18/73 - 01/13/74



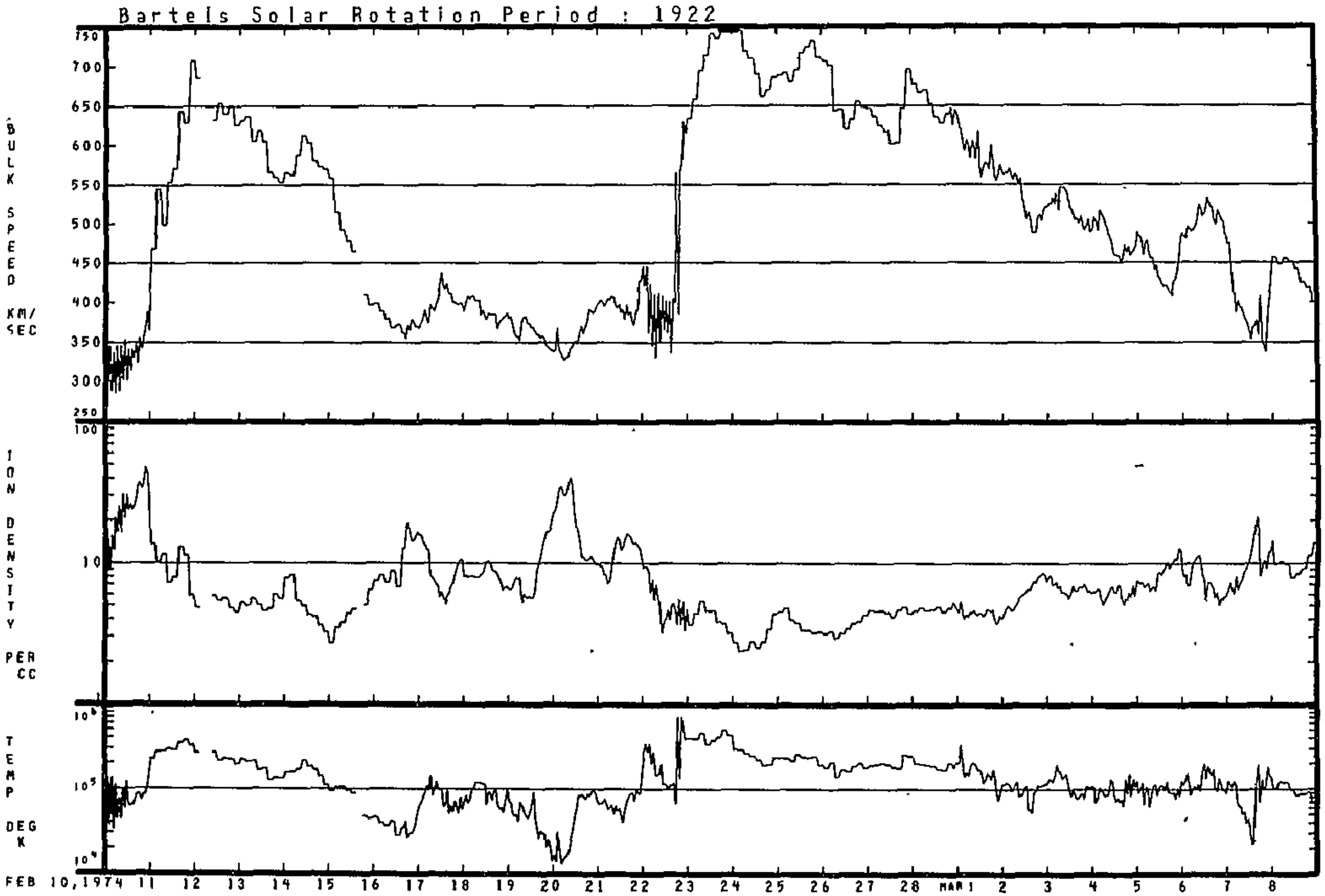
01/14/74 - 02/09/74

Bartels Solar Rotation Period : 1921

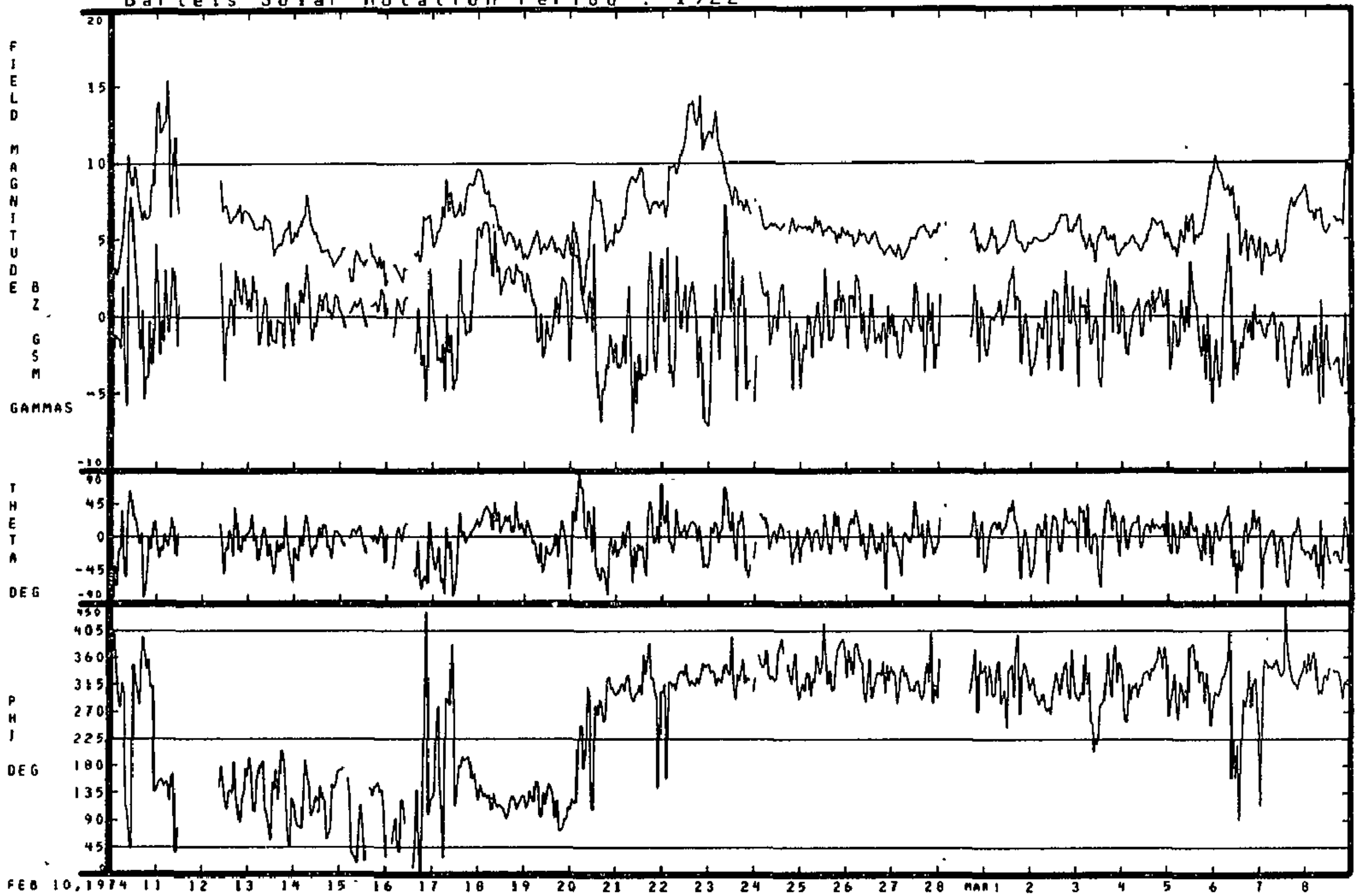


01/14/74 - 02/09/74

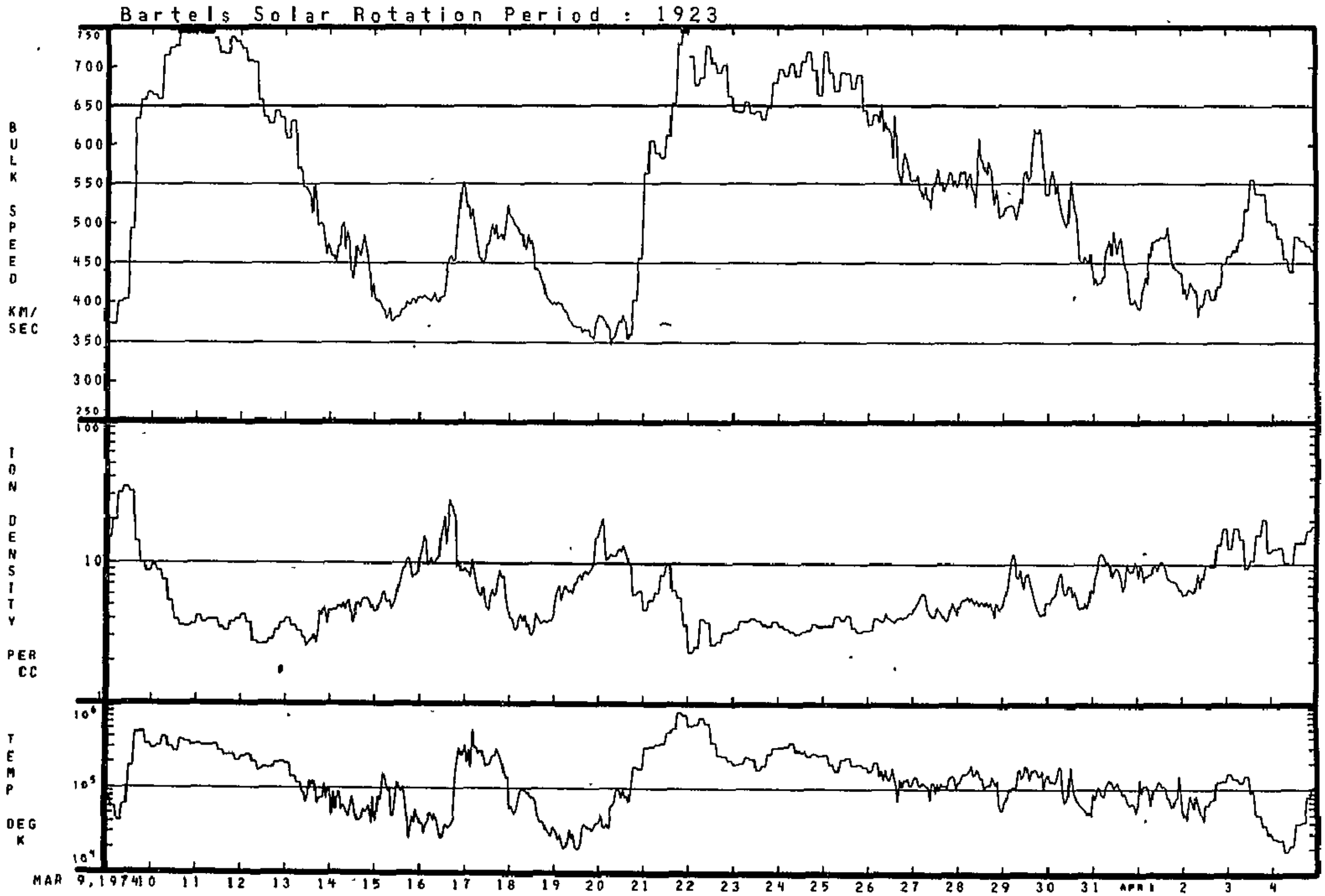
02/10/74 - 03/08/74



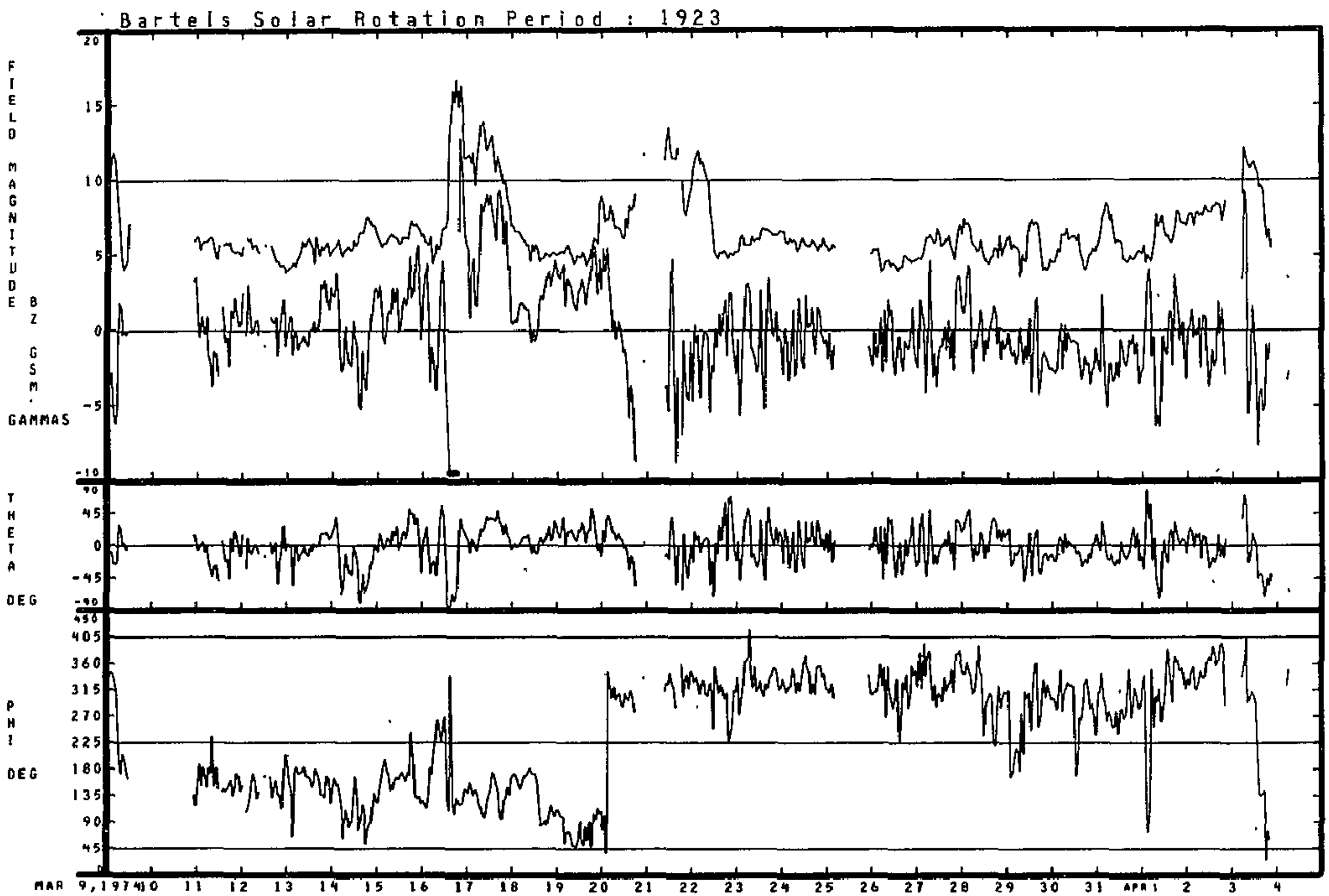
Bartels Solar Rotation Period : 1922



02/10/74 - 03/08/74



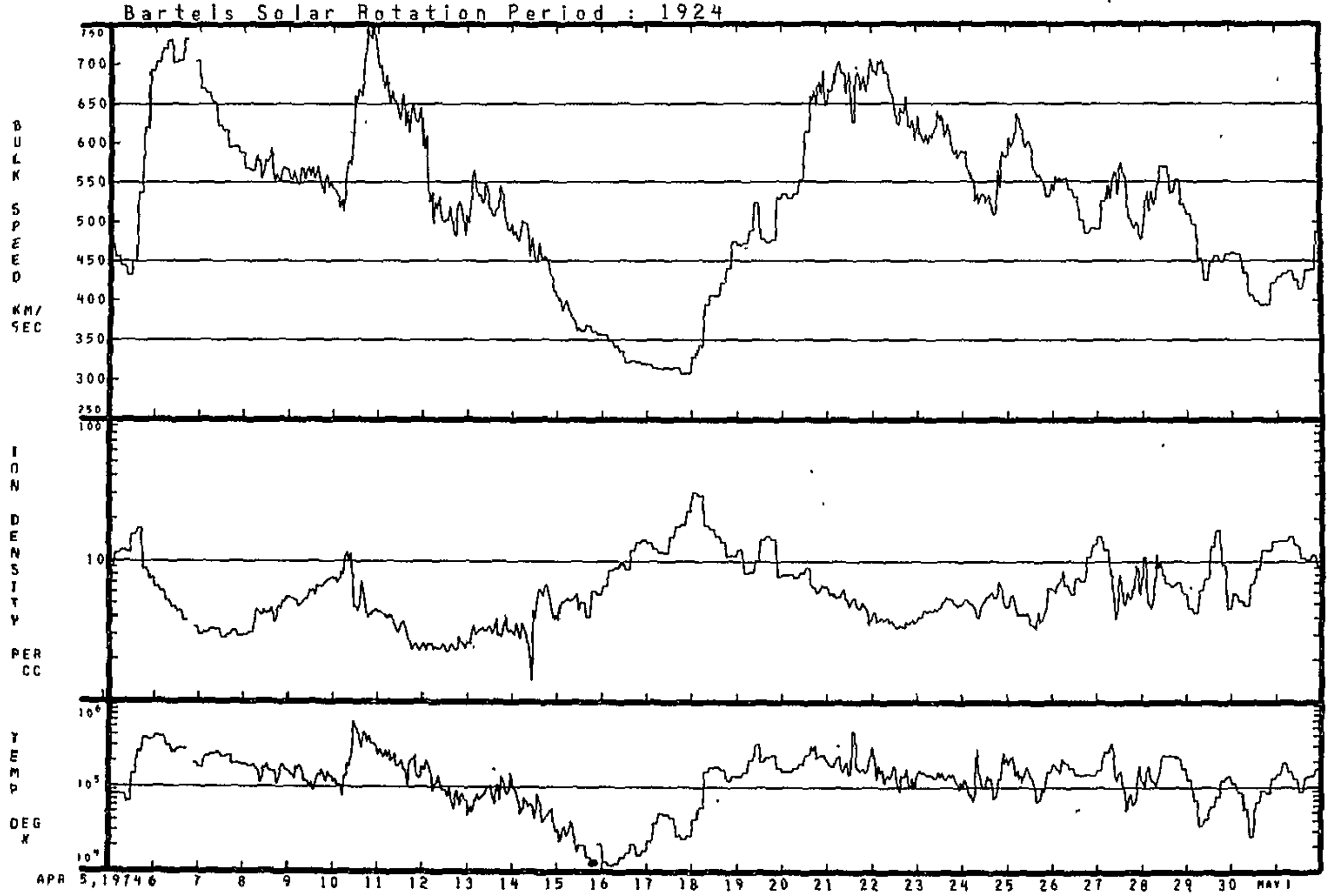
03/09/74 - 04/04/74



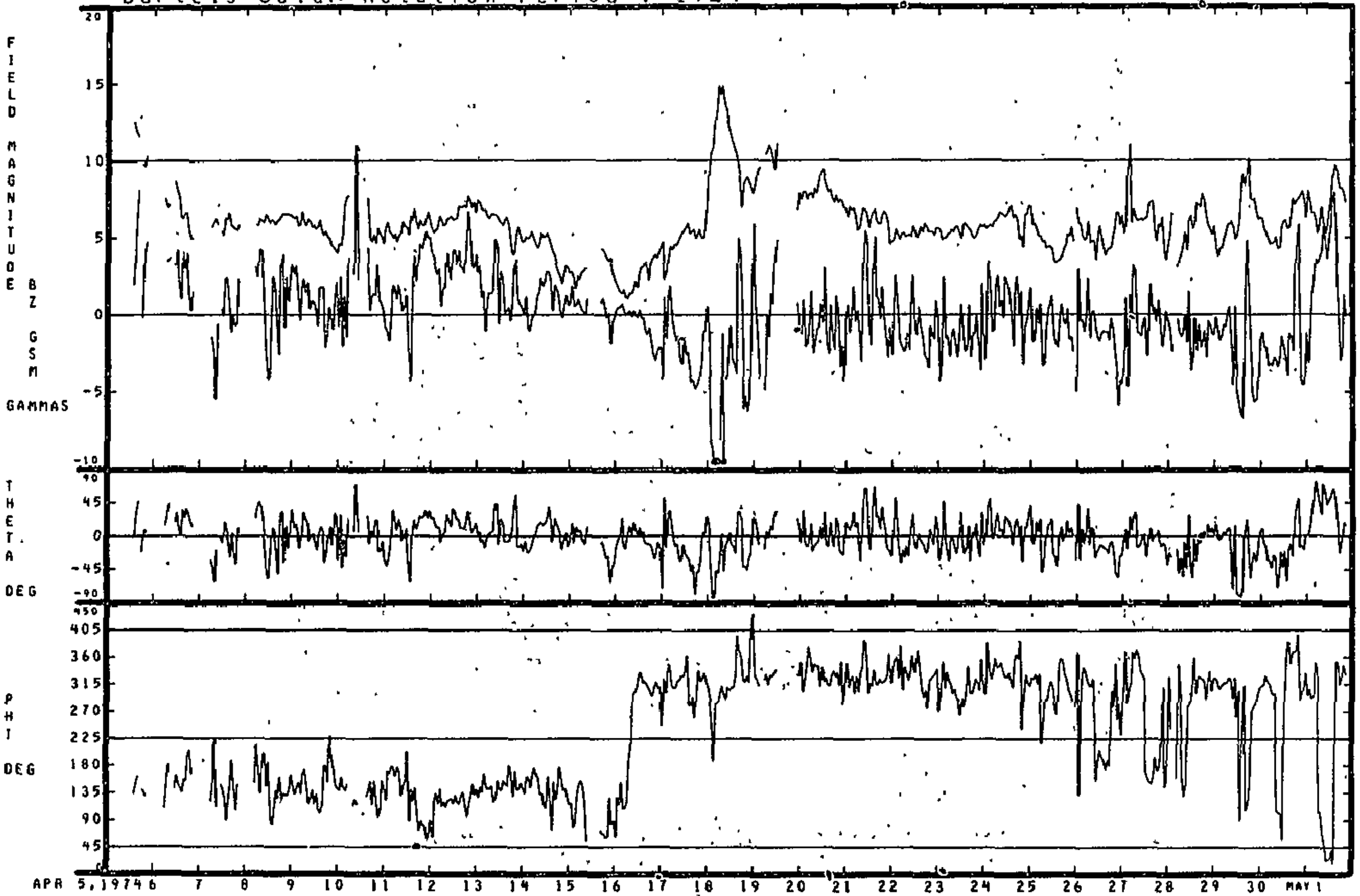
03/09/74 - 04/04/74

04/05/74 - 05/01/74

Bartels Solar Rotation Period : 1924

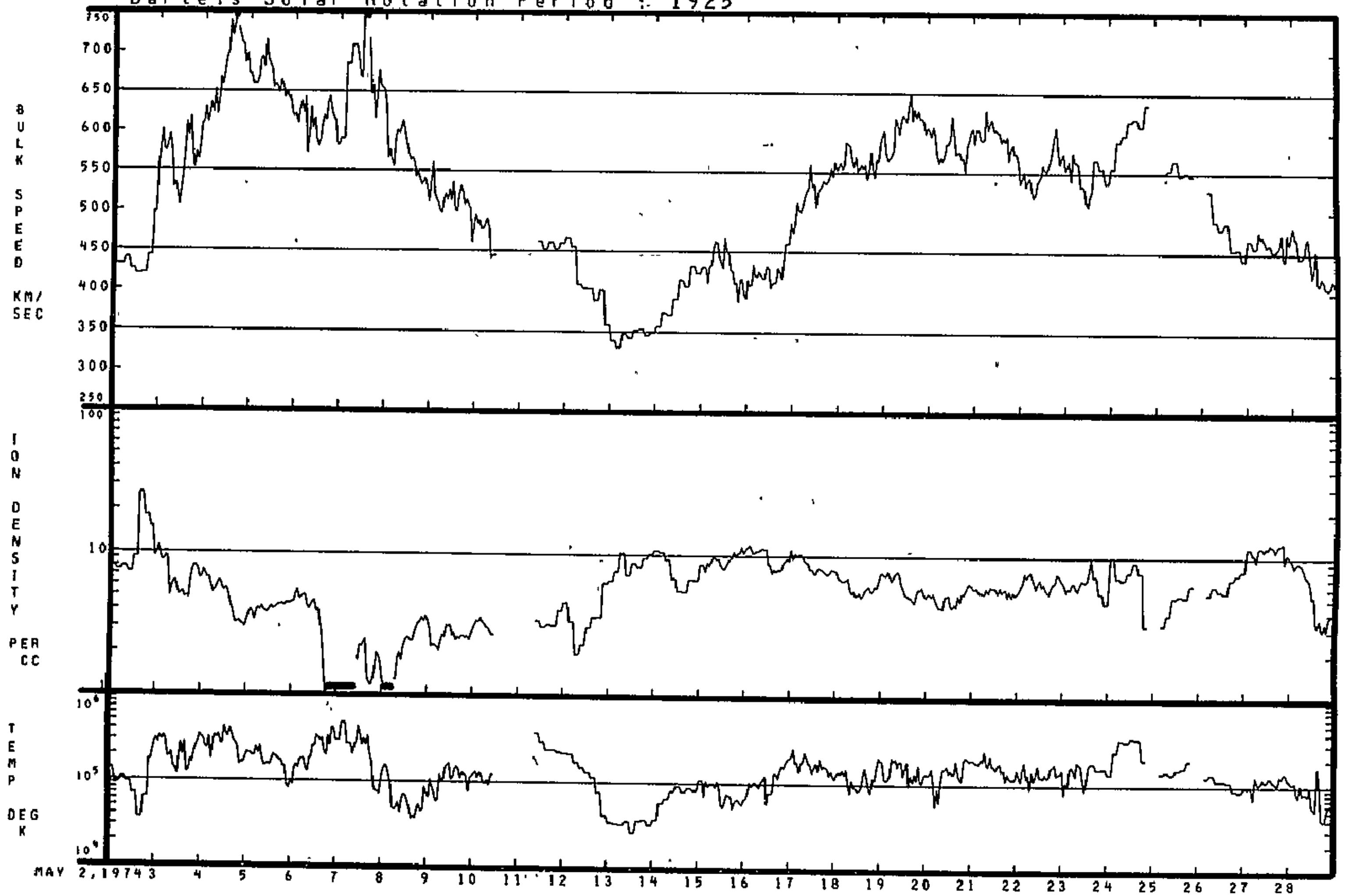


Bartels Solar Rotation Period : 1924

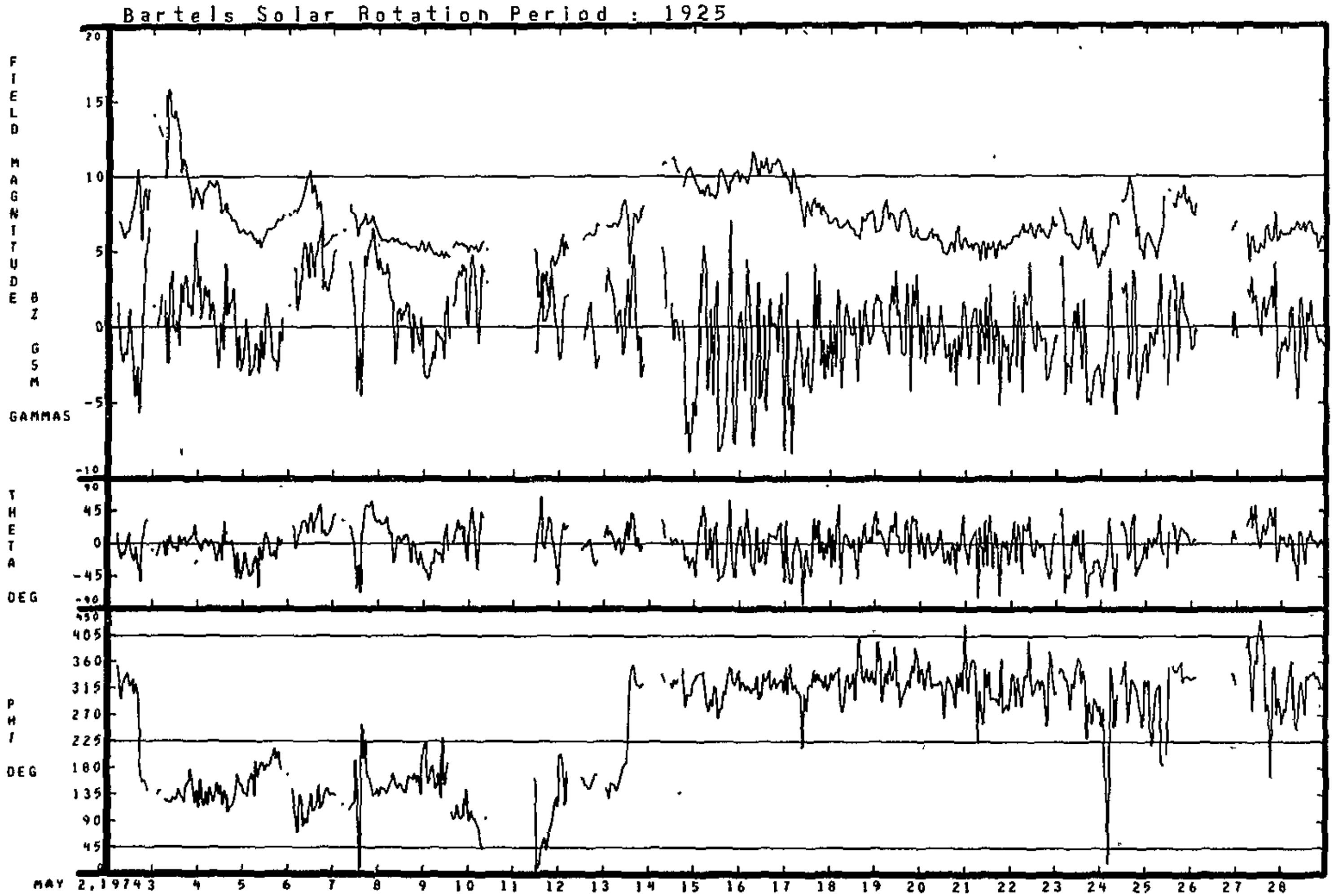


04/05/74 - 05/01/74

Bartels Solar Rotation Period : 1925

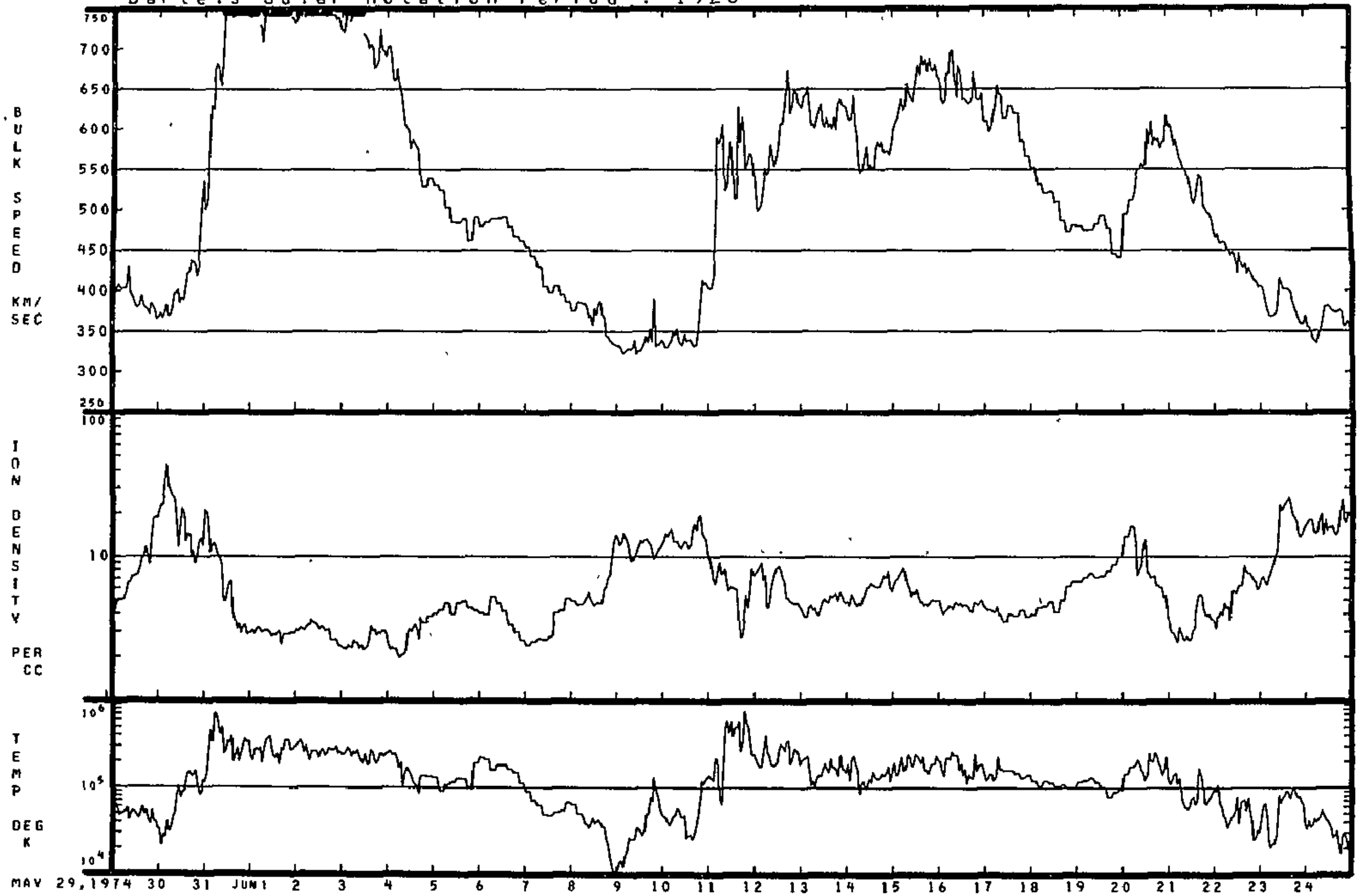


05/02/74 - 05/28/74

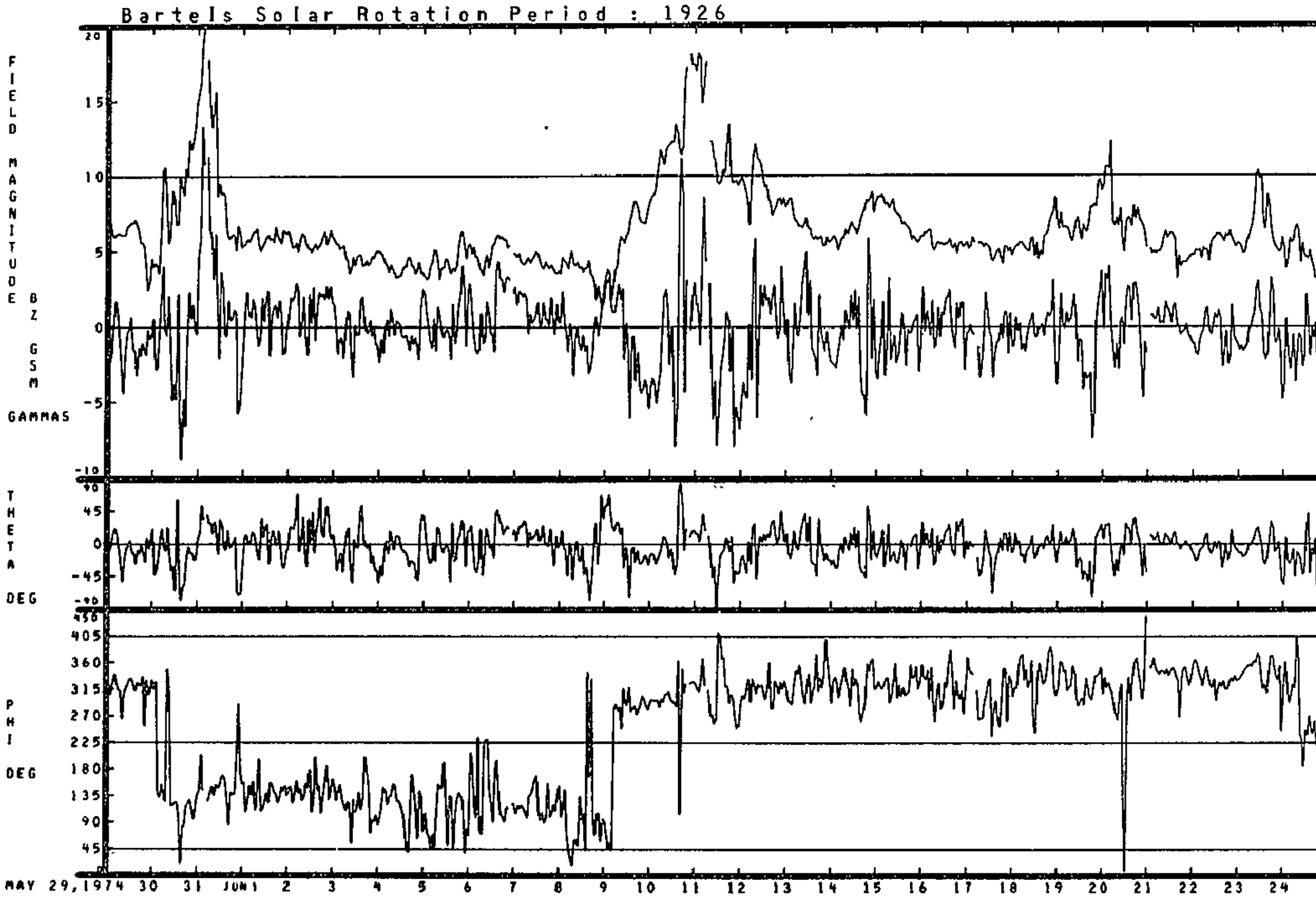


05/02/74 - 05/28/74

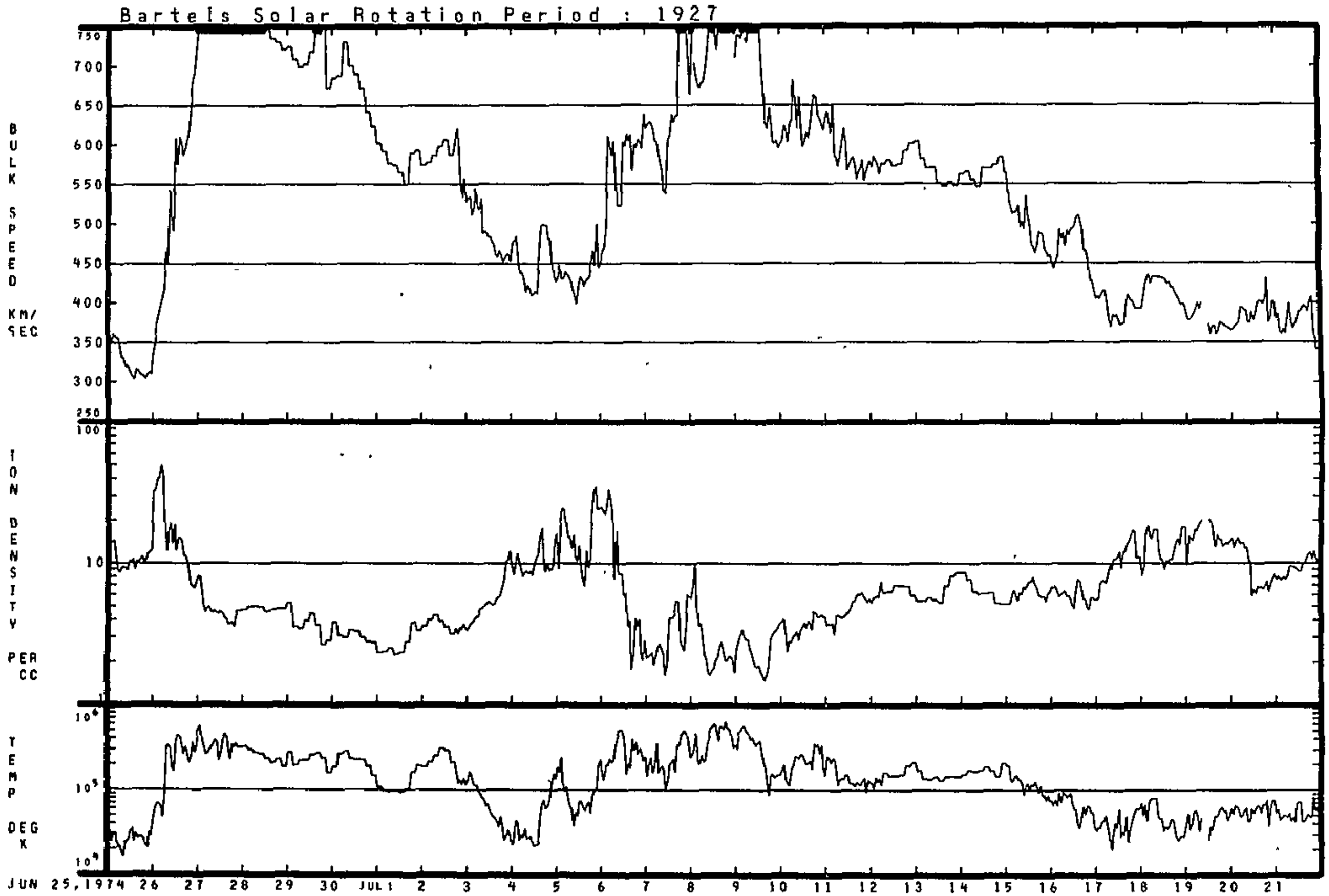
Bartels Solar Rotation Period : 1926



05/29/74 - 06/24/74

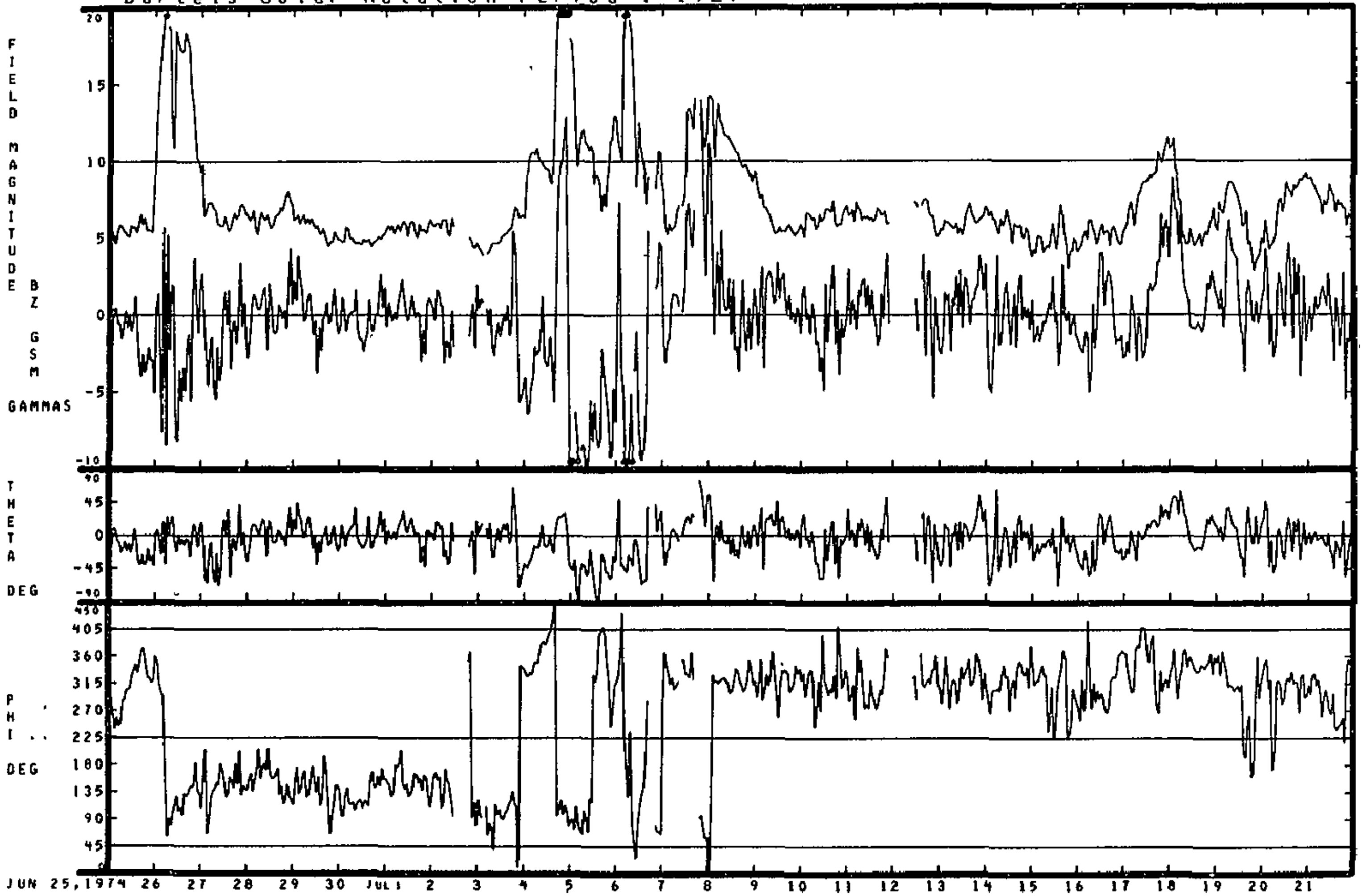


05/29/74 - 06/24/74



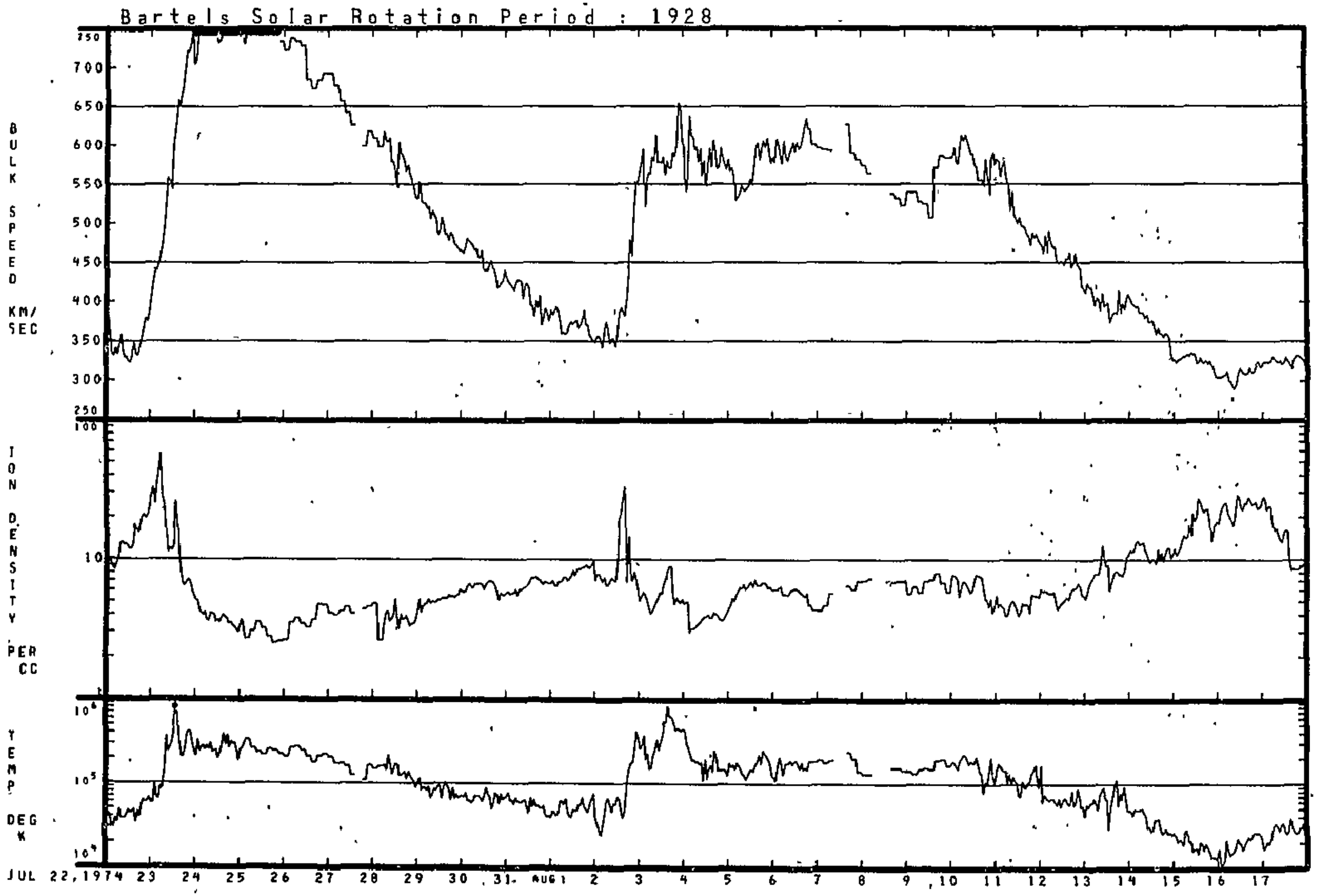
06/25/74 - 07/21/74

Bartels Solar Rotation Period : 1927

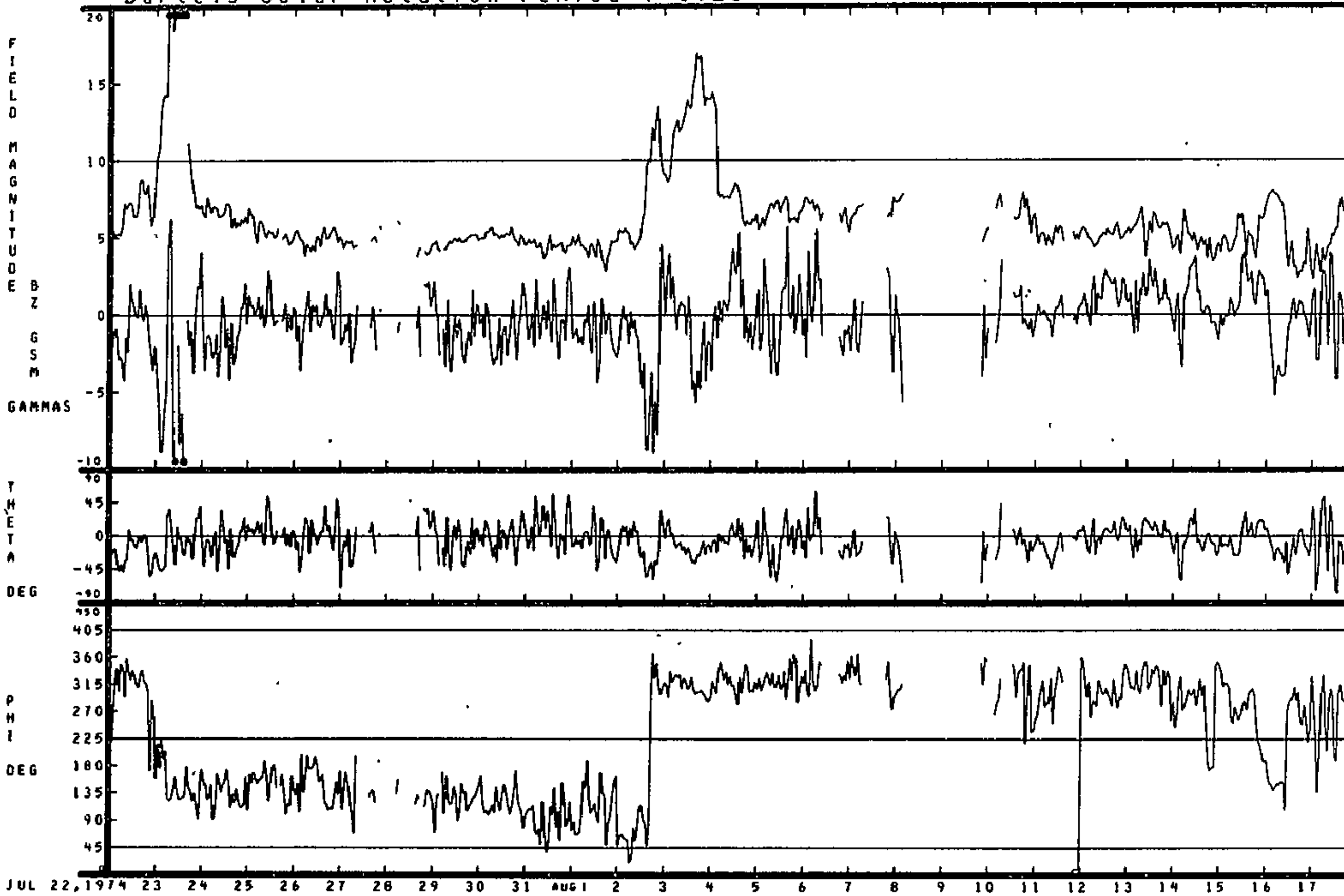


06/25/74 - 07/21/74

07/22/74 - 08/17/74

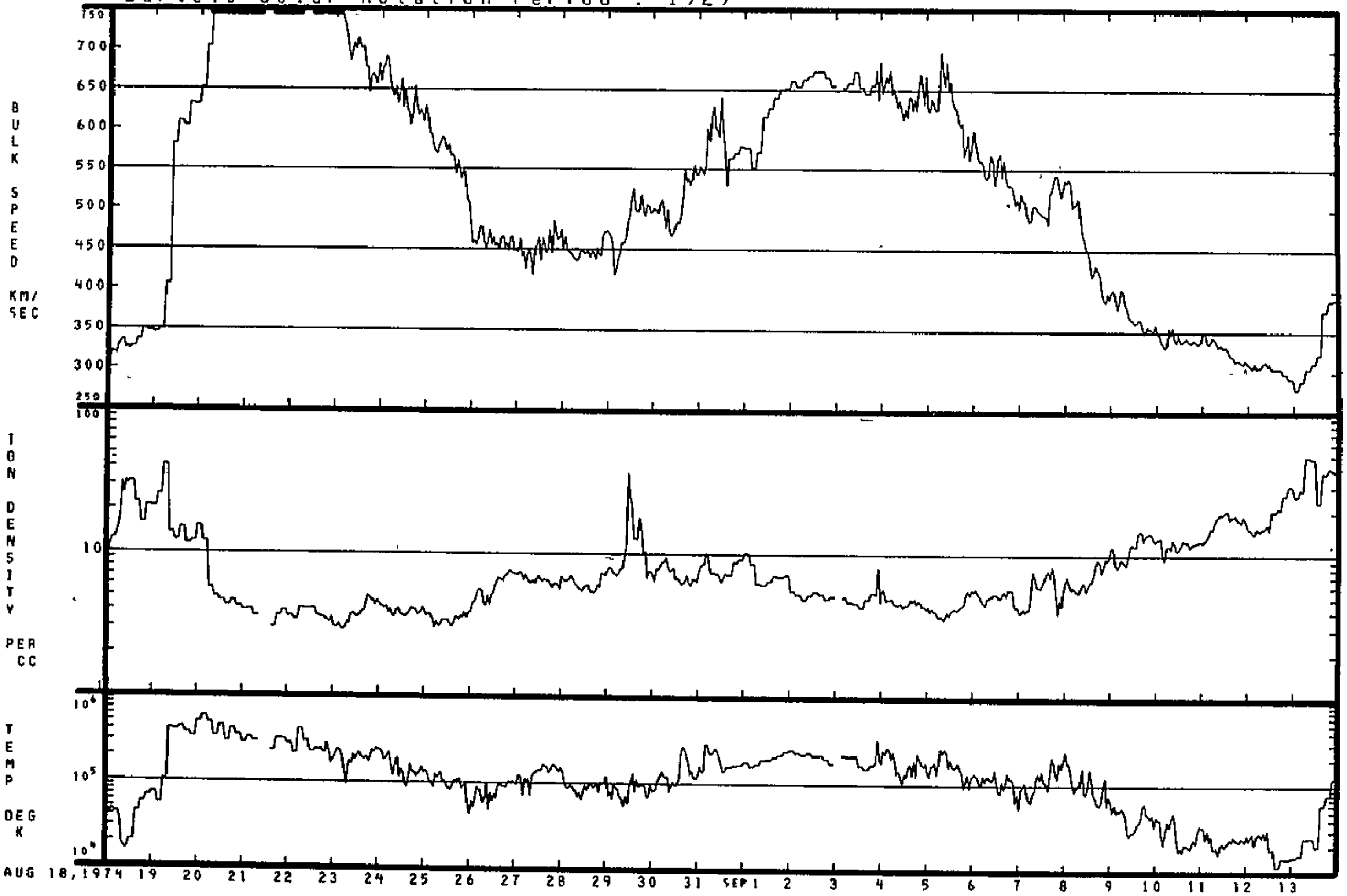


Bartels Solar Rotation Period : 1928

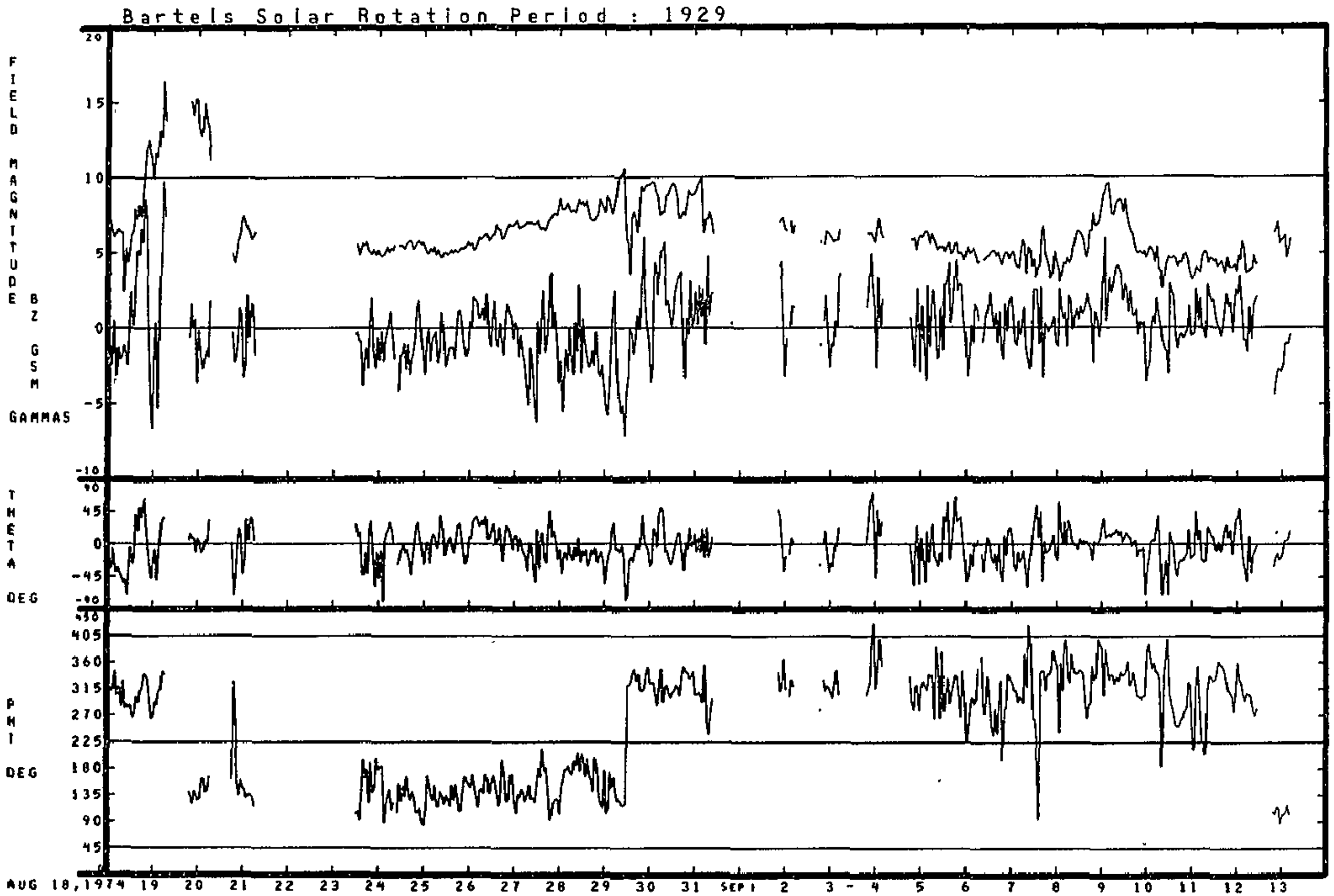


07/22/74 - 08/17/74

Bartels Solar Rotation Period : 1929



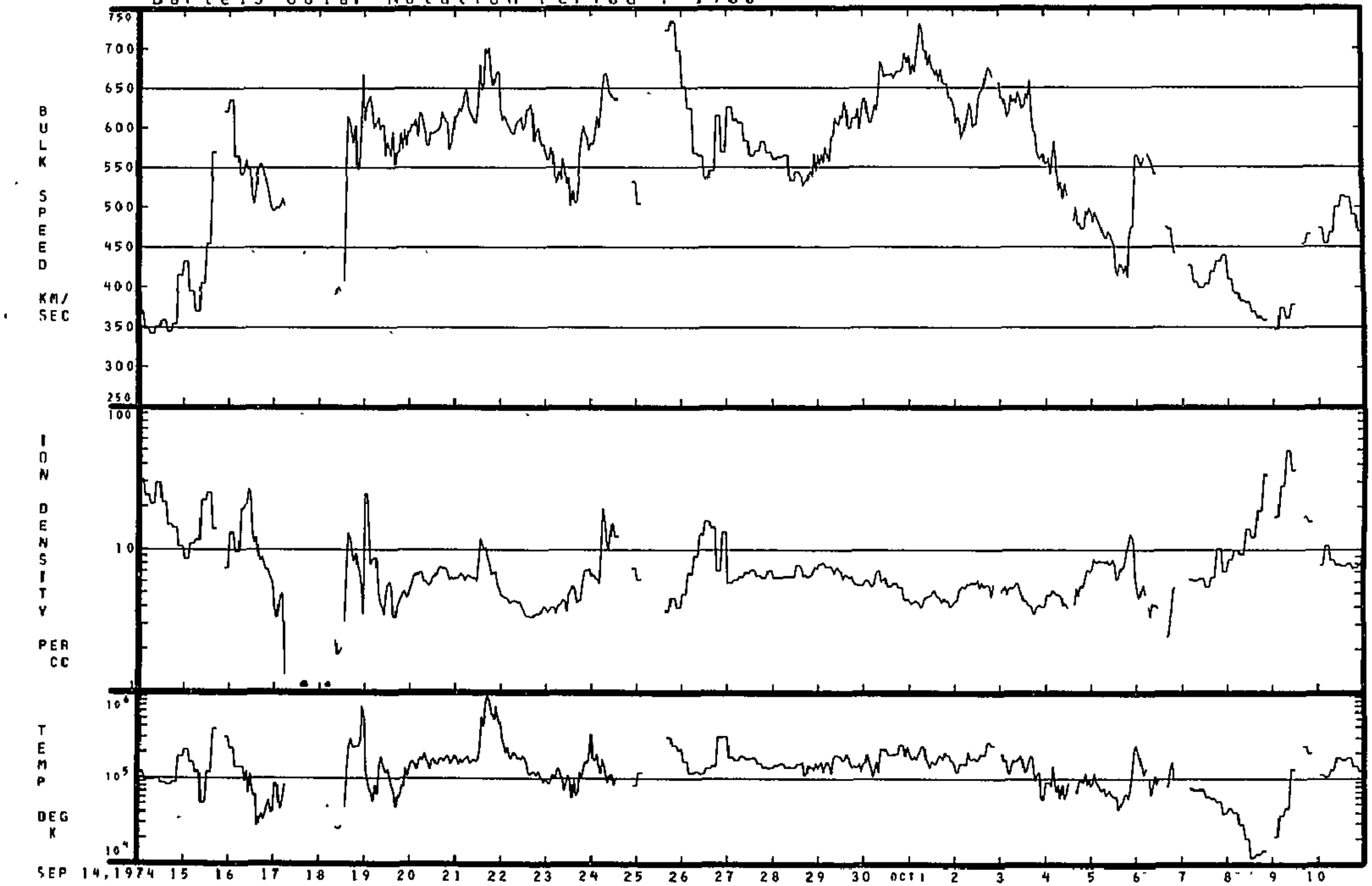
08/18/74 - 09/13/74



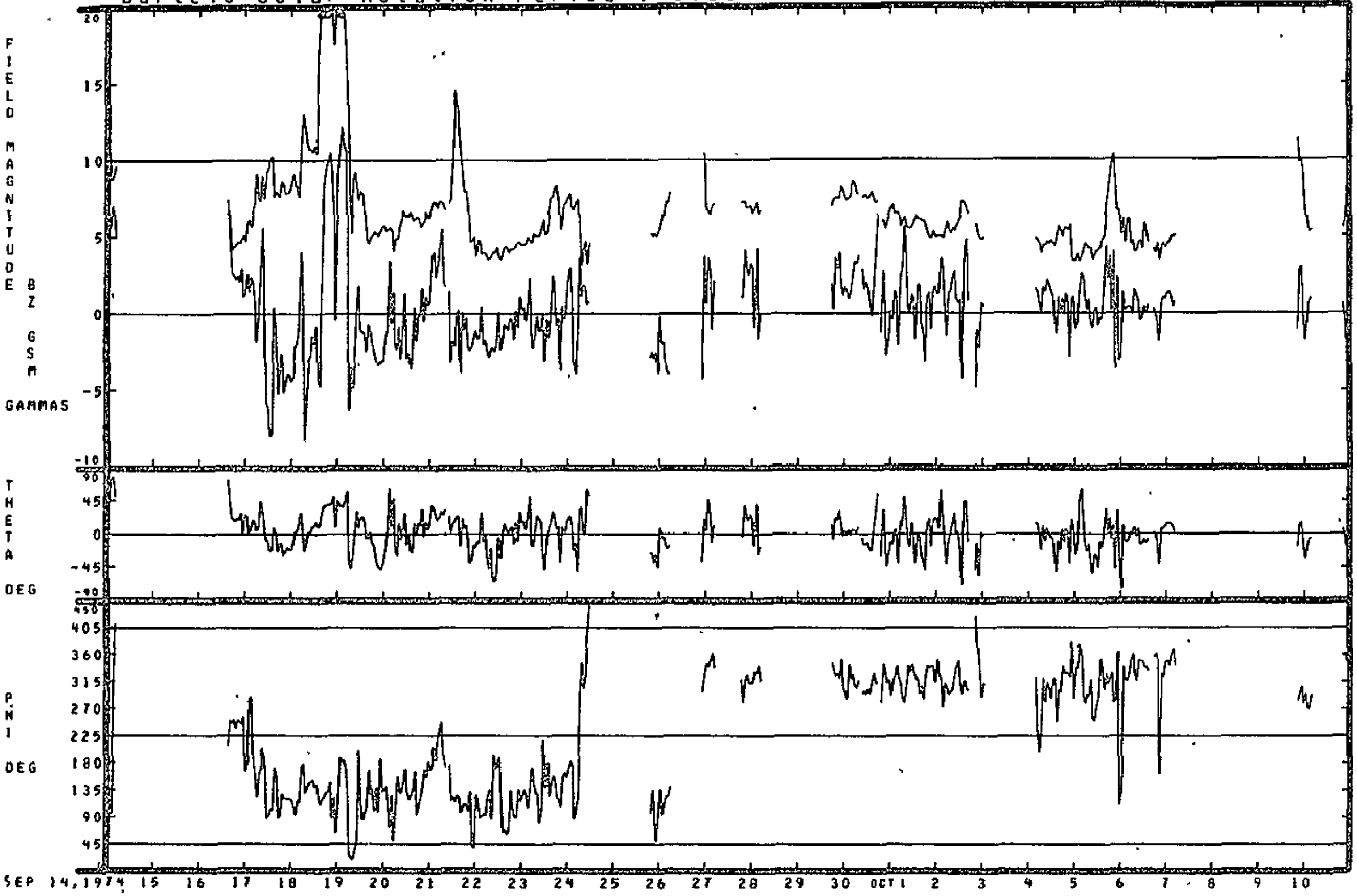
08/18/74 - 09/13/74

09/14/74 - 10/10/74

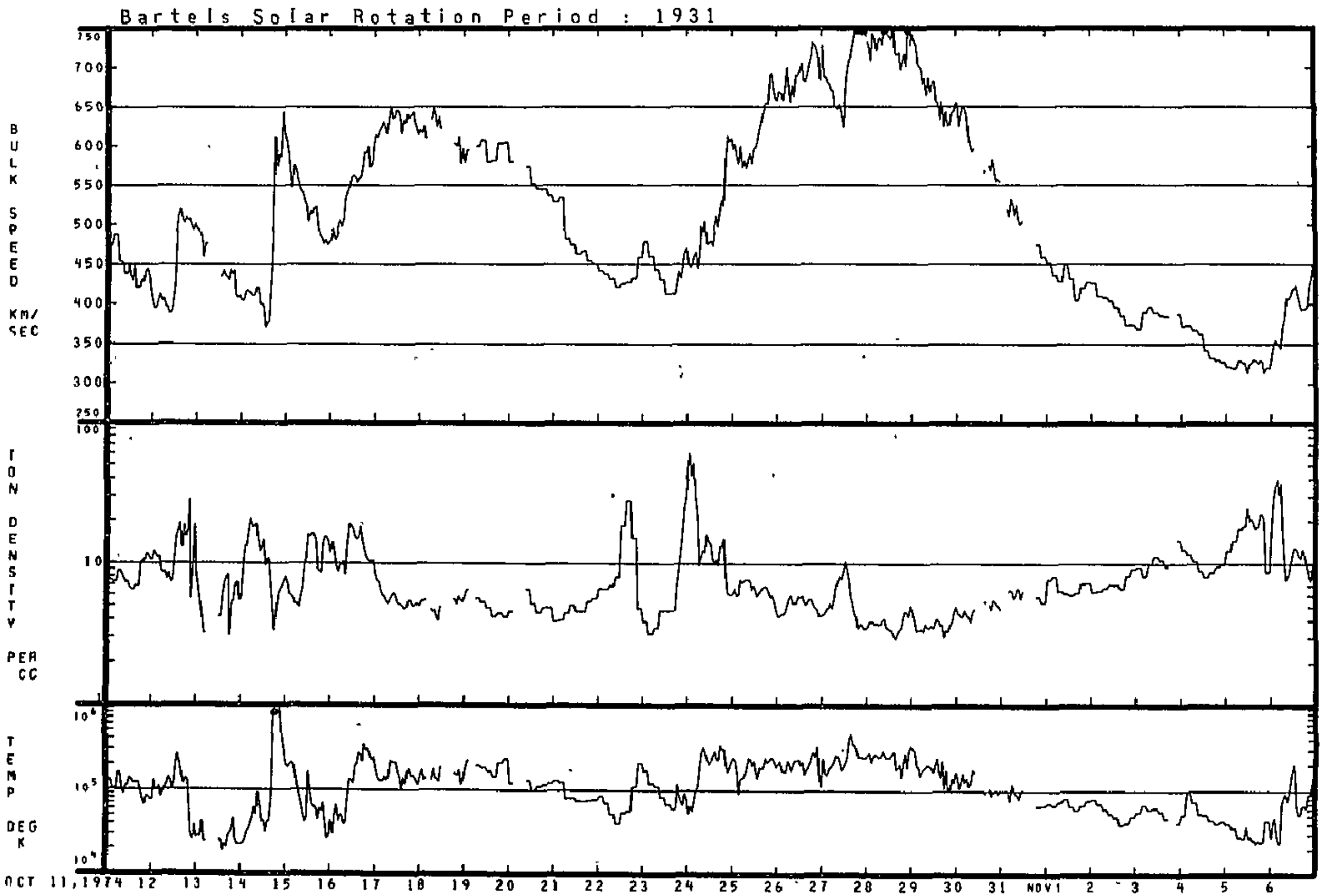
Bartels Solar Rotation Period : 1930



Bartels Solar Rotation Period : 1930

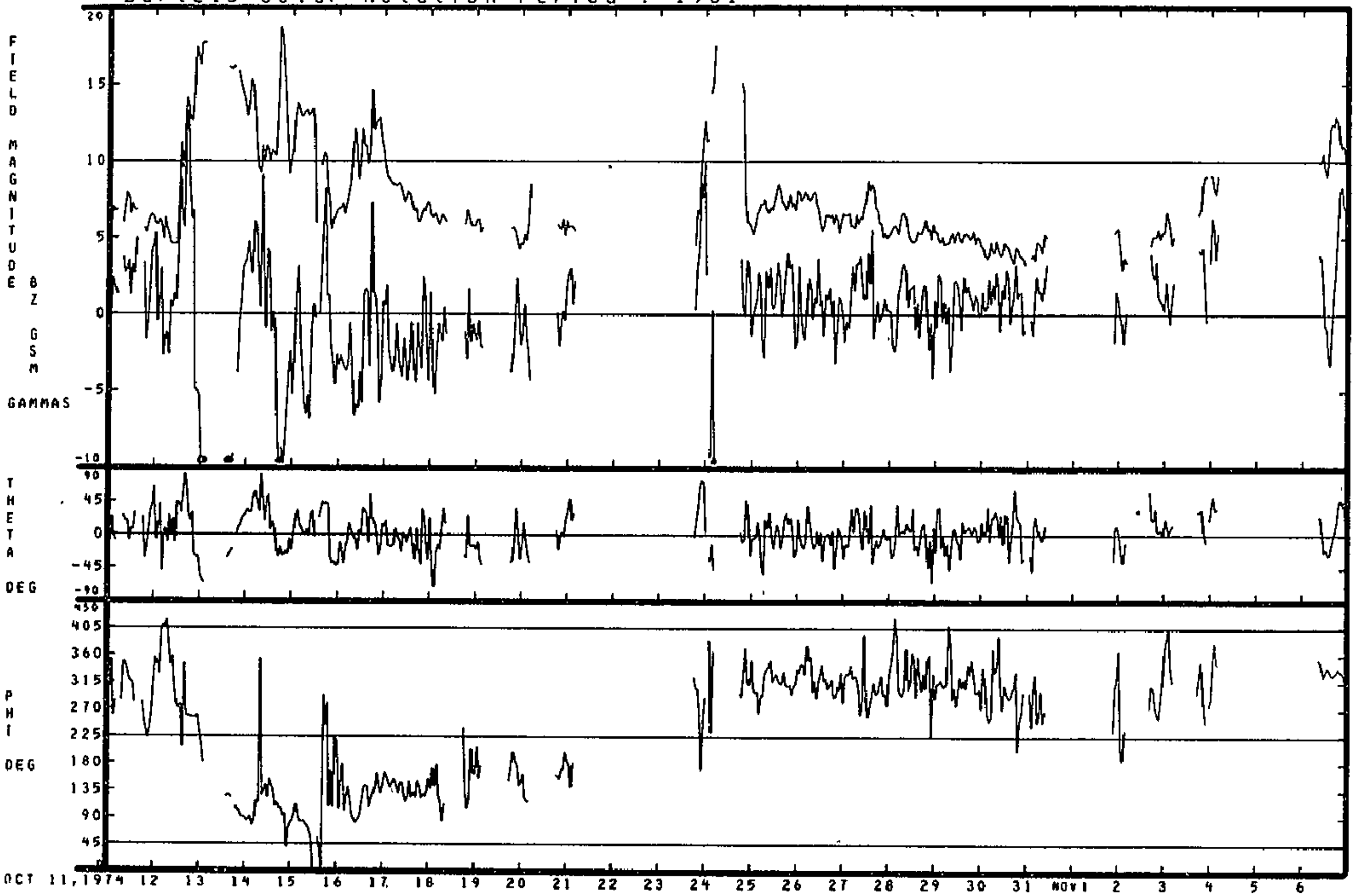


09/14/74 - 10/10/74



10/11/74 - 11/06/74

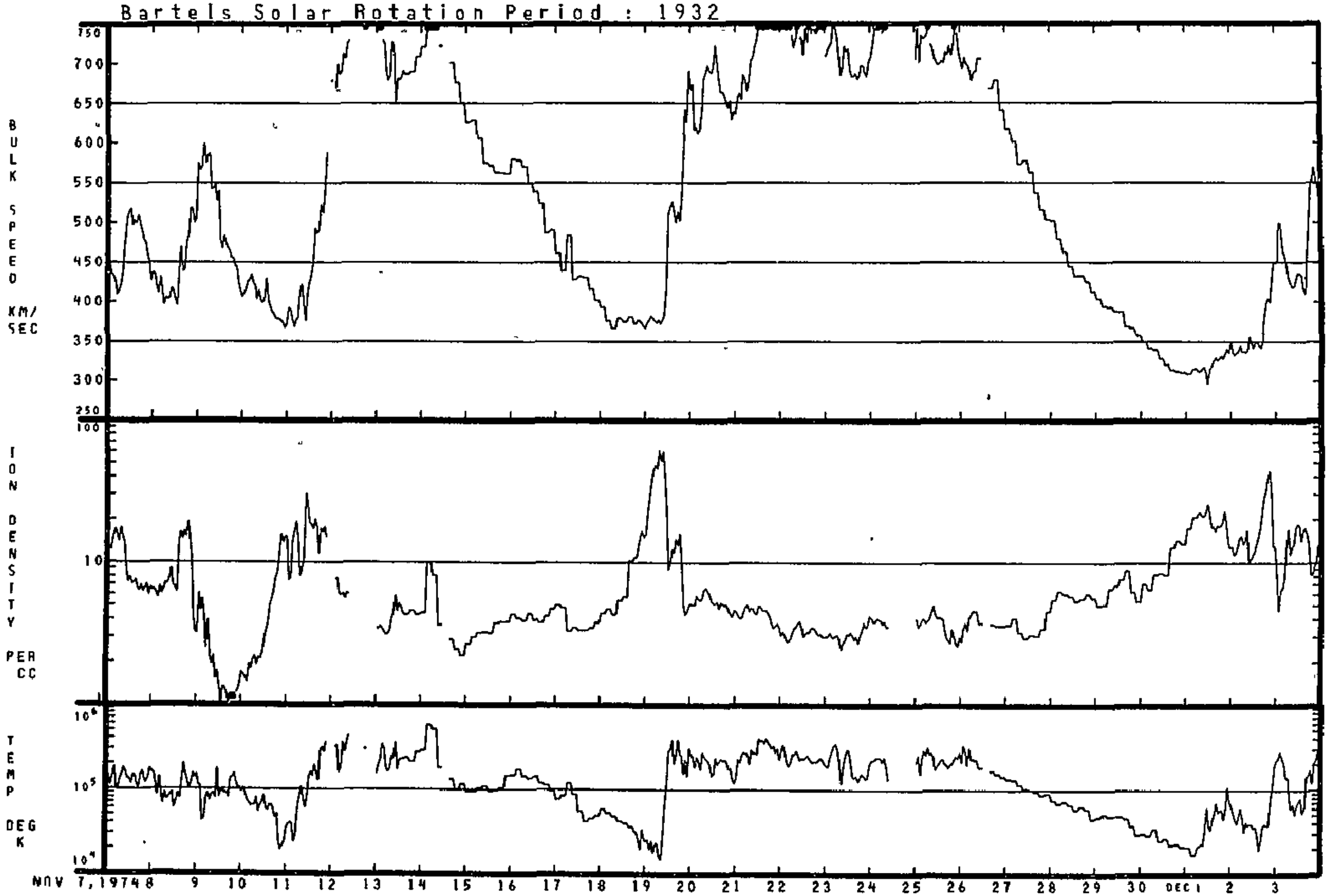
Bartels Solar Rotation Period : 1931



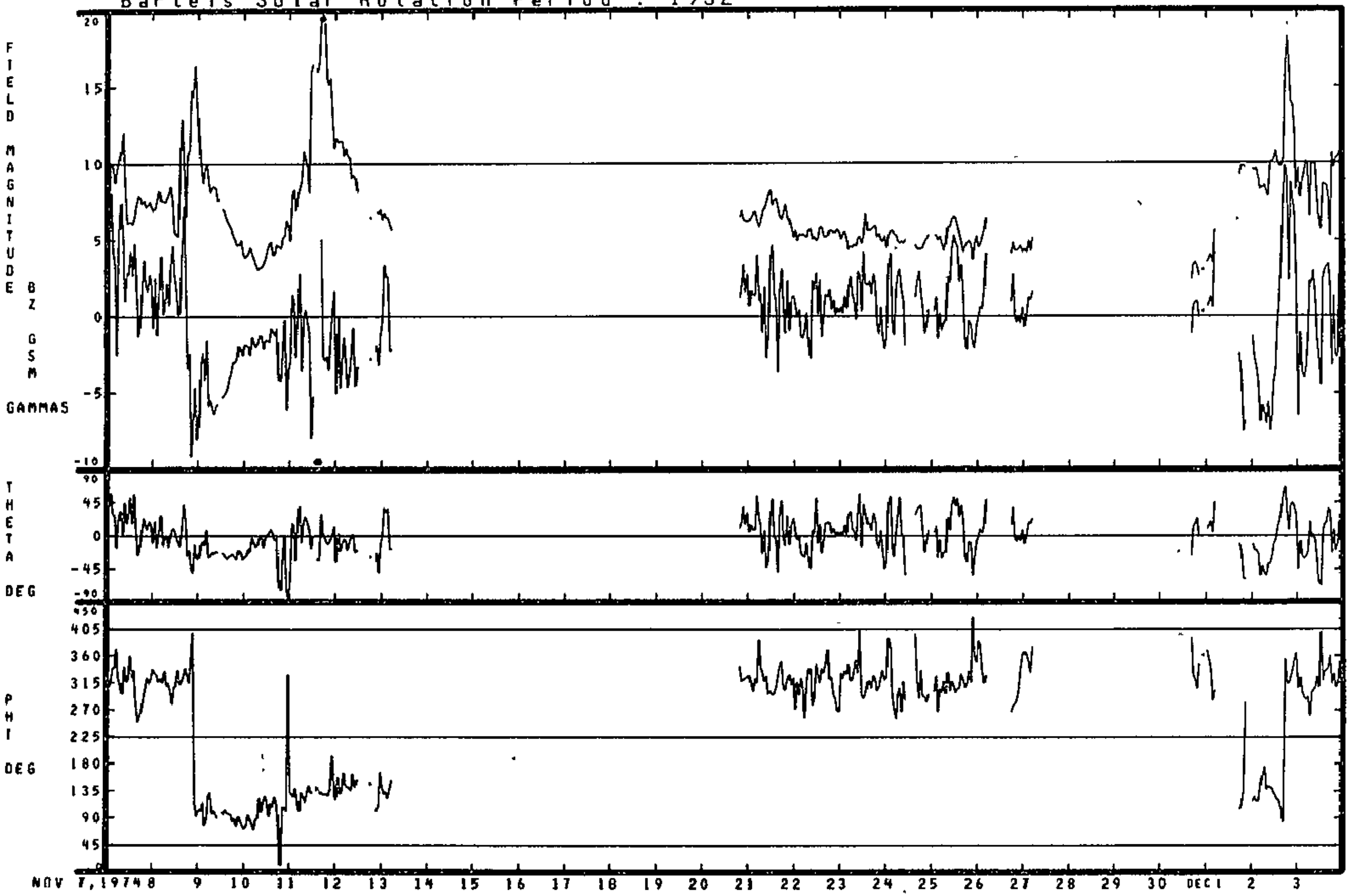
10/11/74 - 11/06/74

11/07/74 - 12/03/74

Bartels Solar Rotation Period : 1932



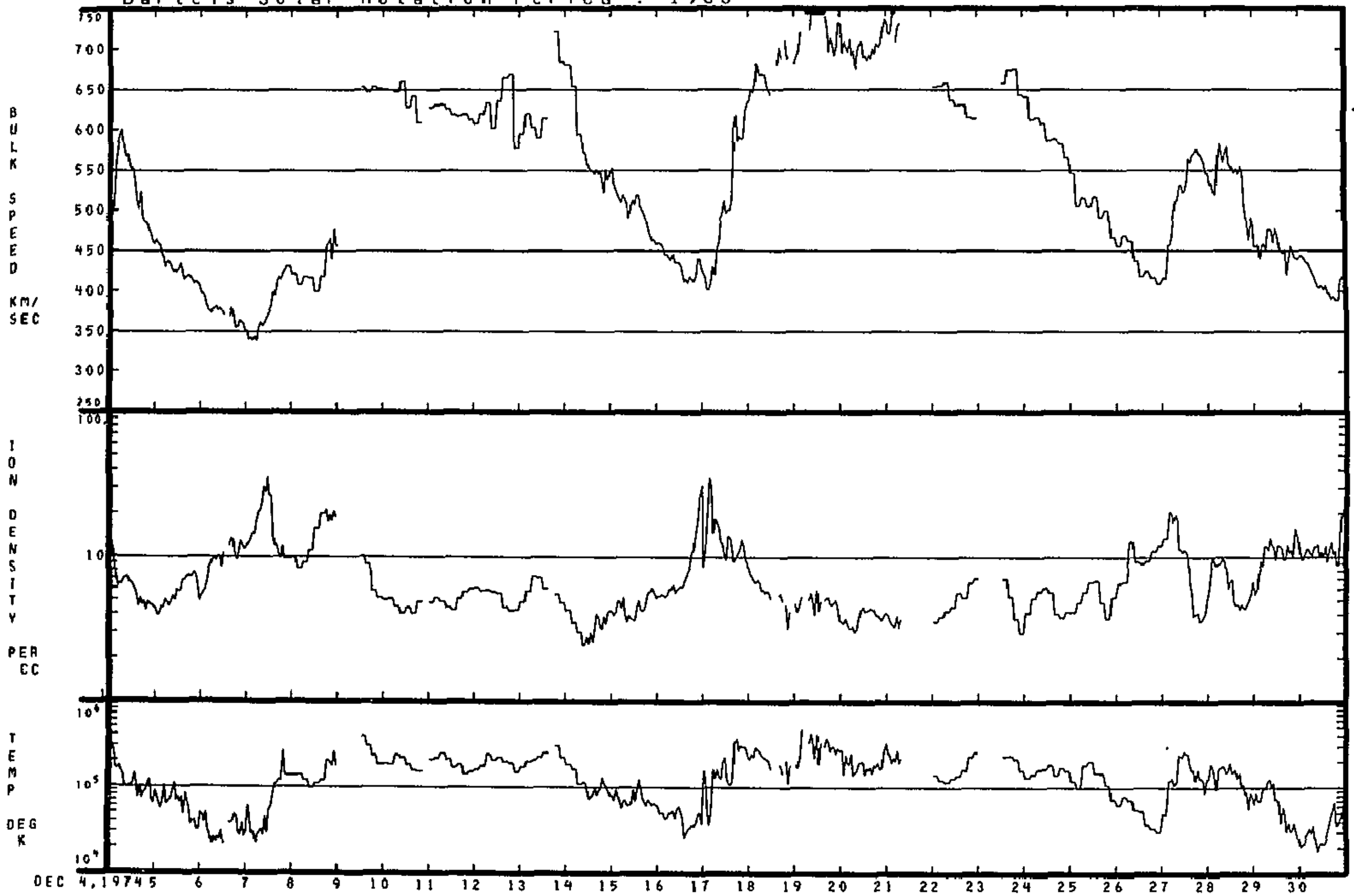
Bartels Solar Rotation Period : 1932

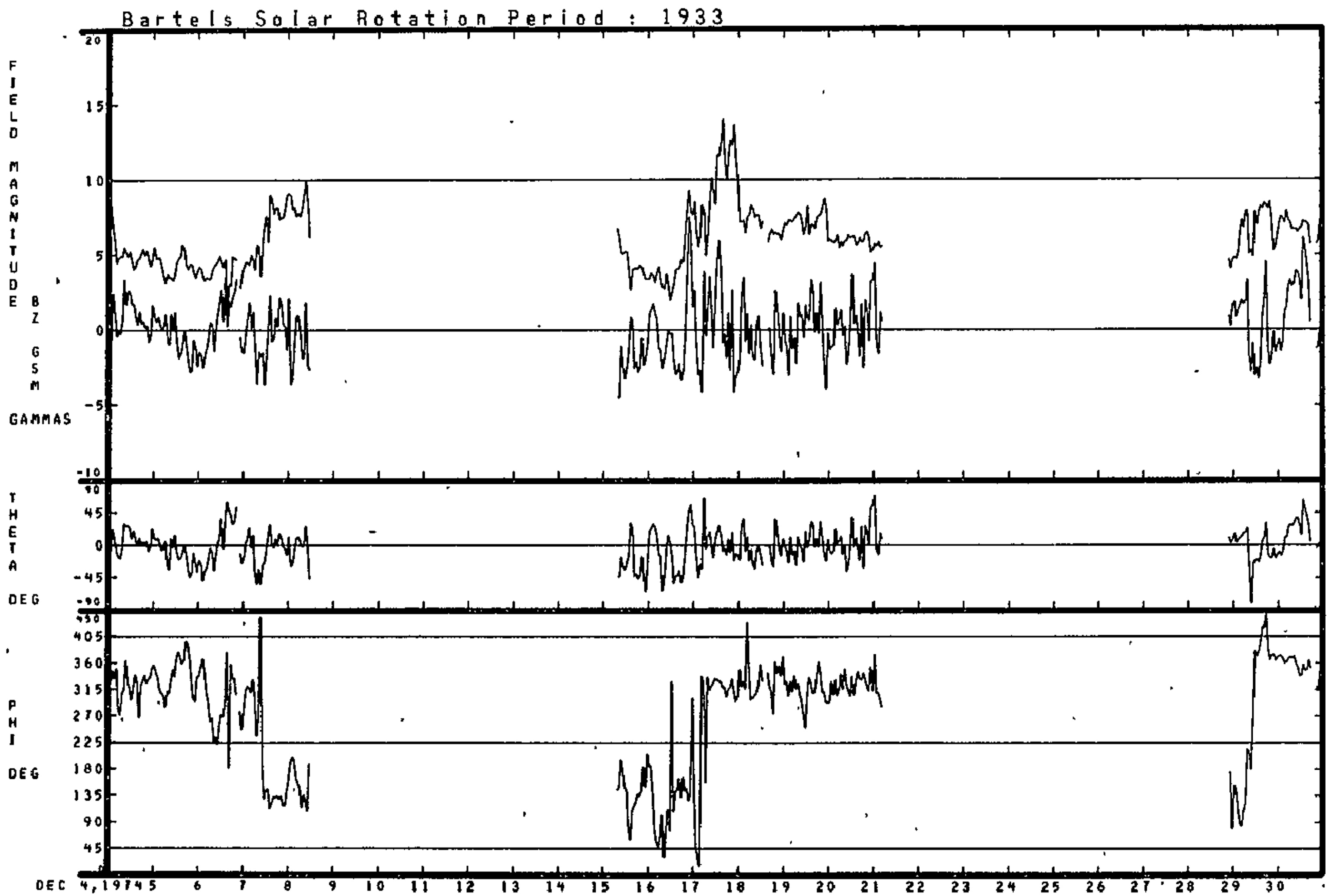


11/07/74 - 12/03/74

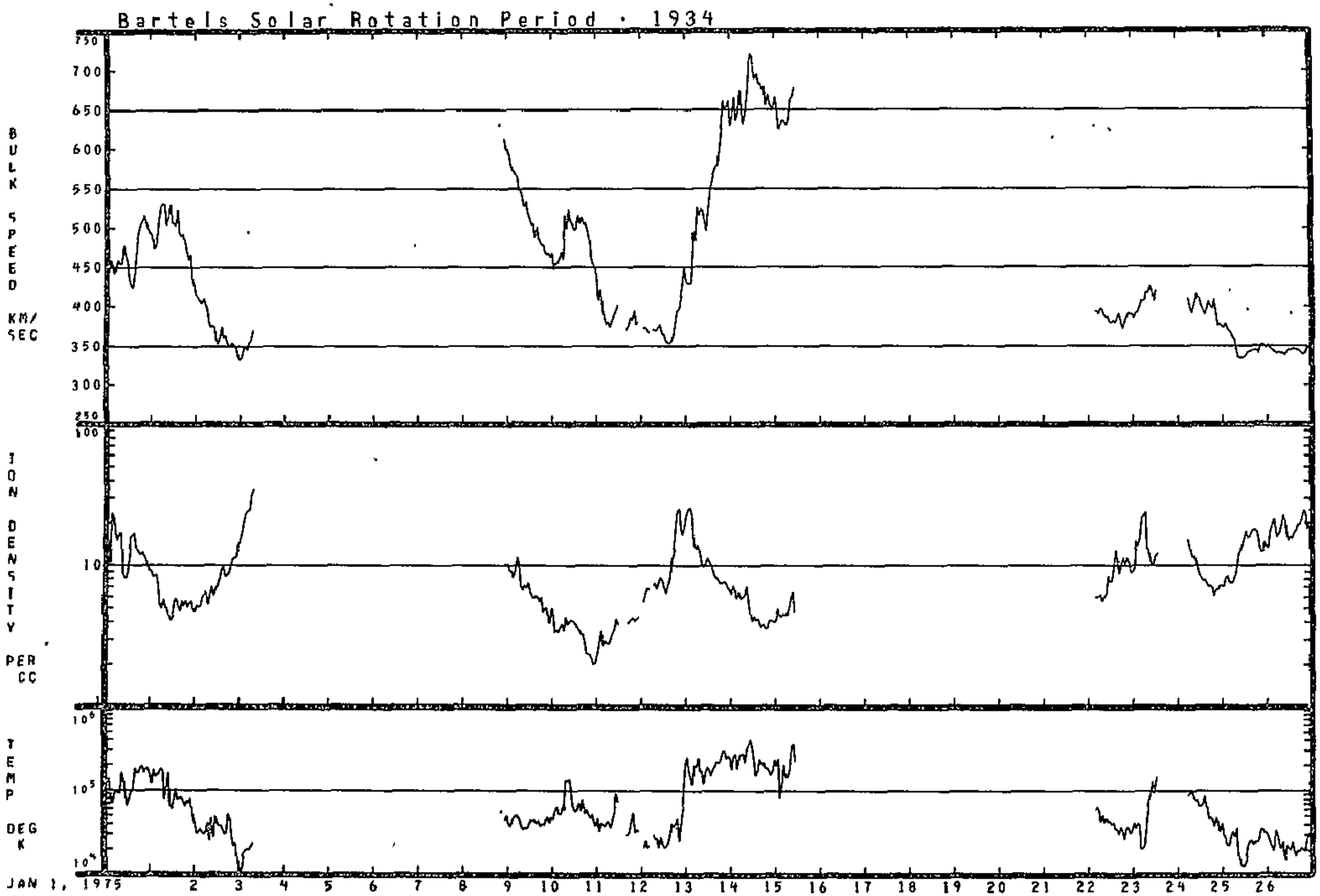
Bartels Solar Rotation Period : 1933

12/04/74 - 12/30/74



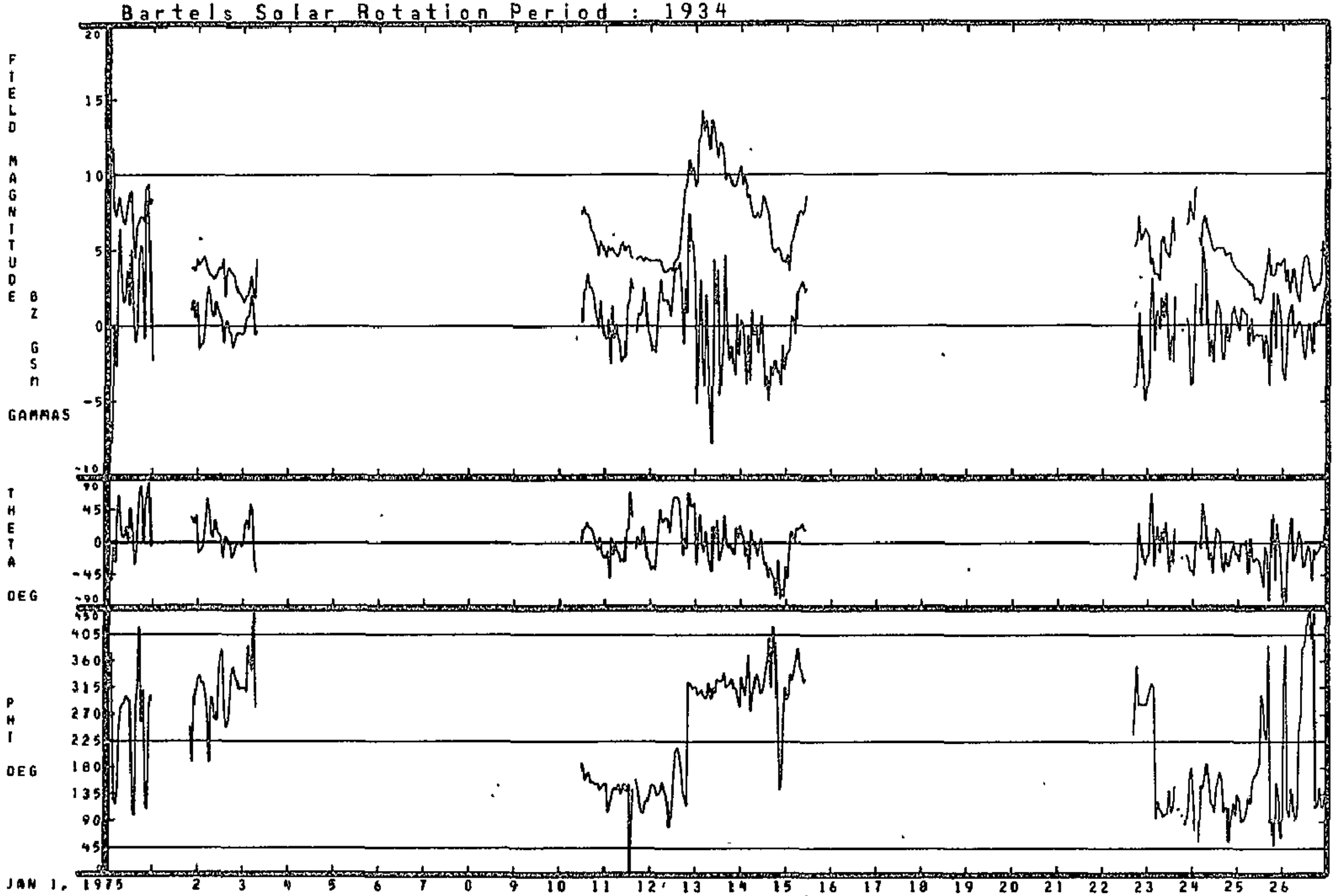


12/04/74 - 12/30/74



01/01/75 - 01/26/75

Bartels Solar Rotation Period : 1934



01/01/75 - 01/26/75

01/27/75 - 02/22/75

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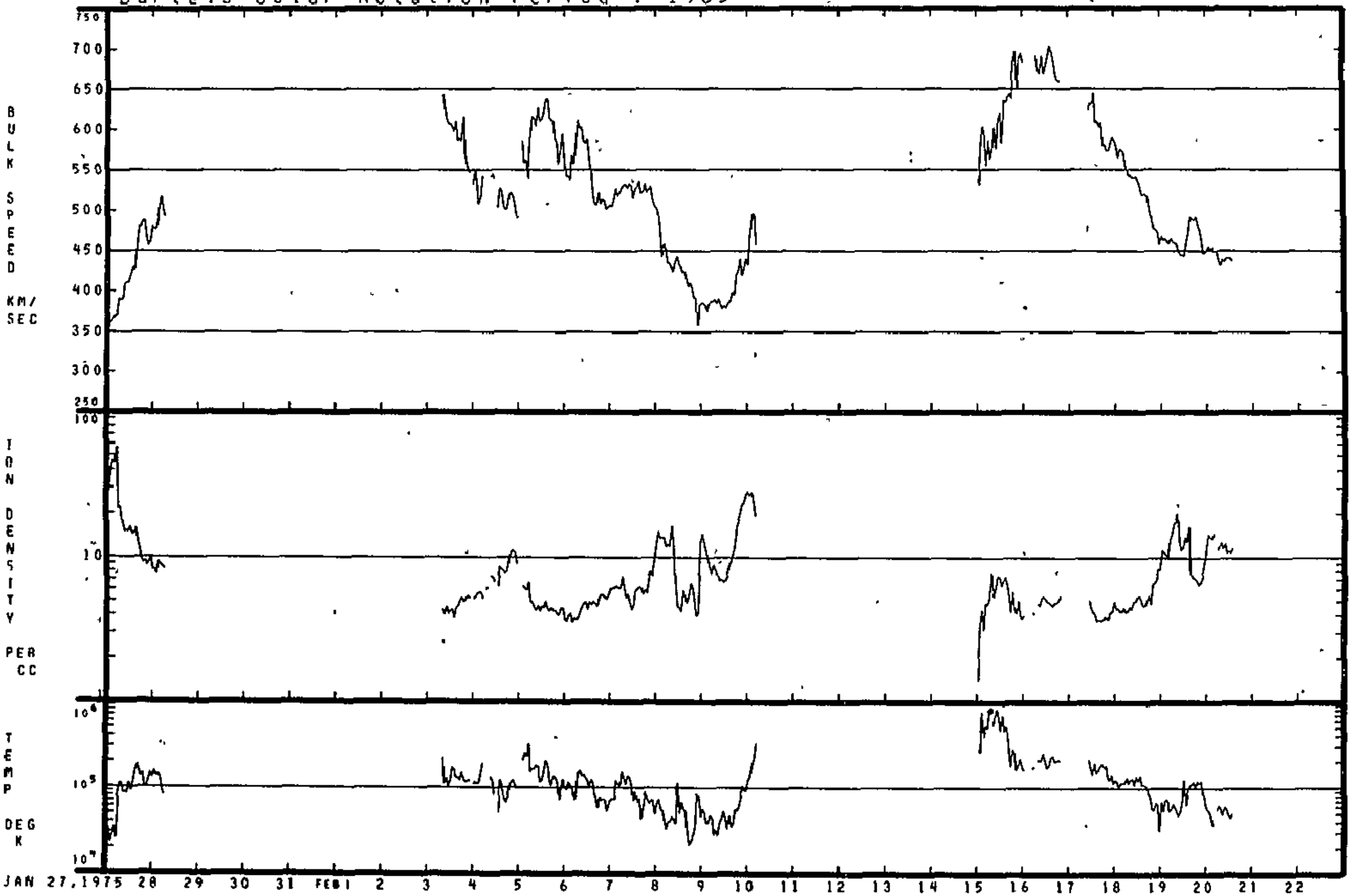
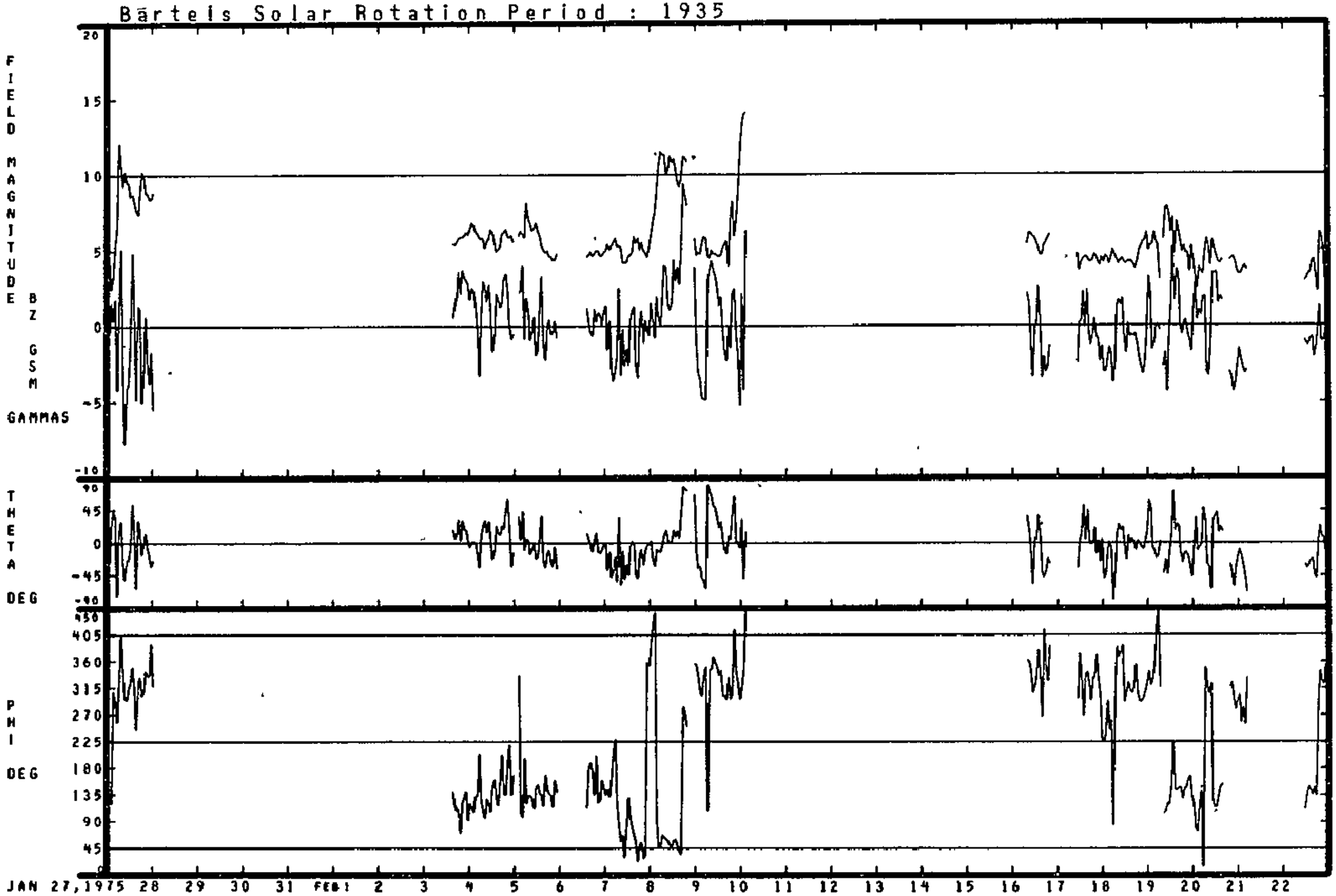
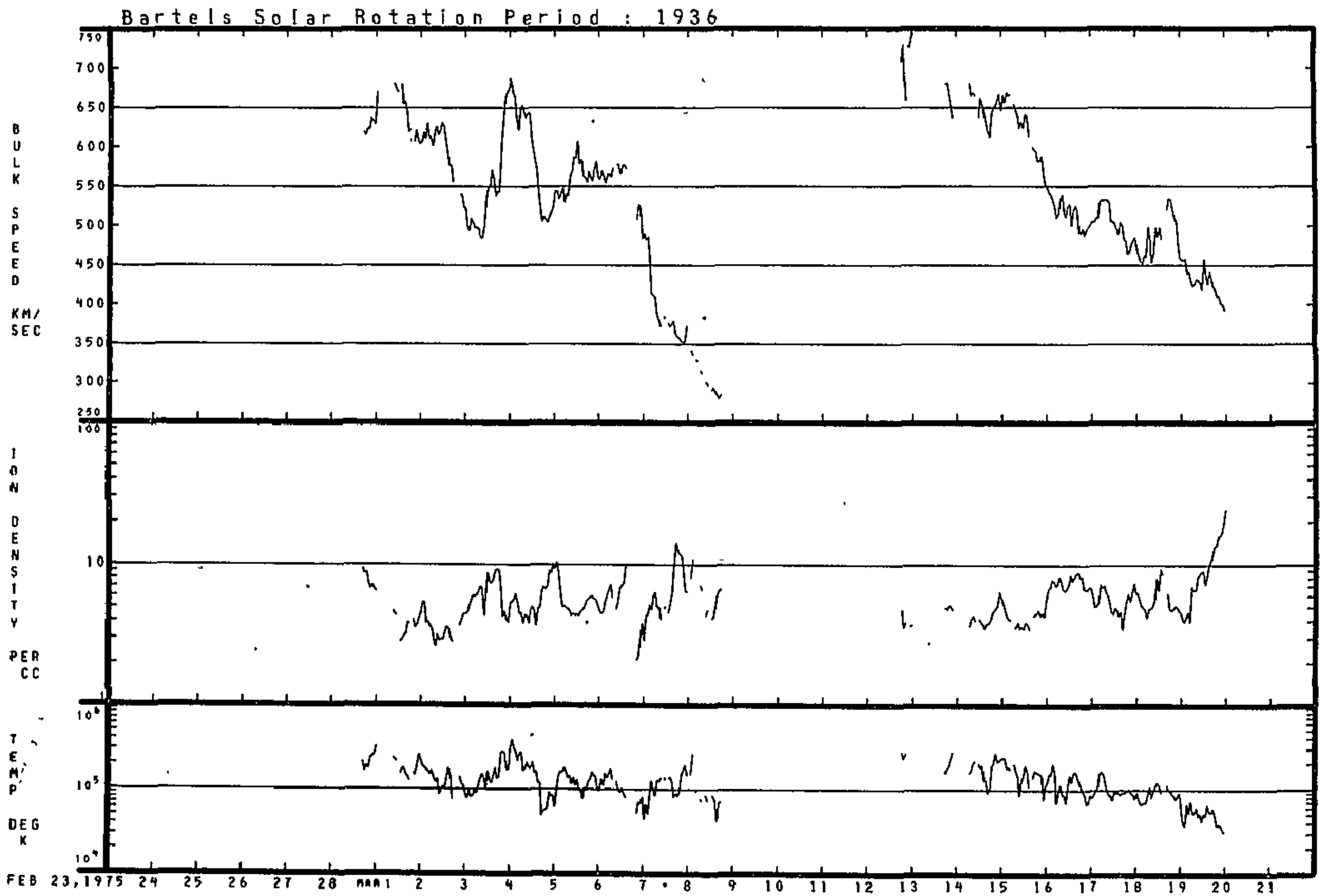


Bärtels Solar Rotation Period : 1935



01/27/75 - 02/22/75

02/23/75 - 03/21/75



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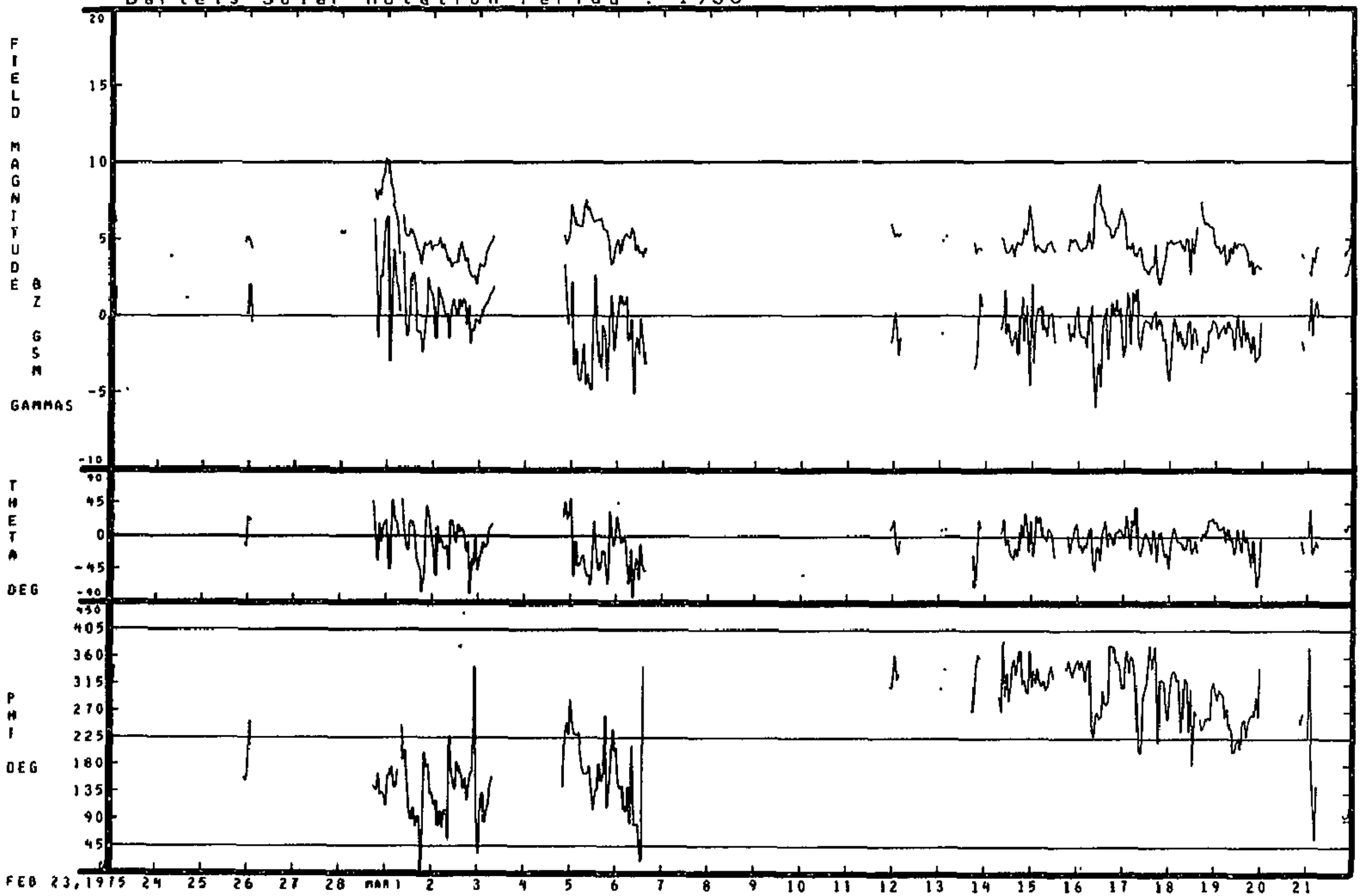
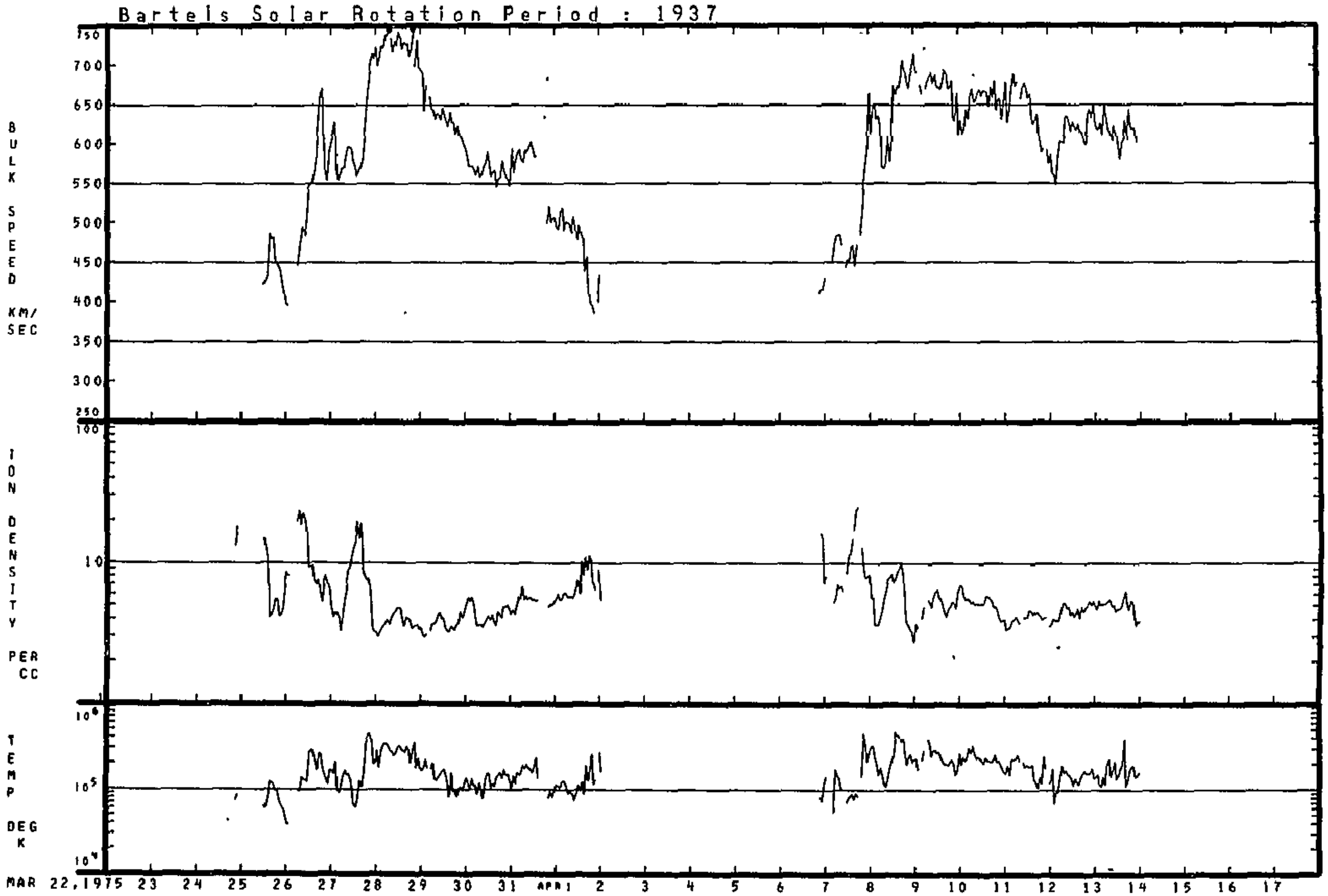


02/23/75 - 03/21/75

03/22/75 - 04/17/75



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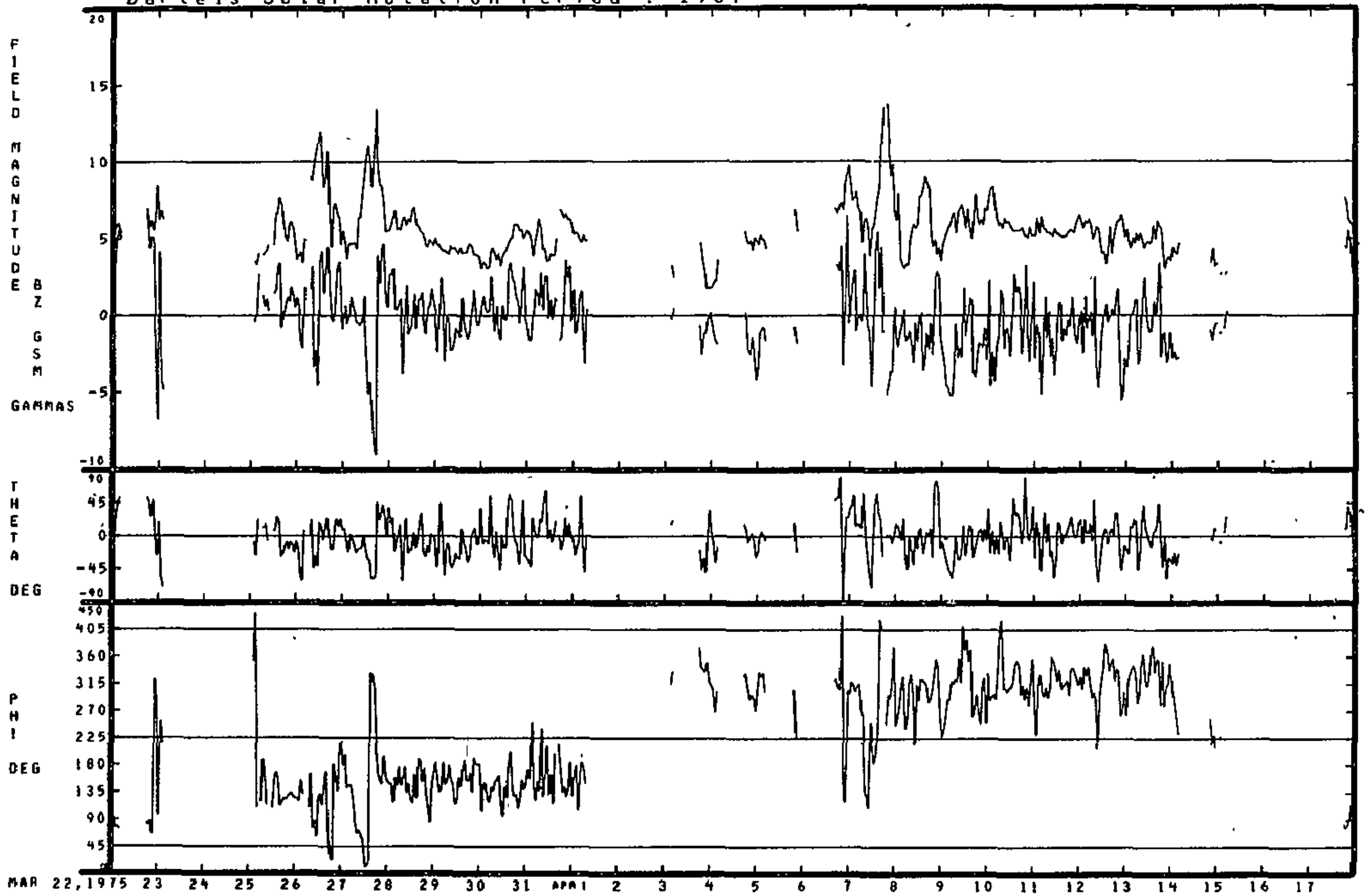
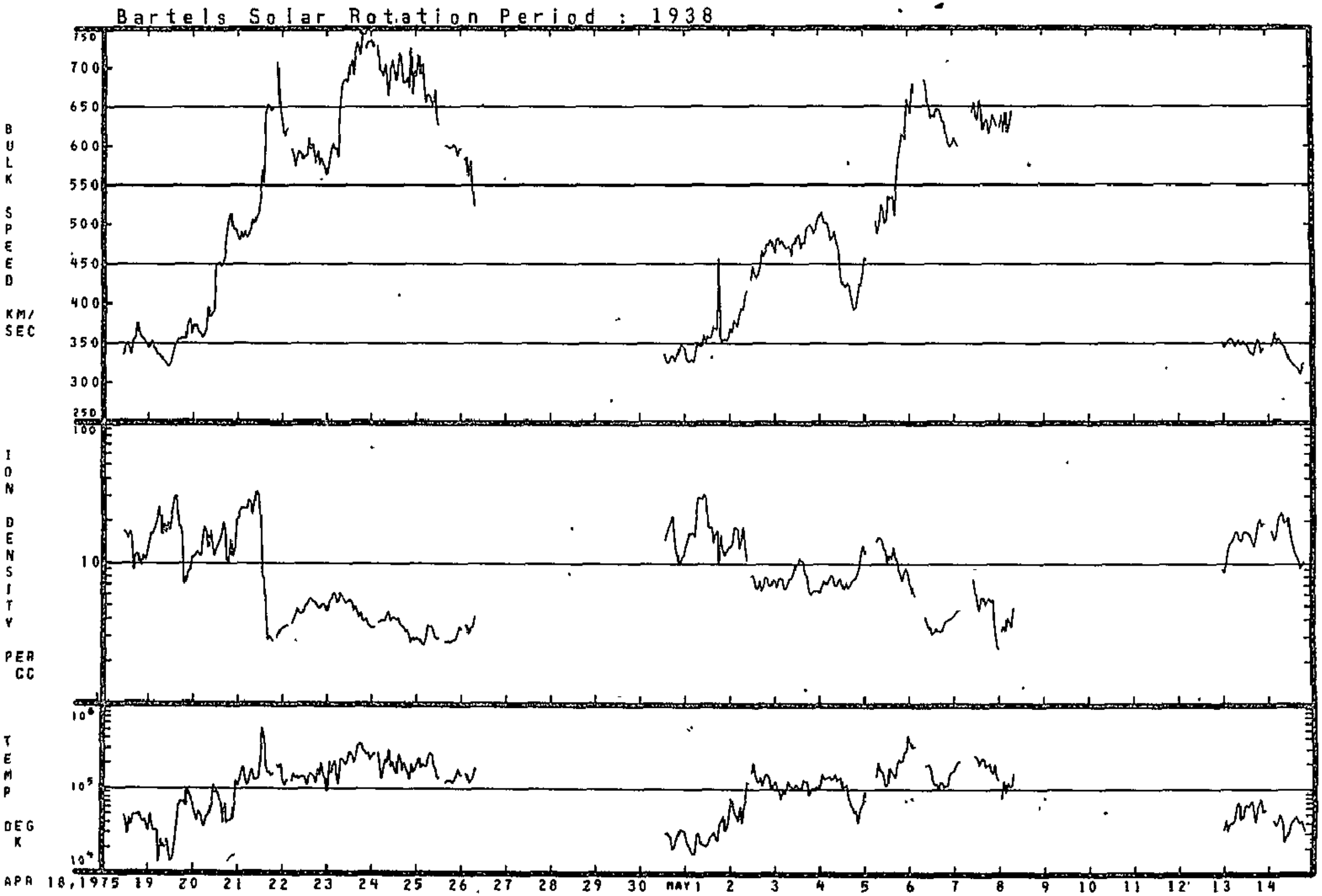


03/22/75 - 04/17/75

04/18/75 - 05/14/75



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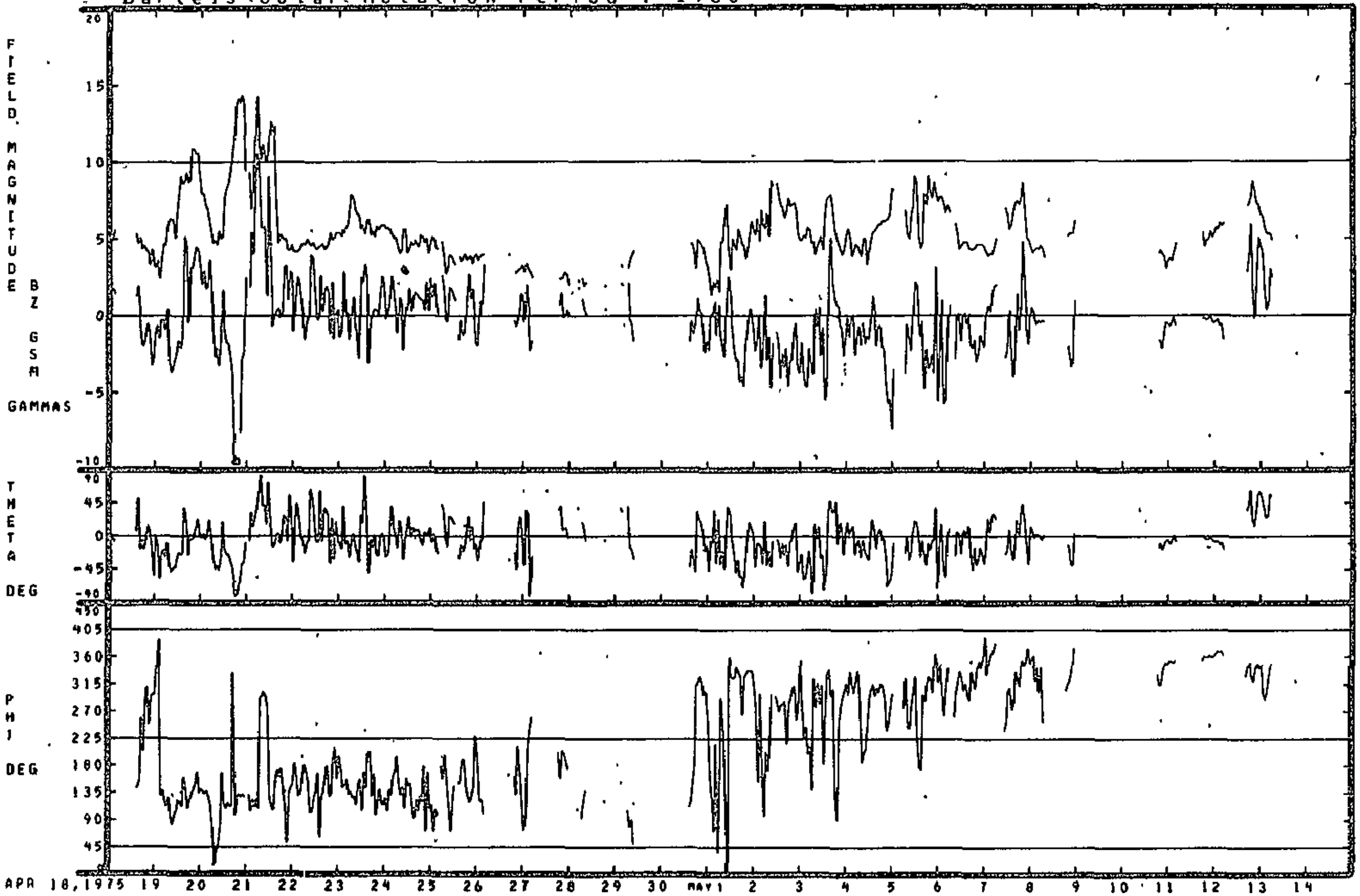
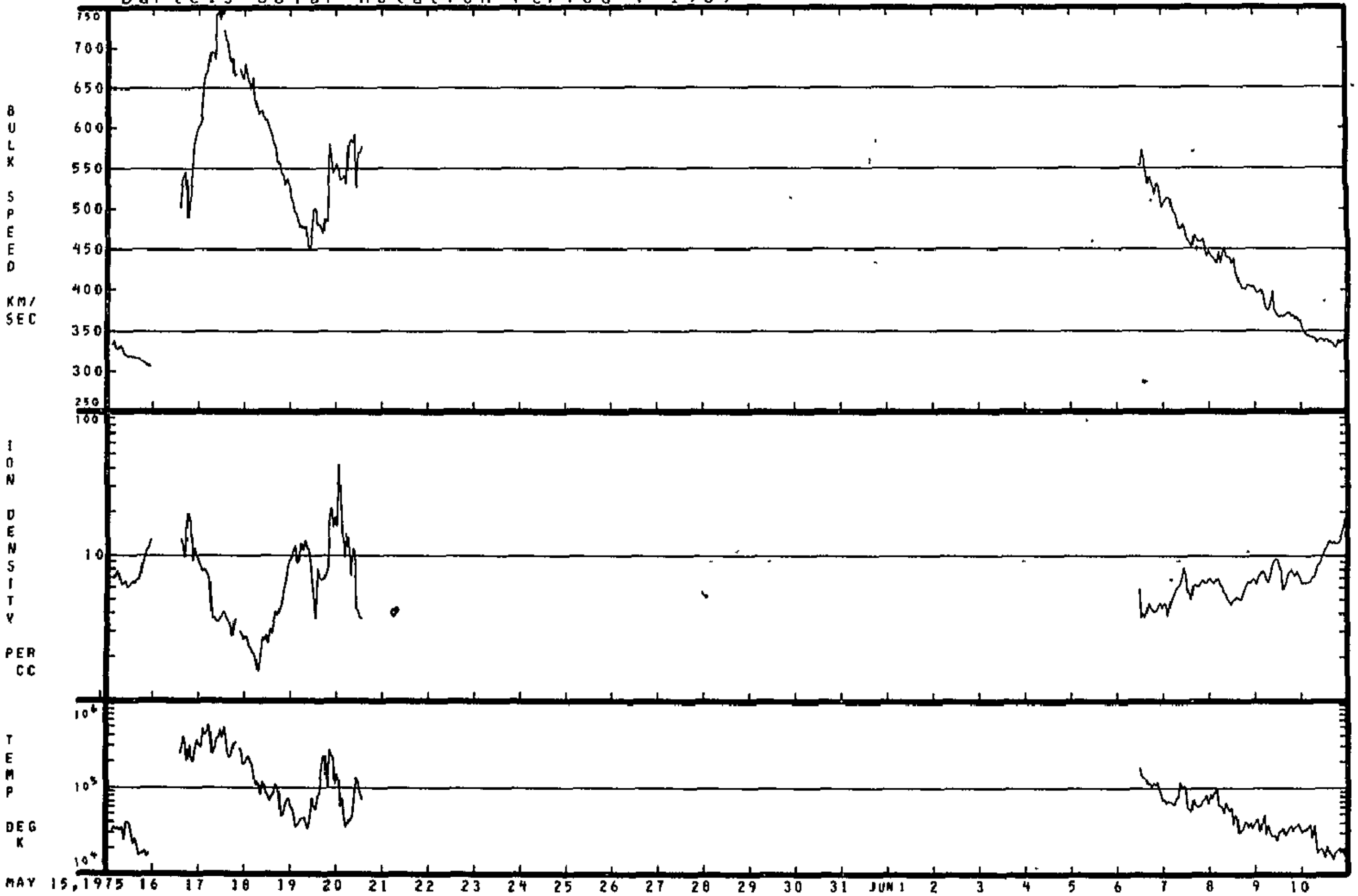
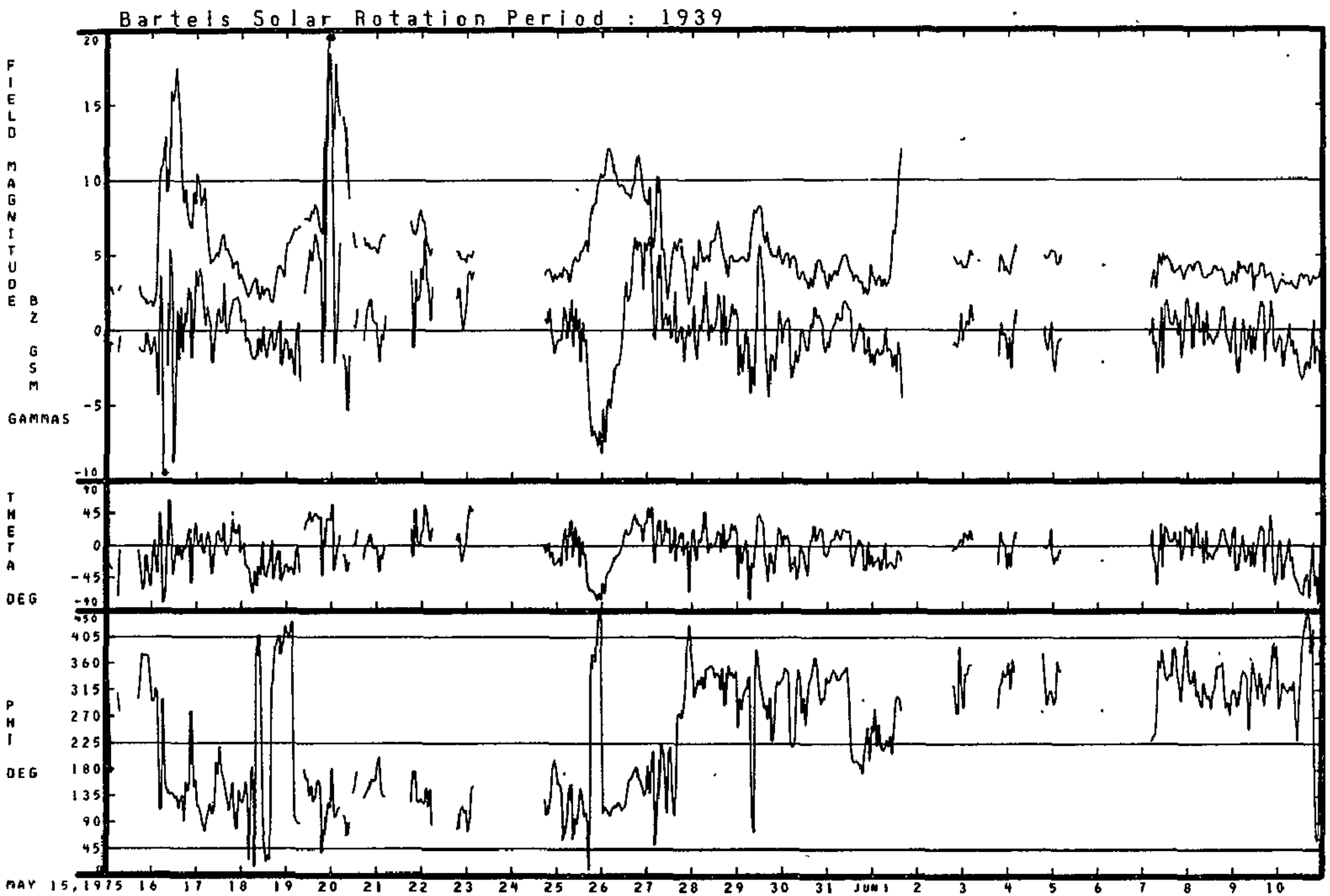


04/18/75 - 05/14/75

Barteis Solar Rotation Period : 1939



05/15/75 - 06/10/75



05/15/75 - 06/10/75

06/11/75 - 07/07/75

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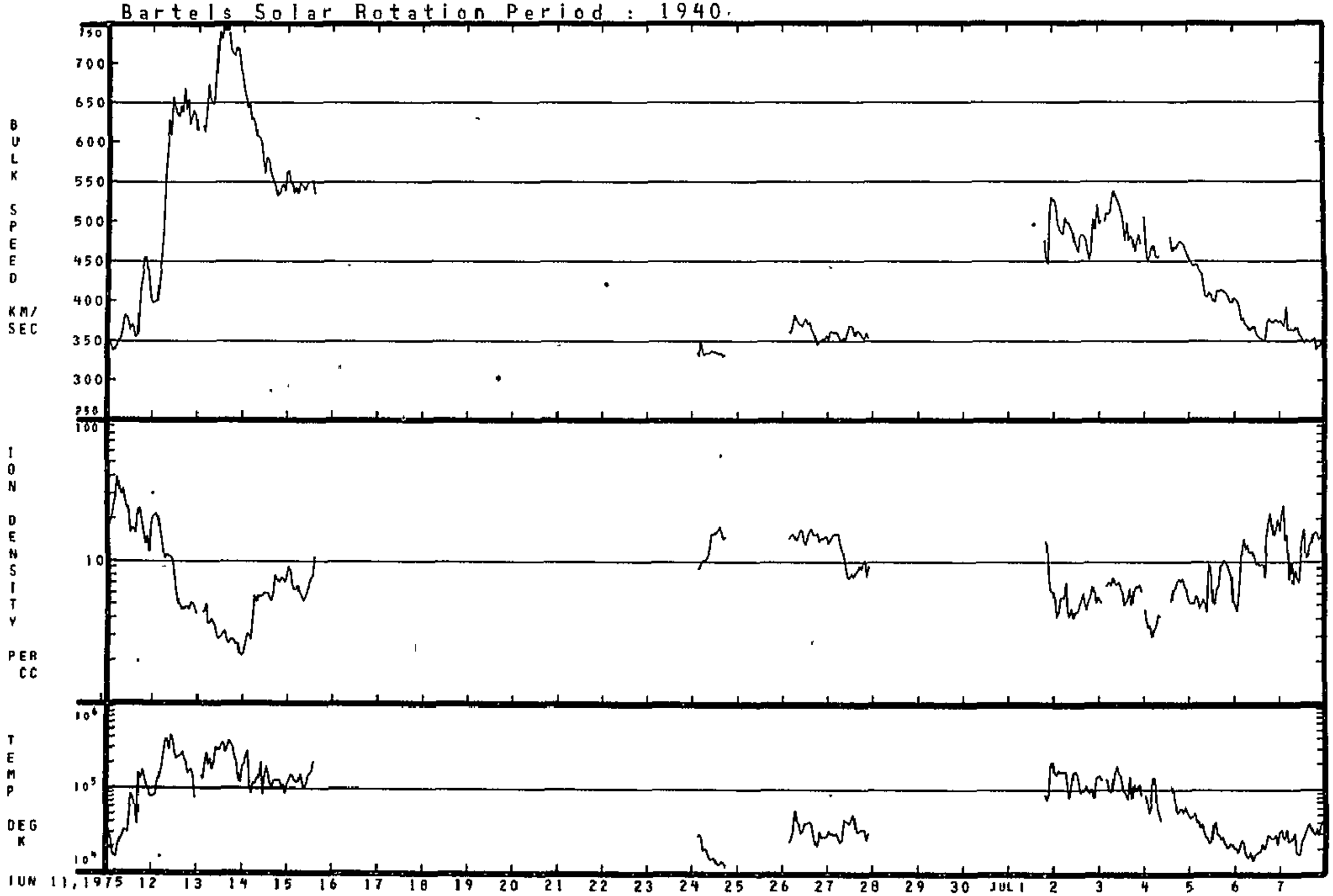

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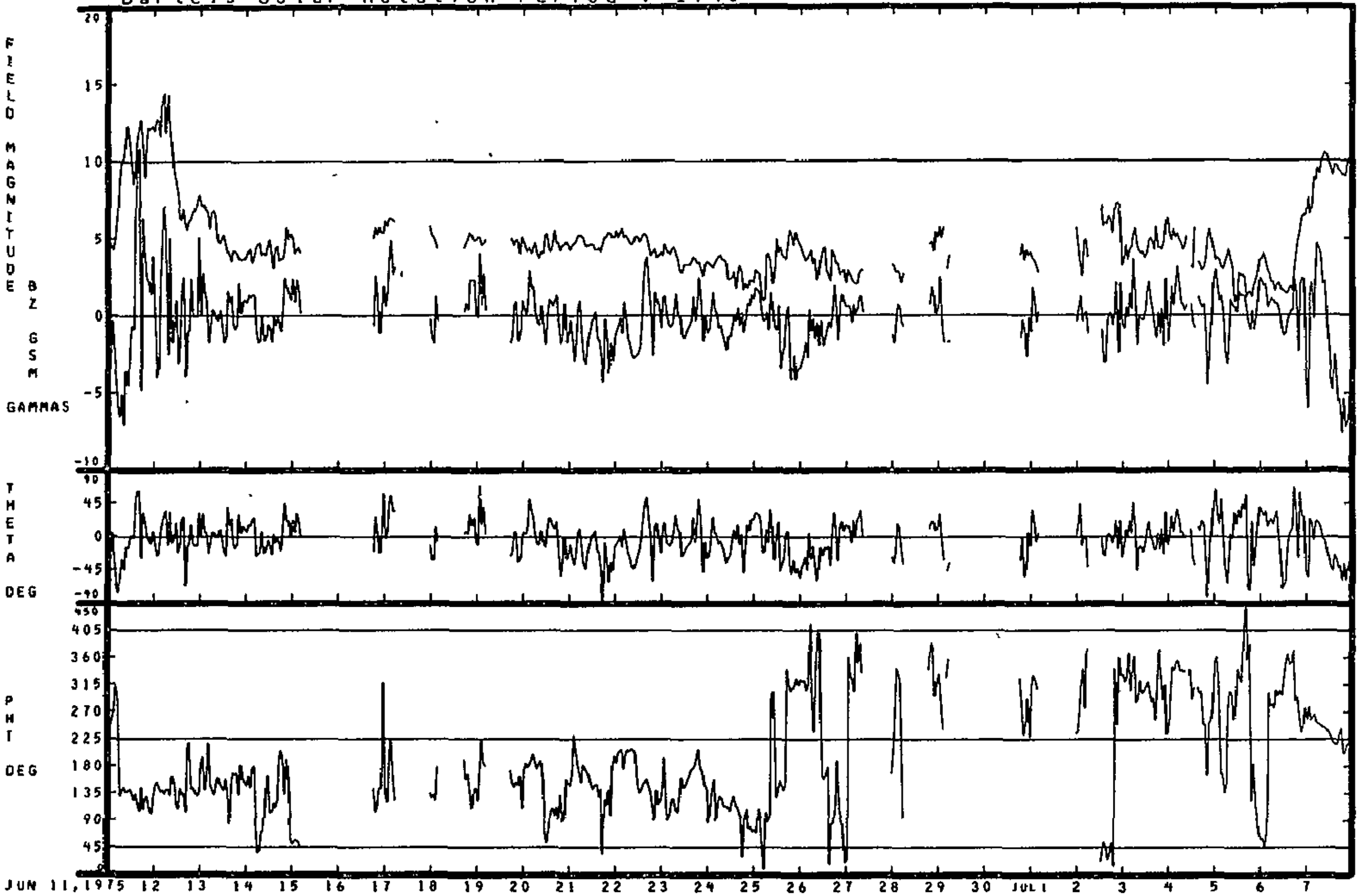
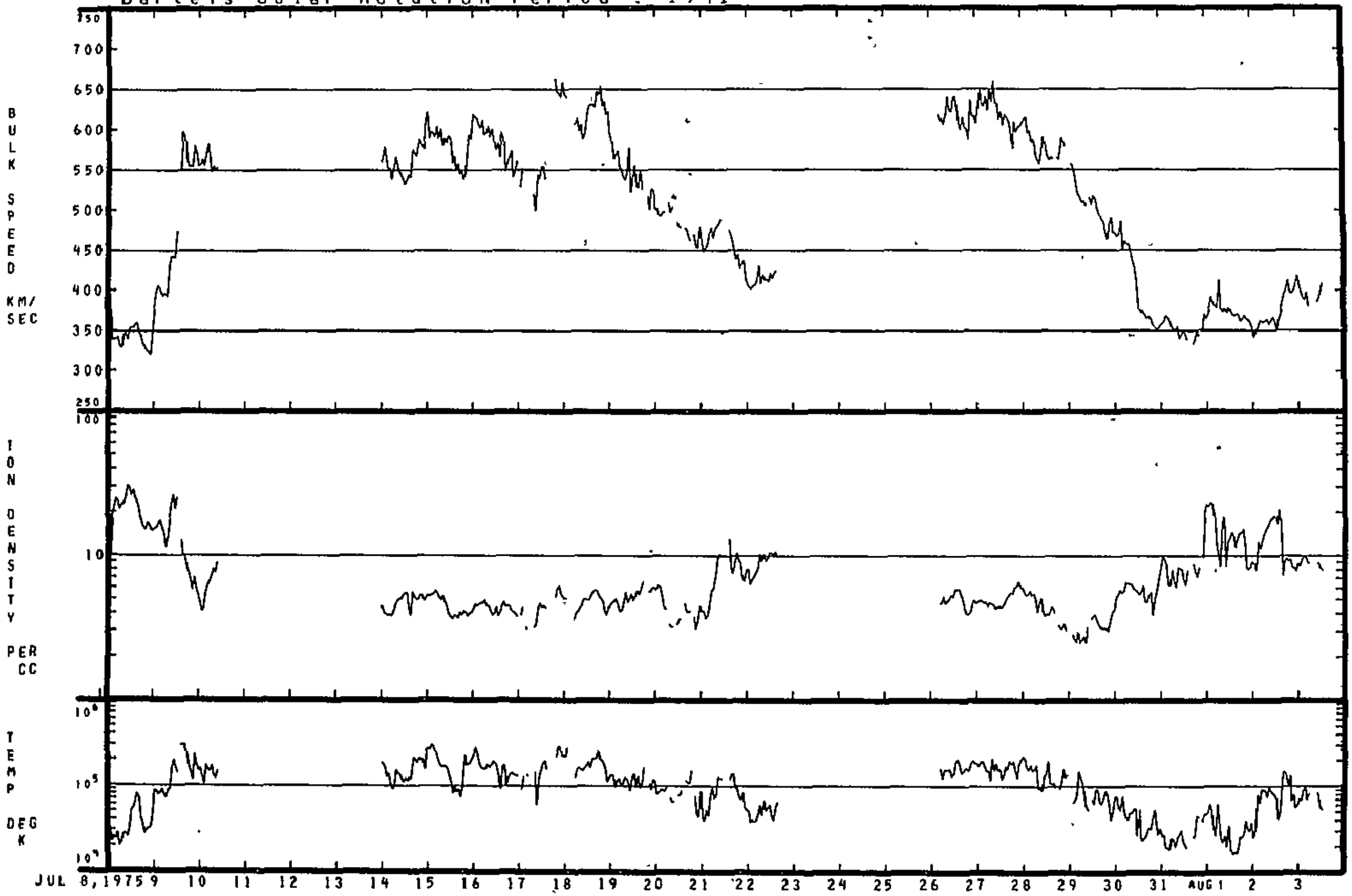
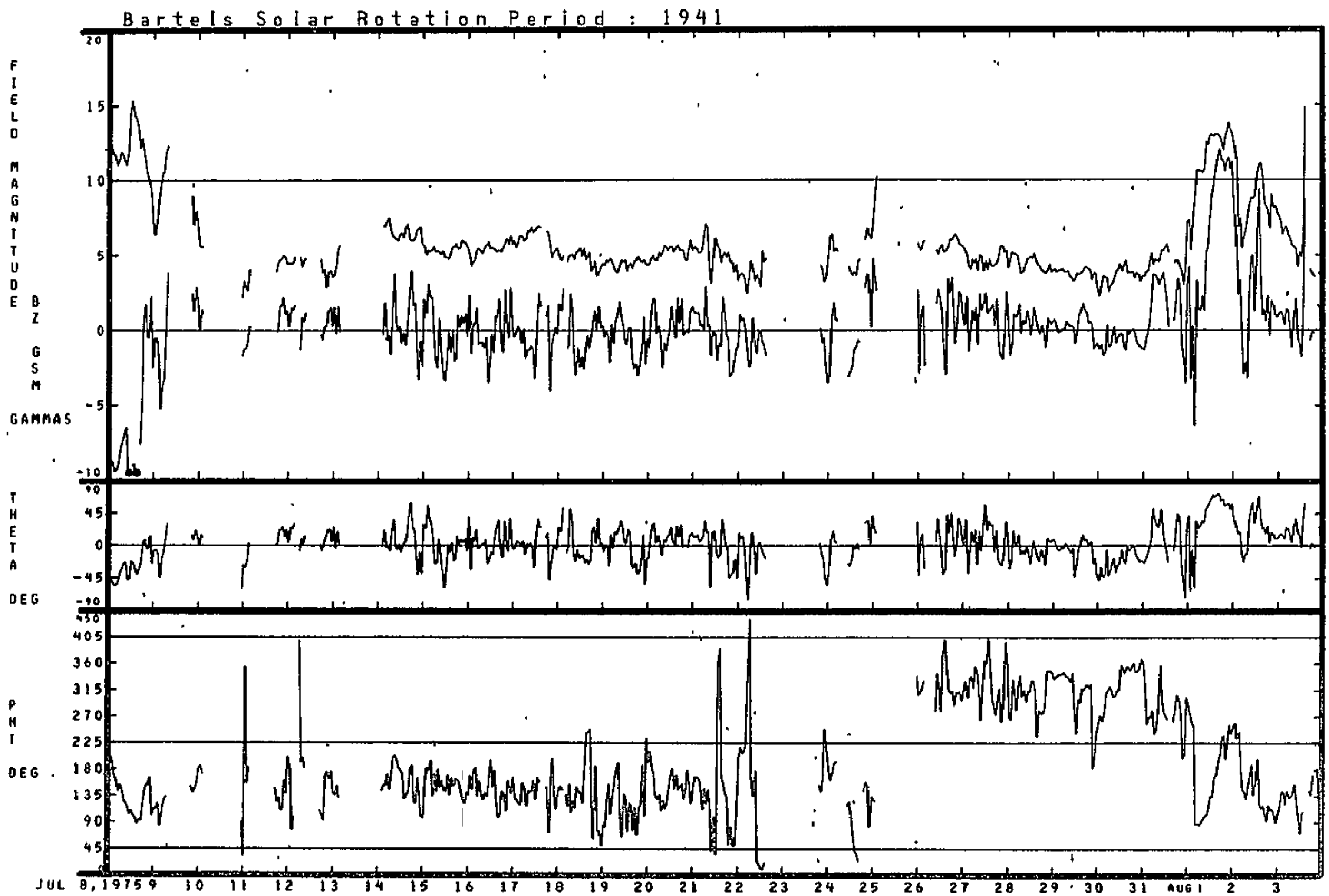


06/11/75 - 07/07/75

07/08/75 - 08/03/75

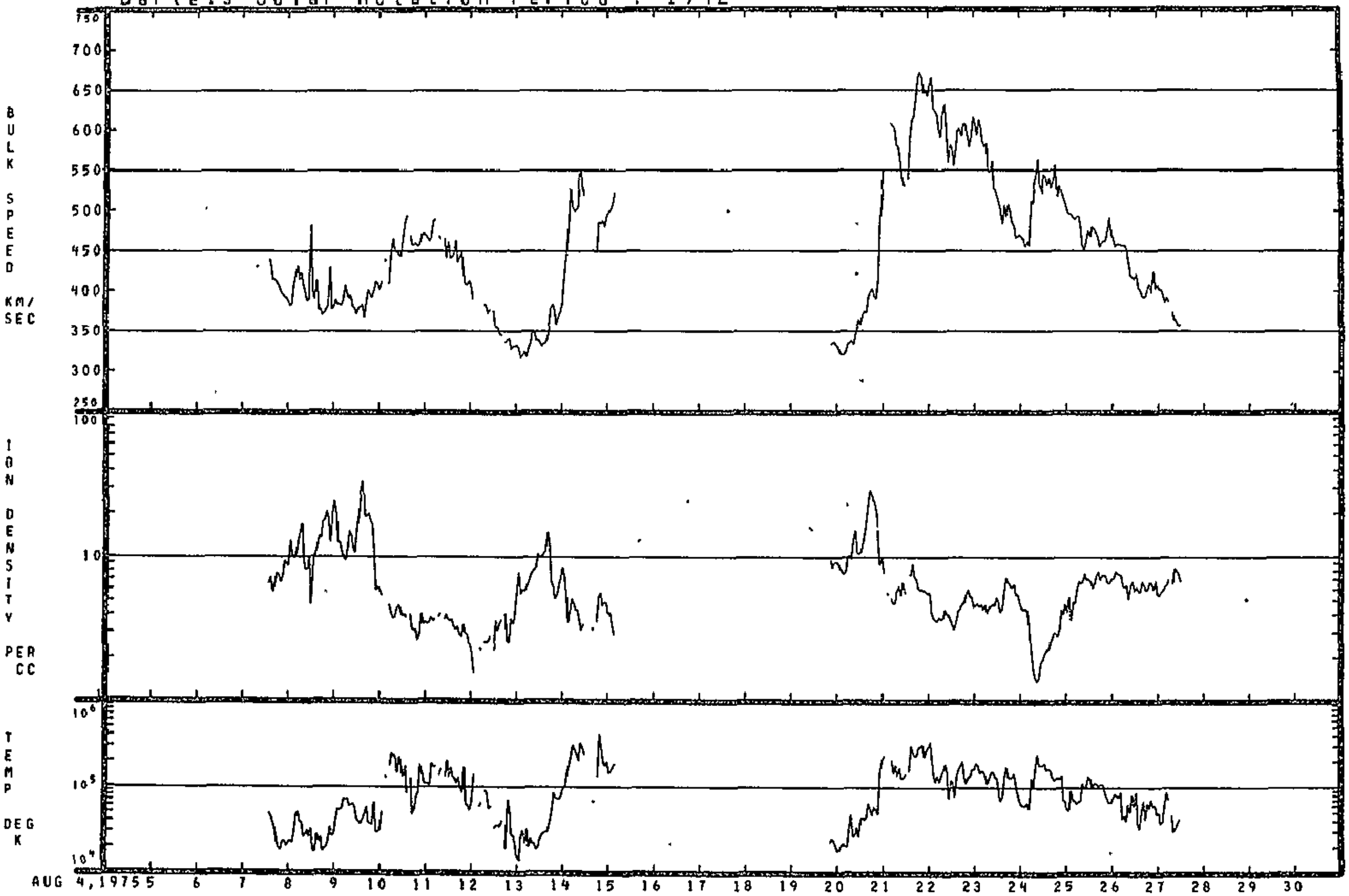
Bartels Solar Rotation Period : 1941



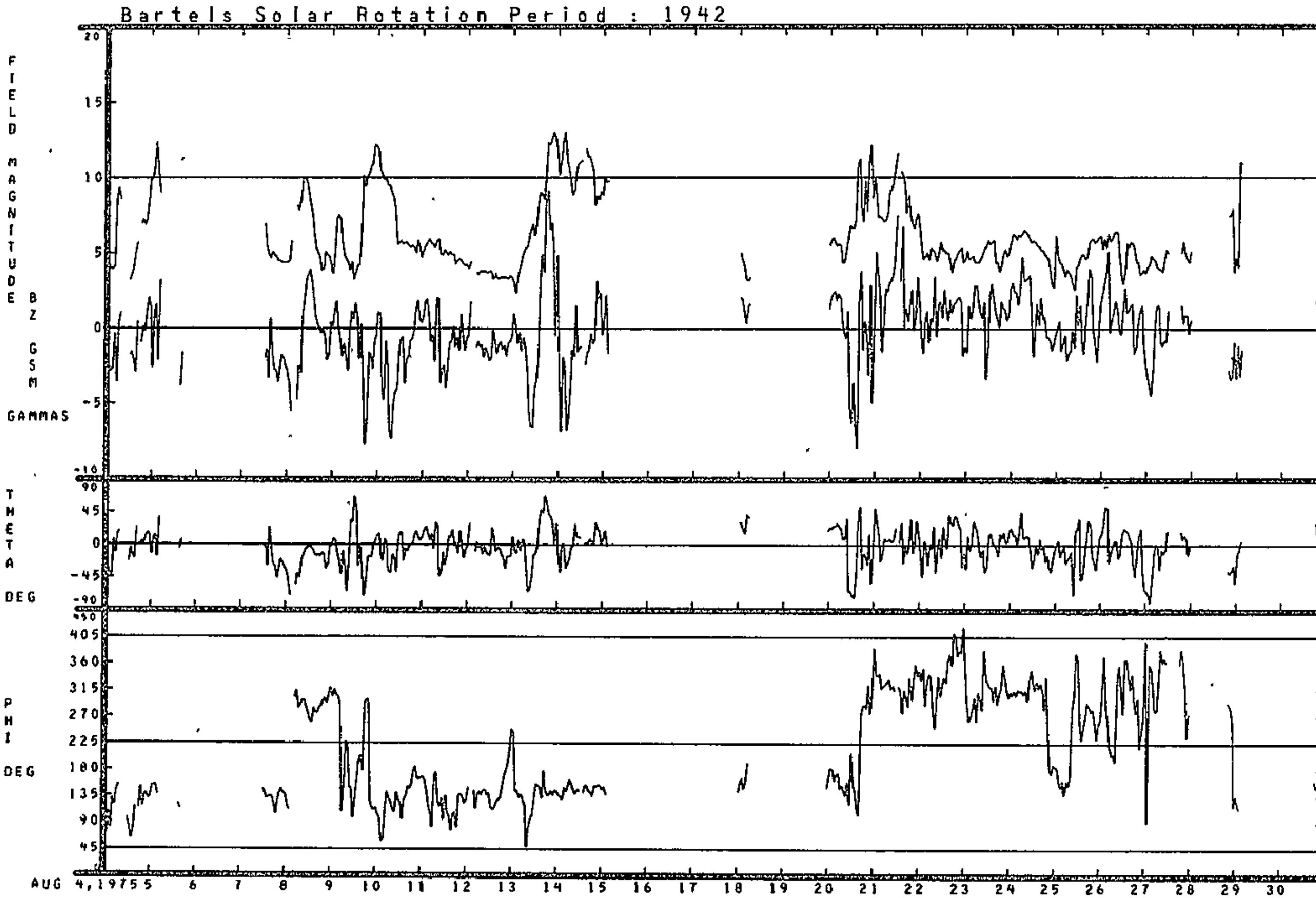


07/08/75 - 08/03/75

Bartels Solar Rotation Period : 1942

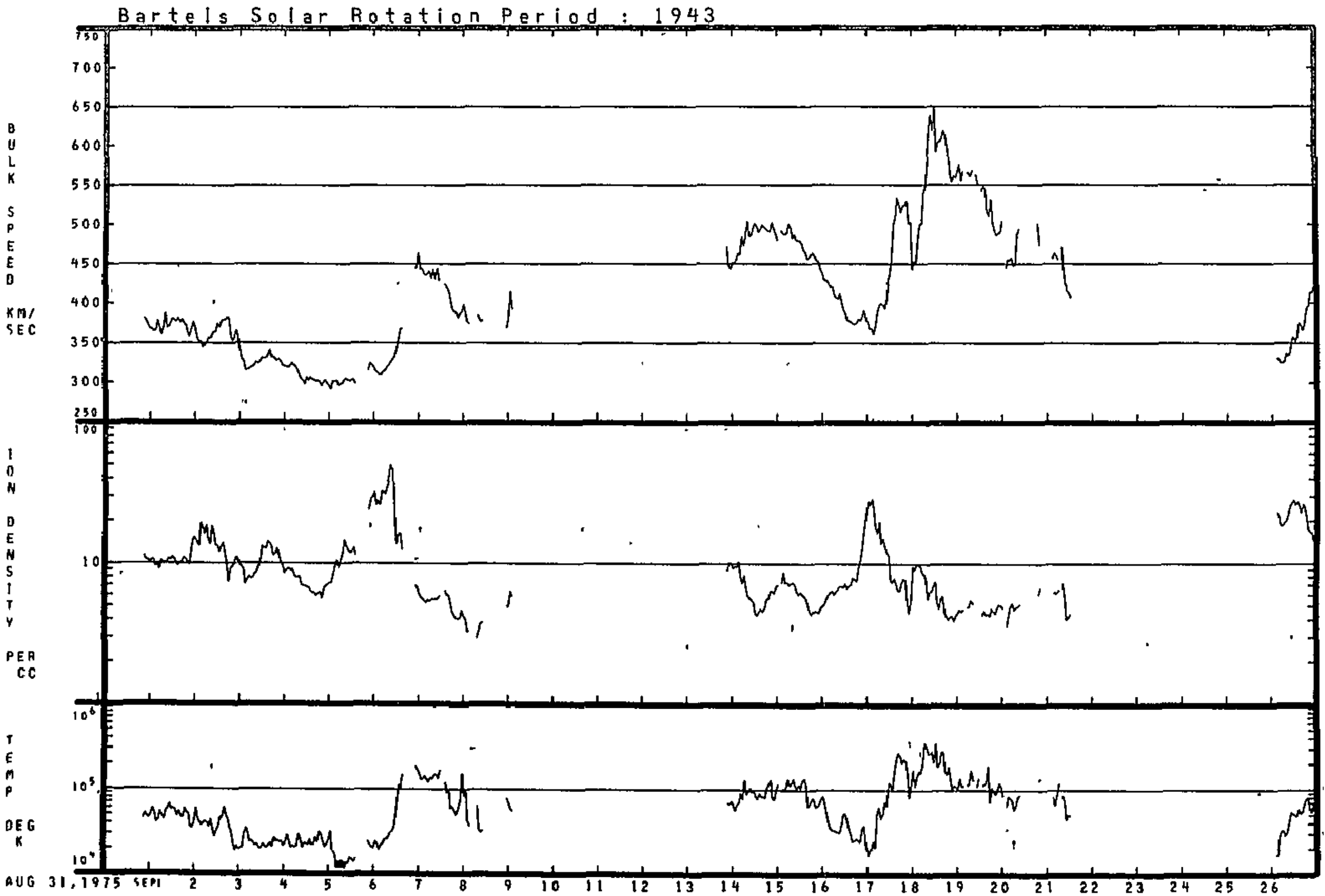


08/04/75 - 08/30/75

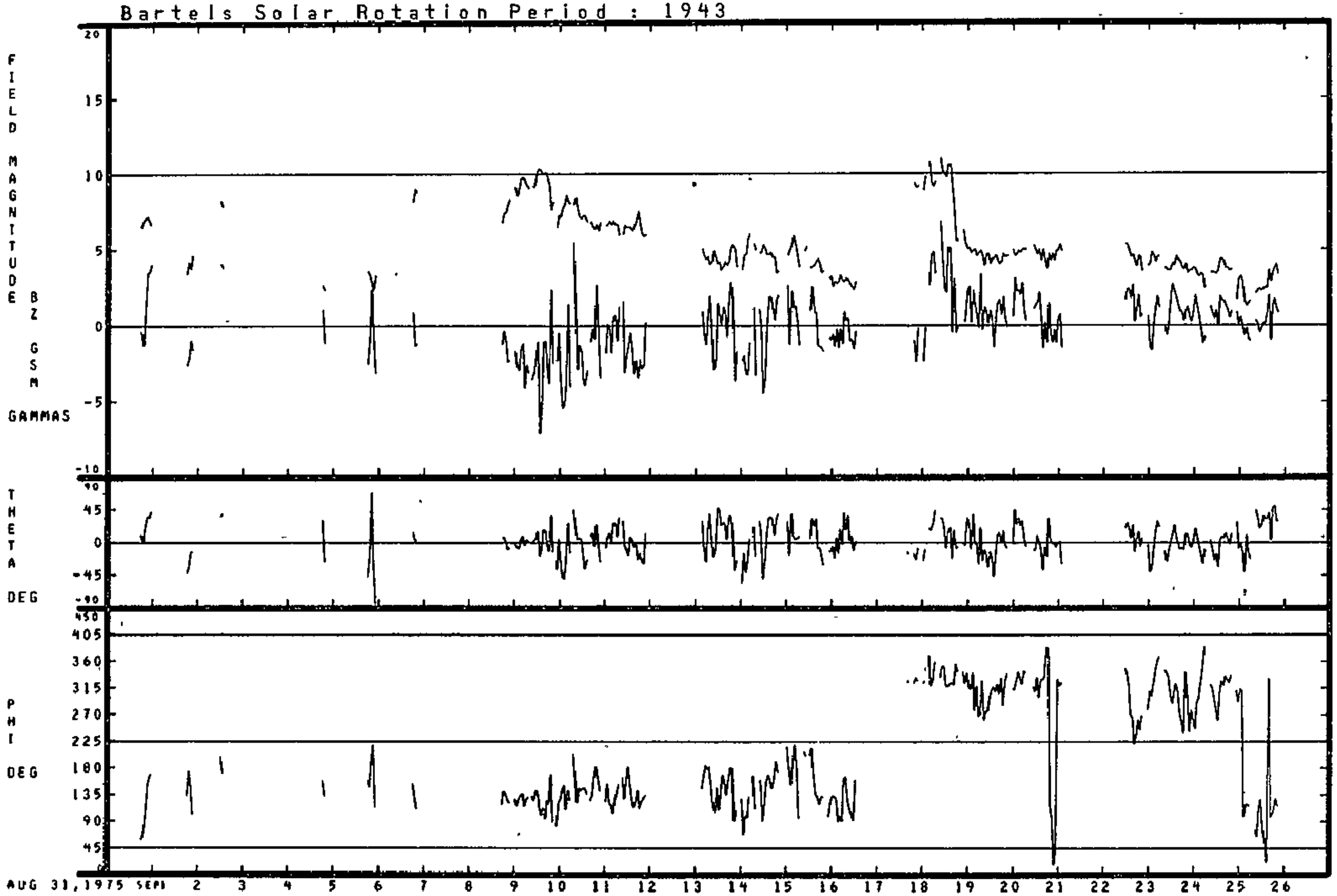


08/04/75 - 08/30/75

08/31/75 - 09/26/75



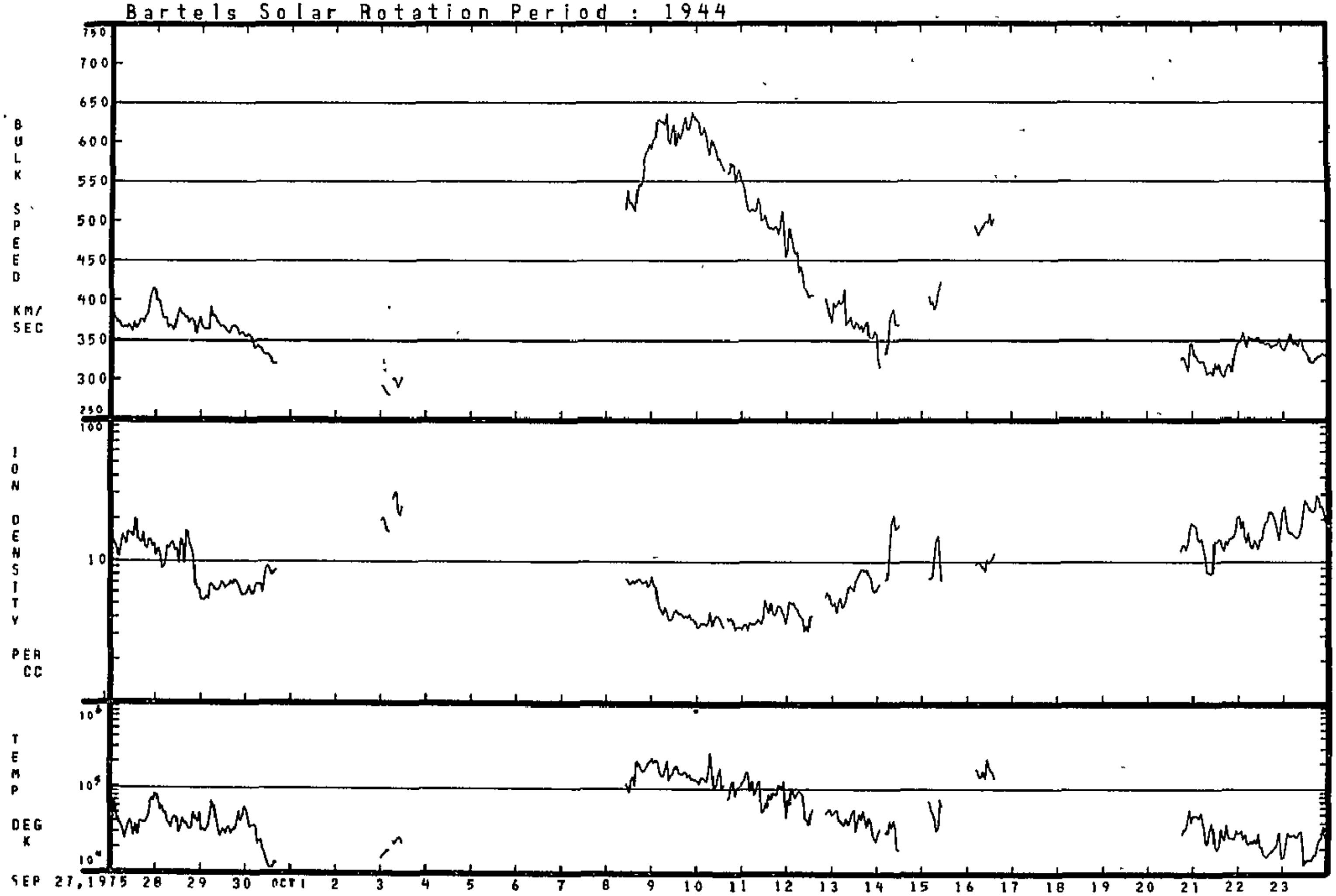
Bartels Solar Rotation Period : 1943



08/31/75 - 09/26/75

09/27/75 - 10/23/75

Bartels Solar Rotation Period : 1944



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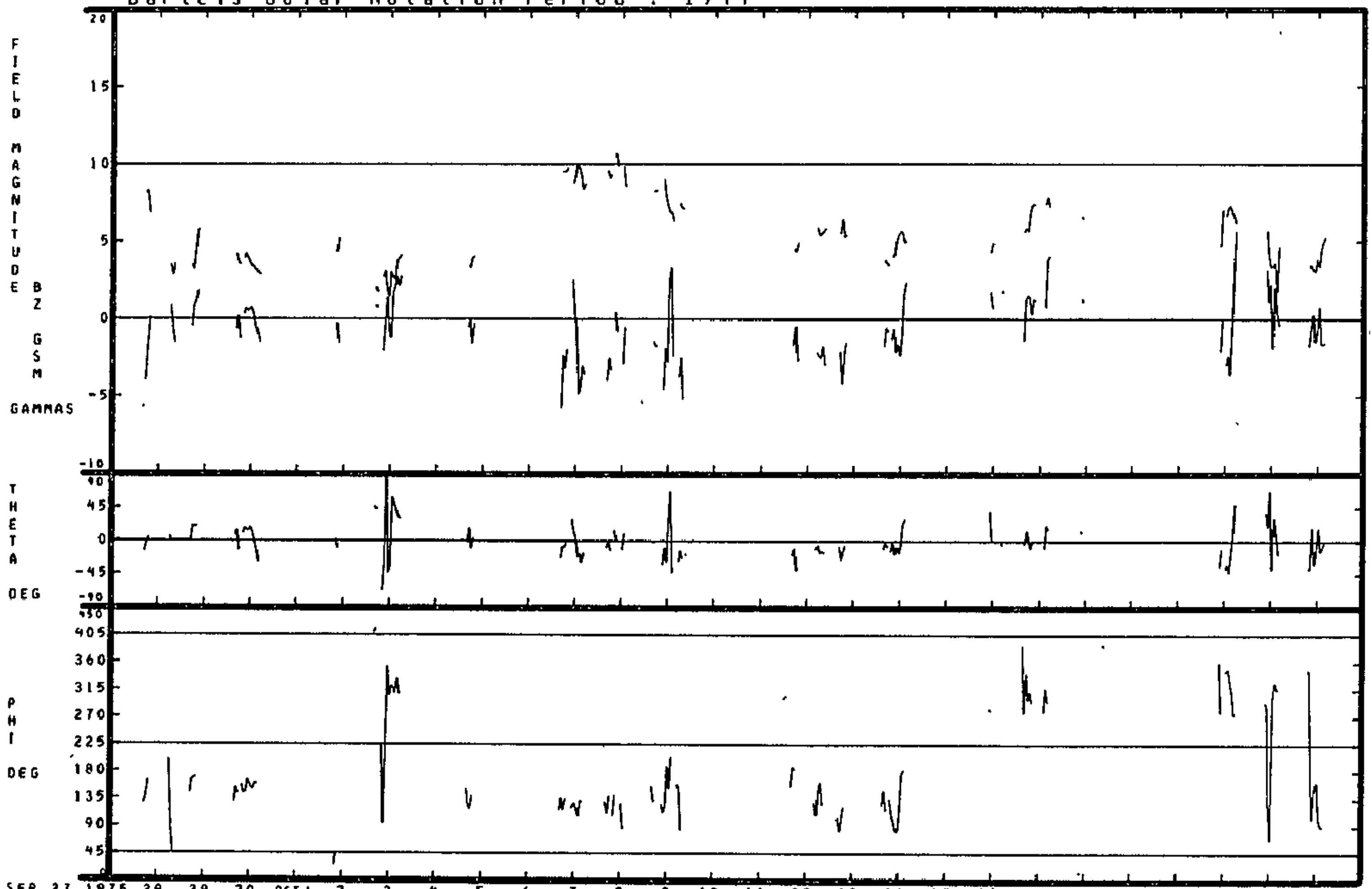
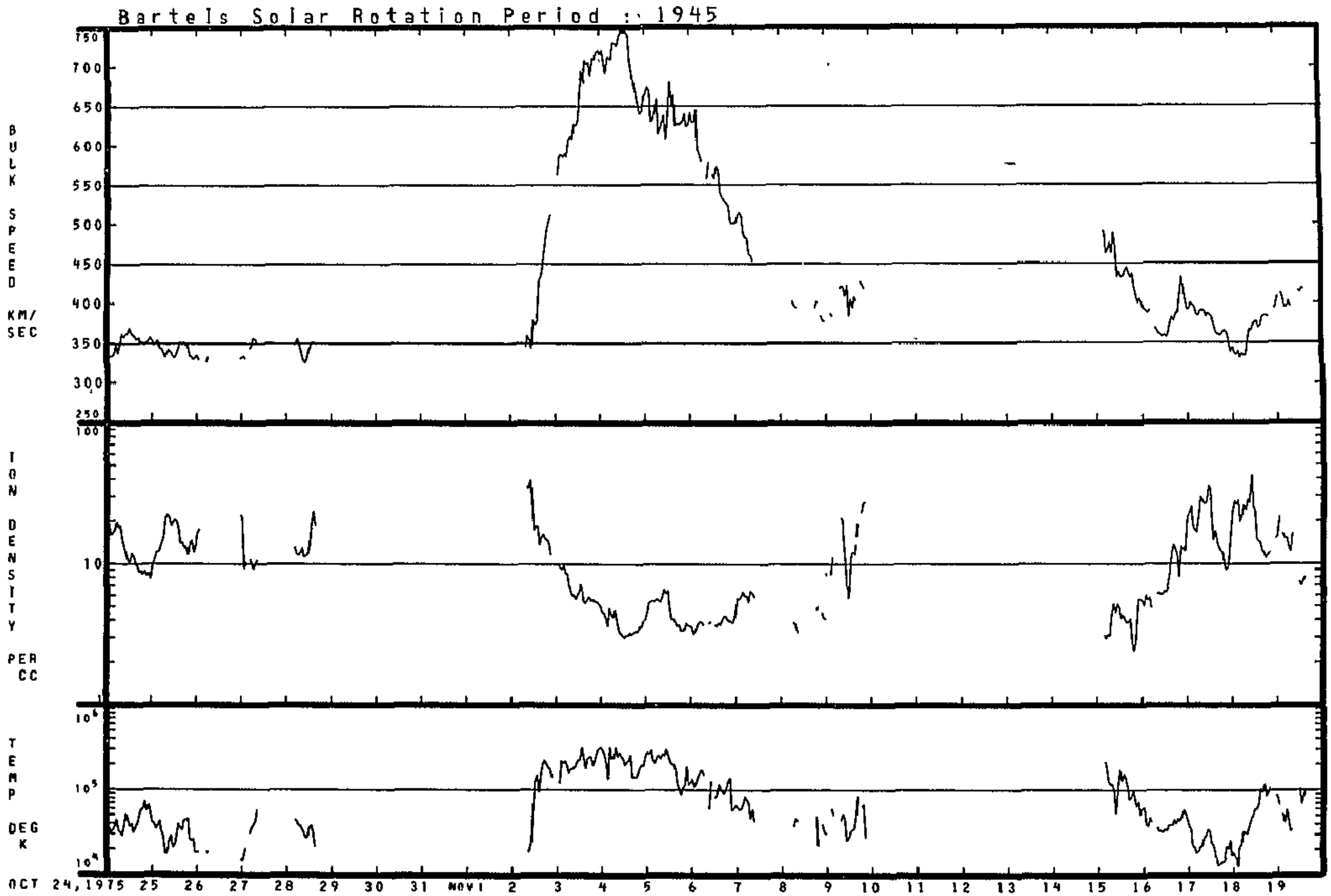
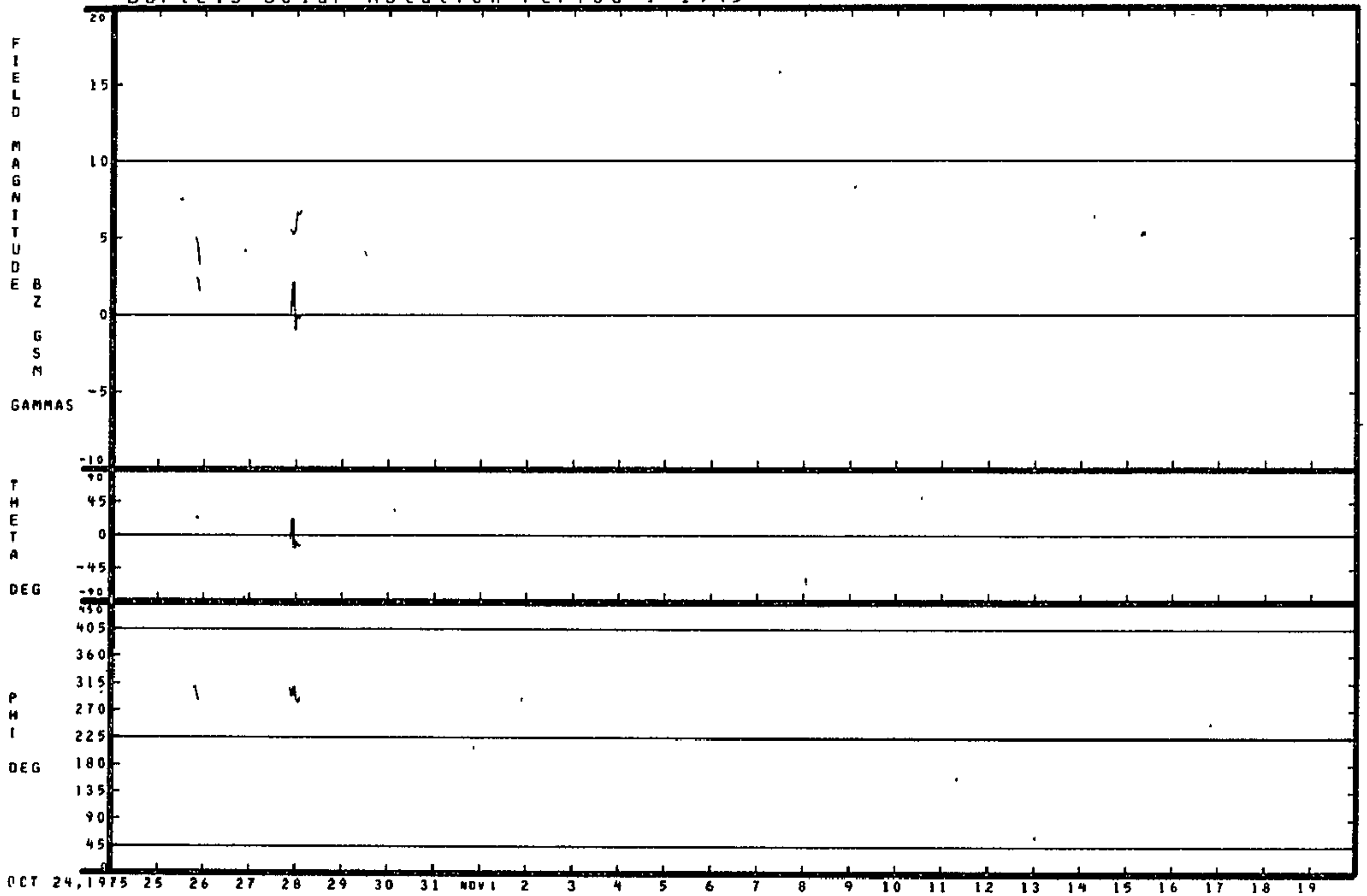


09/27/75 - 10/23/75



10/24/75 - 11/19/75

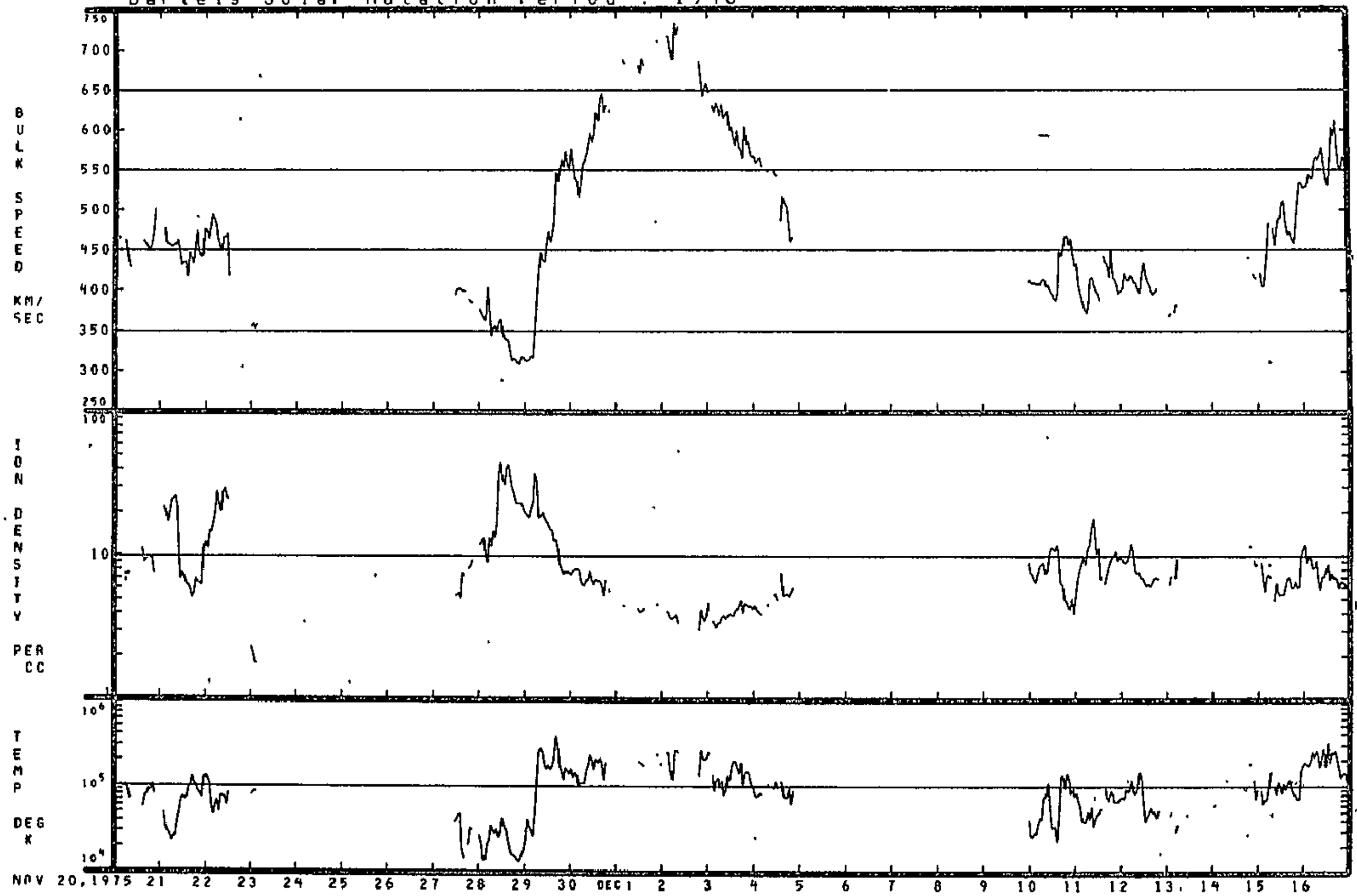
Barteis Solar Rotation Period : 1945



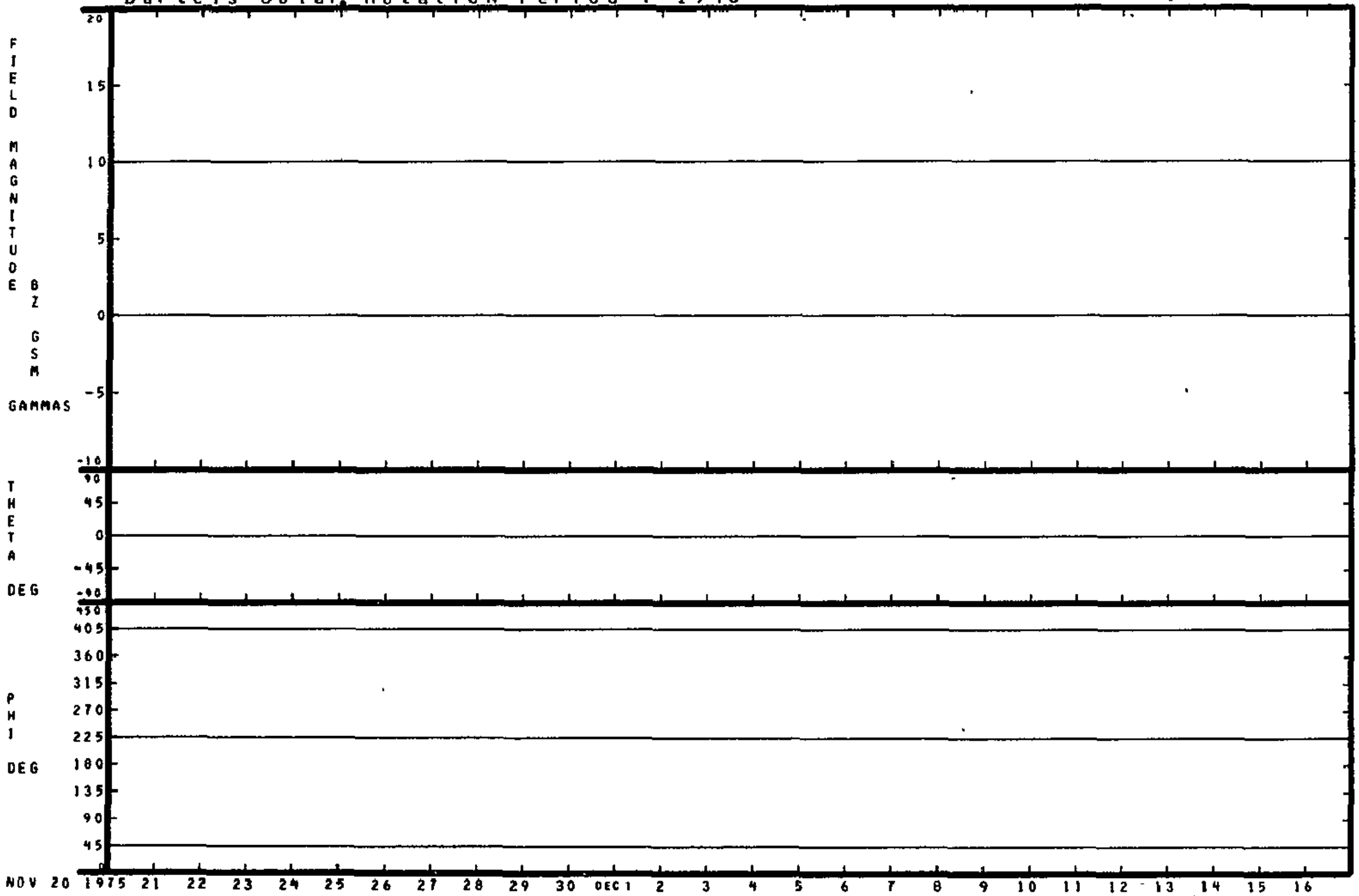
10/24/75 - 11/19/75

11/20/75 - 12/16/75

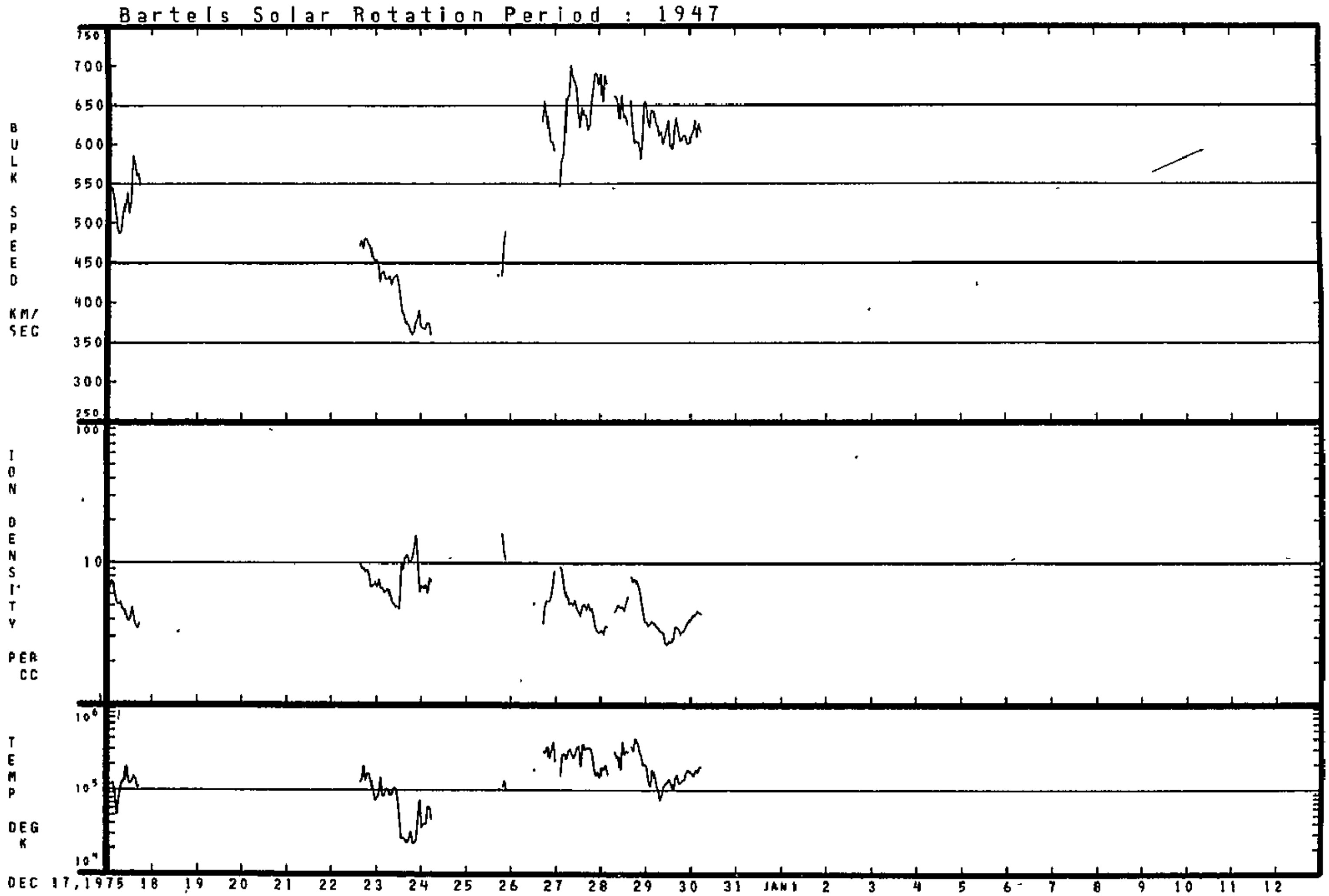
Bartels Solar Rotation Period : 1946



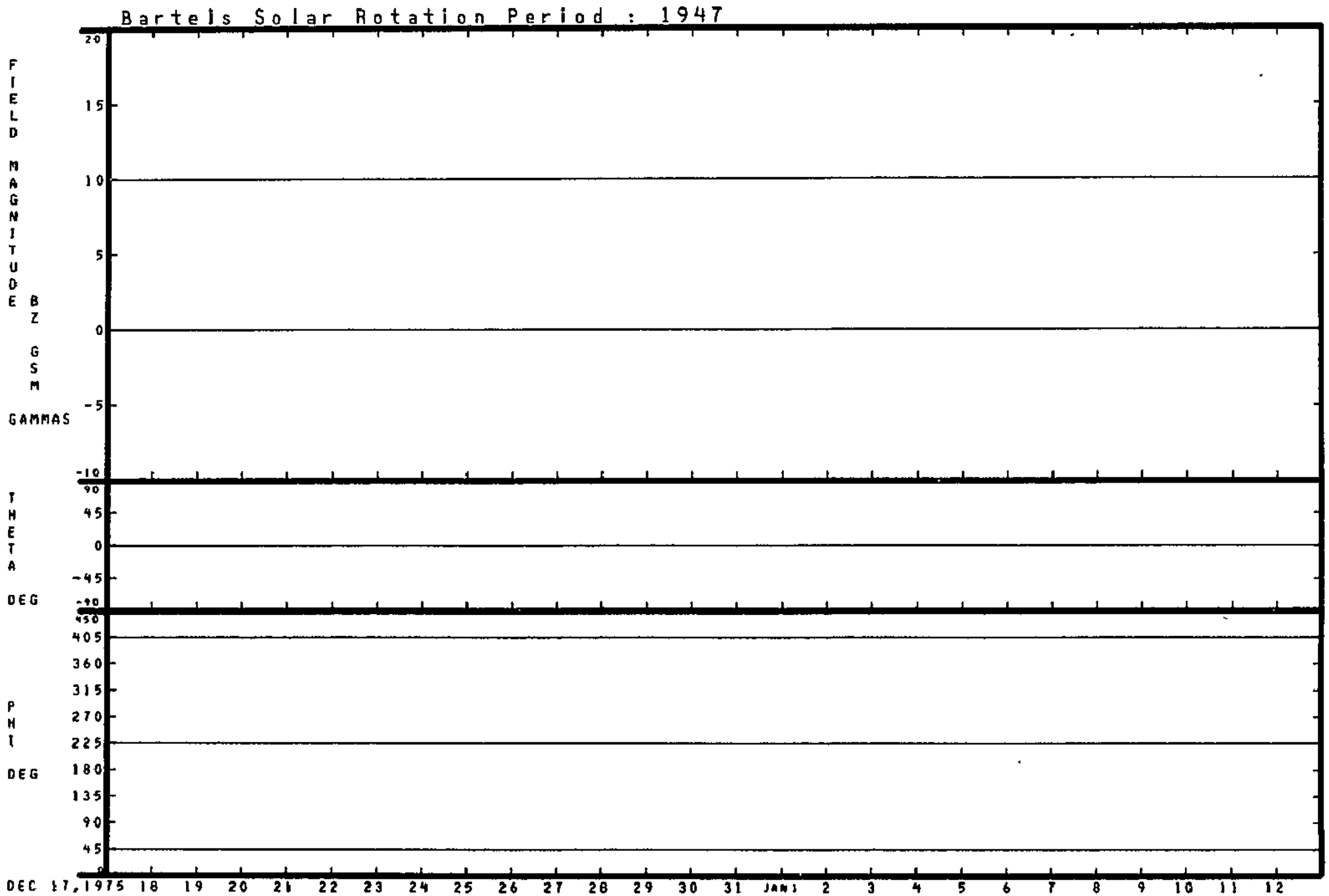
Bartels Solar Rotation Period : 1946



11/20/75 - 12/16/75



12/17/75 - 01/12/76



12/17/75 - 01/12/76

Commercial Practice Based Continuing Education

Courses offered to Practicing Engineers by Commercial Practice Organizations

37-Total Number of Organizations Responding (Base)
67-Total Number of Courses Offered (Base

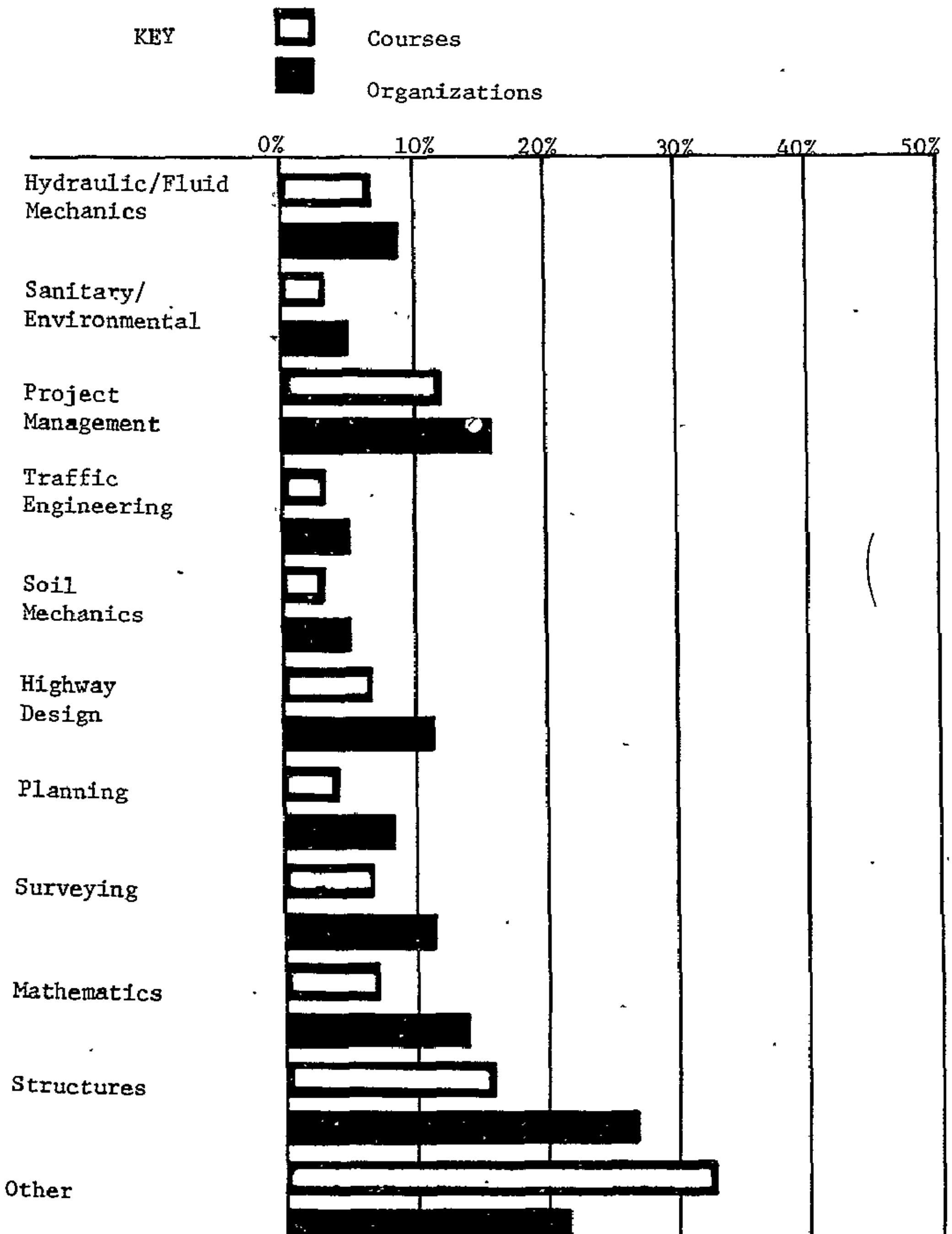


Figure 4

Harvey McComb:

Next is Mike Gaus from the National Science Foundation.

Mike Gaus, National Science Foundation:

First of all, I represent an organization that does not have a direct operational responsibility for generating programs or getting them disseminated. Secondly, because of the nature of our activities, we have a large number of grants which provide support to university researchers for study of a large number of different subjects. As a result of our experience, I have come to the conclusion that there is a large need for computer programs of varying complexity and not just those built for large computing systems. I think it is rather interesting that either this morning or yesterday I was talking to someone on the phone and the question was raised as to how many civil engineers use NASTRAN. So, I got to thinking, well if you visualize the average civil engineering office, it is usually an office which contains, on the order of 14 to 16 engineers, most of them probably, at best, ad hoc programmers, much less software engineers. One may come to the conclusion that probably not many civil engineers do use NASTRAN because the typical kind of problem that they are involved with is more of the garden variety, small building, or small project of some kind, and NASTRAN would be completely inappropriate for their particular application. There is a serious problem in generating and transferring software suitable for an industrial group which is as disaggregated as civil engineering design and construction. A similar situation would be found in the mechanical design area. So I guess a good part of my interest is in a rather diversified group of people.

In connection with large scale computing systems, I think that there is a big job to be done in improving dissemination, verification, certification and so forth. However, the less sophisticated user and the large number of less sophisticated programs also need attention because there seems to be a large amount of difficulty in transferring such programs from the originator to potential users. Within the National Science Foundation, computer-related activities are carried on in several divisions. The Division of Mathematical and Computer Sciences provides support in what I would call software science. The Division of Engineering, RANN, and other programs provide support which often generates programs related to the solution of specific problem areas. To better understand the problems involved in transferring software from originators to users, we have supported a number of special studies over the last several years. Among these were the Workshop on Engineering Software Coordination in 1972, the ASCE Report on an Investigation of the Feasibility of Establishing a National Civil Engineering Software Center 1973, a study on the Attitudes Toward Computer Software and its Exchange in the Pressure Vessel Industry in 1973, a report on Industrial Engineering Software Library in 1973, and a study by CEPA resulting in a report entitled a Proposal for a National Institute for Computers in Engineering in 1975.

The overall problem is not inconsequential. In 1971 the General Accounting Office made a study in which they estimated that the investment by the Federal government in engineering-related software approaches something on the order of 2 billion dollars per year. The question is, having made this investment, how can the public get their hands on the large amount of software for which they have already paid for.

The construction people took a look at this particular problem and they concluded that their difficulty is the fragmented nature of their industry. As pointed out, often we deal with a small office and we need some means for getting both large systems transferred to these people and to allow them to get access to smaller software systems which they may not know about or may not be able to use even if they find out about them. The conclusion was we need some type of software center for the construction industry. We had a study made by the industrial engineers and they reached roughly the same conclusions except that their approach was a little different. The industrial engineers approach was that they felt that they may be at the stage of development where, rather than an actual software center for doing this, what they need was an information system to allow them to identify all the various software in their area and related areas which they can get their hands on.

The CEPA report which Steve Fenves showed you slides of also indicated the magnitude of the problem in just the one area that they looked at. I think they identified on the order of 15,000 programs which are kicking around the country related to their area. Now if you multiply this by the number of engineering disciplines, this will indicate the difficulties from the standpoint of the users in trying to find the software which will serve their particular function.

At the present time there are a number of active groups around the country who are engaged in the dissemination of software; one was mentioned by Jim, COSMIC, which is doing an excellent job in the Aerospace area. There is one which we have been involved with more or less for some years. And this was an effort that was set up in connection with the Earthquake Engineering Program the foundation supports. A National Information Service for Earthquake

Engineering was set up as a part of this back in 1973, and they decided to experiment with making available software which would be required for carrying out structural dynamic analysis related to earthquake effects. This center now distributes software packages primarily prepared by universities, but there are some other people who donated various packages to the center which they had developed but wanted to make them available to other people. More recently they have taken somewhat of a more active approach and they are trying to take some of their packages and modify them so that they will run on a wider variety of machines. In this way they make software available to all who are primarily involved in structural dynamic analysis related to earthquakes. There still exists a lack of verification, certification type of activity for these programs.

There is a great need to complete the loop, and Harry Schaeffer, who is sitting in the front row here, has been very active in pointing this out, that in order to have a successful dissemination program, you really have to have a feedback route to get some feedback from the user who will get this software.

In conclusion, there is a question I would like to throw out and I would like to get some discussion going on it, this is that there is not only the documentation, standardization, and so forth of giant multipurpose programs for which I think we are doing a rather excellent job but, in order to look at the broader aspect of dissemination and technology transfer, we must also look at a diverse group of people who make up the present community of software users. This is the end of my presentation. Thank you.

Harvey McComb:

Thank you Mike. The audience has sat there very patiently while we unloaded all this stuff on you. So we are going to take a break in just a minute while I make two announcements. In order to provide large space for some of the technical sessions, there may be a relocation for some of the sessions tomorrow. They still will be in Marvin Center. But you are urged to check the registration area on the fourth floor lobby for any posted changes.

PART II - - AUDIENCE RESPONSE AND DISCUSSION

Harvey McComb:

I open up the meeting for discussions now. If anybody in the audience has any questions or comments or remarks, if you would, please state your name and affiliation, we would appreciate it.

Dr. Bodhe, Stone & Webster Engineering Corporation:

We are involved in nuclear power business and we are users of very large systems such as STARDYNE, ICES-STRUDL, SAP, and so on. There are two or three questions I have in mind; one is the full implication of certification and verification. What do you mean by that in actual terms? Have you considered the cost involved in that? A typical example is taking ICES-STRUDL, which happens to be one of the dynamic systems. We are testing a structure with several hundred joints whose analysis consumed about 13 hours of our computer and cost 15 thousand dollars to run it after the bugs are out. So, how are you going to pay for this, and who's going to pay for this? How are you going to certify programs like this? How are we going to certify the people who are going to use this? And the second thing is, are we serving the industry by having this kind of certification, verification, and what purpose are we achieving?

Bob Nickell:

The question is directed toward certification of software and people, and the question had like three parts to it. How can we justify the cost, and who's going to pay for certifying programs and people, and what purpose does it serve to certify people? I think those were the three parts of the question. And the way the question was asked, implied that there is someone up here who was advocating certification of people and of programs, and I don't believe that is the case. We are trying to find out from you whether or not the user community thinks that it would be desirable. My comments were aimed at trying to define what certification involved.

Ok, here's what the costs are like. A typical program for certification such as in the case of Nuclear Class Welders is extremely expensive. We are talking about thousands of dollars per individual. I do not think that certifying computer program users is going to be any cheaper because you are going to have to send them off to short courses, you are going to have to send them back to school, you are going to have to conduct in-house training programs, and all that kind of overhead items. It is going to be very expensive. What about the cost for programs? I do not think anybody ever estimated what it takes to certify a program; I do not think it is very desirable in my own personal opinion. What purpose does it serve? If it serves the purpose of protecting the public safety, and it was absolutely required, it is going to be done and somebody is going to pay for it. In the case of certification of Nuclear Class Welders, it is paid for by the company itself, who tries to achieve the goal that the Nuclear Class Code stands. It is not paid for by an external agency. It is an in-house expense, so to speak. Anybody else want to comment?

Ralph Miller, Boeing Commercial Airplane Company:

I would like to just make sort of three statements about the contexts of what has been said, and I would like to cite as a reference to my remarks our experience as engineering users that utilize three CDC 6600's essentially 24 hours a day and, in case of structural analysis, programs such as NASTRAN which use about 1/10 of a 6600, day in and day out. And from that I would like to say that I think the programs work well enough. I do not believe there is a significant demand for program certification. I initiated that activity at Boeing to try to achieve that. I think it is technically feasible, and I do not think the cost is prohibitive, but I do not think that the user community and the management can pay the bill on its own. So, therefore, I concluded it is not required. I do think that training and usage, however, are a major problem. We do use black boxes now as engineers. All of us use Roark's handbooks, textbooks. We do not want to think they are black boxes but they really are. Very few of us have ever done it or are capable of verifying what is in those boxes and I think the computer programs are one of these. And that the industry will, in fact, use them as black boxes. And the third point I would like to make is that I believe that the marketplace is ready for programs that are warranteed. By that I mean that the user can truly treat them as a black box. He can drive the car, he can get on an airplane and not get off at 3962 meters (30,000 feet). Programs do make errors, and his interest in warrantee is that his labor is not going to be wasted, which in turn may have an impact on the schedule of his work. And I think the marketplace is really right for the warrantee of programs so that people can put these bolts and nuts together to build the products that are in their domain.

Jay Wiley, Bechtel Power Corporation:

Being in the nuclear business, I am also a program technical specialist, and my responsibility is keeping the computer programs technically correct. And the question of certification is very important to me. I see people doing very sophisticated calculations, and then going out and taking these numbers and using them as the gospel. Under that set of circumstances, I consider it to be extremely dangerous because I do not think we know all there is to know about what happens in the real world. So I find having to have some sort of correct or acceptable answers is very essential, yet to do that is an extremely expensive process. And unless somebody comes along and says you are forced to do it, it will not get done, simply from an economical point of view. The question about the Welder Code — sometimes the Federal Regulations are basically an implication of quality assurance requirements. The same federal code could be applied to computer programs also; the government would have to simply say this is part of the standard U.S. requirements for all nuclear work and then it becomes mandatory. But I do not see the federal agencies, specially the Nuclear Regulatory Commission having any guts to do that because we had an example earlier in the opening presentation where someone asked a question about doing automated design, and what happens if you use the code and the man said that from a basically legal point of view, if he does it by the code, that is his defense if it falls down. He follows the code which is considered standard acceptance. If the Nuclear Regulatory Commission comes along and said I certify the STRUDL program, and I go off and use this STRUDL program and something falls apart, I may come back and say I did what you told me to do; it is not my fault. And I would like the panel's comments with regards to this particular type of problem.

Bob Nickell:

I guess I draw that assignment again. Let's see if we can recap what constitutes the question before the house here. As I understand it, you are wondering whether or not the federal agencies really have the nerve to impose such a standard through the Code of Federal Regulations, and if they will do so. And I will say that the comments that I excerpted about computer programs, the classes A, B, and C are, in fact, excerpted from the Code of Federal Regulations. It is an amendment, shall we say; it is 10 C FR 50. And I see no reason why NRC would not, in fact, write an additional amendment for computer program user certification, but they will not do so if the ASME Boiler and Pressure Vessel Code and other Standard Organizations do not see a similar need. So I think that we have to have more representation in the code committees by people who are software users, not just those people who sign stress reports and who are registered, but the actual people down on the floor who are doing the computer program calculations. We still have not made our point to the code bodies. And I do not believe that we are still making our point very clearly even to the Nuclear Regulatory Commission. And all I can suggest to you is to keep pushing. They are getting the word, both from the front door and the back. And in another couple of years I have a feeling something is going to move.

Anonymous:

I know I just had a conversation with a fellow just the other day who is working on what he calls verification reports for a program. And he went to an unnamed individual in the Nuclear Regulatory Commissions's technical staff and said: "We have this thing and we would like you to review it." And this man responded by saying, "Well, until you prove that a sufficient portion of the industry is using this thing, we are not going to bother to do it." And that is what I mean: they are not going to do it. As far as the people in management, you come along and tell somebody you are going to spend $50,000 running simplistic set of test cases on a major software system, you are not going to find management too excited about doing that either because eventually that will be paid for by the client, and the client will charge the public and so forth. It all ends up back to the guy who paid the ultimate bill. And I see we come in here and we talk about this thing every couple of years and we do not move very far. You are correct, as far as I am concerned, that there is a law already on the books that says you have to do it. Since then everyone is conveniently avoiding the issue.

Bob Nickell:

Let's make sure we differentiate between the inability of a NRC man to find the time to review your particular verification process and somebody who is actually interested in it. He probably is quite interested in it, but he does not have the time. Those people get swamped with so much paper work that it is unbelievable. I think that the proper place to work again is through a professional society, and through Standards Organization, through the ASME Boiler and Pressure Vessel Code, not so much through the NRC people.

N. Krishnamurthy, Vanderbilt University:

My remarks are addressed to Dr. Fenves. I am sure that a man of Dr. Fenves' experience and stature has thought his remarks through very carefully. I must believe it's just a matter of semantics. It intrigues me, however, that he used the phrase "ad hoc programmers" nine times and added the adverb "unfortunately" three times and to me it sounded as if, and I want his clarification on this, it sounded as if he lumped most of the university professors who have come through the ranks and almost learned for themselves the crafts of programming and suffered through and published some and then went off to conferences, and their graduate students go through the same way and they make mistakes and do make inefficient programs but somehow in the end they seem to learn from it; and, I may also add that most of the programs that are popularly available today seem to have been produced by graduate students who sometimes have been ad hoc programmers in their own right; so, I would like Dr. Fenves to expand on these remarks and tell us, and I am sure other professors and faculty members would be curious, if he considered any of this process as inadequate or if he only referred to jumping into the ring with inadequate preparation or inadequate or limited experience.

Steven Fenves:

That is quite a challenge! I can only refer to this conference. This morning I was sitting in the back and listening to the papers. The papers are all uniform. Most of them have 20 slides, 10 slides of matrix derivations, problem formulation, the speaker clears his throat, and the 11th slide comes up, beautiful computer plotted output. Nothing is said about the efforts of six months, nine months or one year period from the time the last equation is written down and the first tentative, plotted output, not even about the dump output, is produced. That is the area of software engineering. The point that I try to make is that there are tools, disciplines, and mechanisms. The panel has mentioned many of these which are available for this process. For many of the small processes that we are talking about, it is not worth while applying them to that. Even the chart from the Bureau of Standards shows that the degree of documentation in the guidelines is a function of the cost in manpower effort. So I think I will stand by my statement that most people at universities are in this area which, by the definition of the slide, would not require any documentation, and in fact there is not much documentation on it. It does not require any formal tools, data structuring, and so on, beyond that solving of problems and the programs; in fact, we do not have any thing more than that. A number of people, you, your colleagues at Vanderbilt, lot of other people have gone beyond that, and have evolved computer programs which through one way or another have gotten into practice. The experience has been that these are quite difficult to maintain in most cases, and that in many of the large organizations, McDonnell Automation, for example, do not use the programs as coming out of research. They only use algorithms and descriptions and then manufacture from them the products they want. So I do not think I have to take anything back. And the other thing I like to point out in terms of what you said about change: Yes, there is change coming; our students do go over and take courses in numerical analysis, do take courses in Data Structures and so on, and software engineering in their work and it does show up. The thing that we learned on our own to survive on earlier machines, the students now learned it in a formal discipline. And I hope that this middle layer of ad hoc programmers eventually disappears. But there will not be a need of reinventing the wheel and redoing a lot of ad hoc software in order to formulate and solve the type of problems that have been discussed at many of the sessions at the Symposium.

N. Krishnamurthy:

Now I agree with you because you have explained the intermediate layer of training and discipline that we did not have available ten or fifteen years ago. So I tend to agree that if this is true, if professor and students do not take this initiative and opportunity and try to start from the do-loop stage of computer programming, then he is in for trouble. But I was wondering about the lack of explanation of this and now I think I got the idea. I am sure many of my colleagues insist upon the students going through this discipline and although the professor might not have gone through these courses himself, would learn by reflected experience. And now we do not have to insist on it.

John Hendrick, FMC Corporation:

I am a practicing engineer. I do not go too much with theoretical figures and I'd like to direct a question to Mr. Johnson. You made a comment that struck a tender nerve. You said something about implementation of computer programs by the engineer. Now our company is not large enough to employ a lot of computer programmers so I am at the mercy of the universities to create the programs that I need for me. Quite often I am called upon to go through the literature, find the program that I really like, and then try to implement it. It is a real difficult problem sometimes and requires an awful lot of manpower. I would like your comments on how we can go around this "National Society of Computer Implementators" or whatever you might want to call it.

Jim Johnson:

I appreciate your problem because sometimes poeple think that I am the "National Society of Implementation" as well. I gave you the illustration of the lieutenant that gave me a call Friday, but the week before that I got a particular question about NASTRAN, where a captain called me from the West Coast and wanted to know why is it that he cannot use the output 2 file which was generated by NASTRAN. He was an engineer and output 2 file meant nothing to him. He probably would not even understand the concept of a file being processed by a computer, and did not know what installation-defined defaults are, which could affect the formation of the data on that file. So we gave him no help since we had nothing to work with other than to tell him to go back and take three steps back and start from the beginning.

The direct answer to your question: Unless you are an ad hoc engineer, I strongly urge that you do not try to implement the computer programs of any significant size and scale. First of all, it is a useless waste of resources and manpower. Most installations do have professional software engineers, that is what they may call them now, but we use to call them at Wright-Patterson System Analysts, Computer System Analysts, Software Engineers, anything you want to. If the program is of any significant magnitude in terms of complexity and size, the most important job of the engineers is to get out of the loop during the implementation. And that includes you and me. Ok, does that answer your question completely?

John Hendrick:

As far as you are concerned it does answer the question, but as far as I am concerned it does not, because I am still in the same predicament that I was ten minutes ago.

Jim Johnson:

I have a solution for you. You have heard papers at this conference presented by what I like to think of as data service companies or organizations. Most of the major computer manufacturers do provide customer services other than hardware acquisition, and the universities are a very good place to look for assistance in implementation. I am sure the rates have changed, but one could pay a student at least a $1.50 an hour to put up a program on the machine. This would be an excellent avenue for technical assistance on implementation of computer programs. It not only provides some funds for the graduate students that need some financial assistance, but it does provide a training ground and some educational opportunities for them. Now if you are one of a more prosperous firms, you can go directly to a Software Services Center provided by specialized organizations like SDRC, or EMRC, or MARC, or similar places, or you could go to the Computer Manufacturers themselves, but then you may not be a Boeing or McDonnell or something like that. But maybe you can pay the high rates. But certainly if you are small as a firm, the universities are an excellent place to get implementation assistance. I should caution you though, that in the university environments, the necessity and desire are always there for experimentation and these graduate students can fix it so you will always have to come back to them.

John Swanson, Swanson Analysis:

I think I have heard a theme throughout the conference both in the keynote address and the panel, today, that the user has become much more sophisticated. I think I would echo that, and I think I would like to state in the area of verification, for example, that if the user wants verification he is going to get it. The same as if the user wants documentation he is going to get it. And I think that what we are beginning to see is the user saying I want this; and, because, as some of you may have noticed, since there is some competition going on in the software industry, the user is in the position to say what he wants, and he is quite likely to get it. I am not sure how this applies to the Public Domain Programs, especially in the area of verification of the Public Domain Programs. Who verifies a Public Domain Program and guarantees that it stays verified? Also, the comments on distribution referred to a good point that distributed programs have to be updated, but who would guarantee that the distributed programs are updated? You have done your job distributing the update, but it never gets into the program. There are a lot of questions here, but I think there has been a change and the user now has a lot more say in what he wants to happen. I am sorry to say that we are finally getting to publishing the Verification Manual and it is not because of the U.S. users; it is because of the European users. The Europeans are much more insistent on seeing good verification on a computer program.

Bob Fulton, NASA Langley Research Center:

We have heard a lot of discussion about dissemination and documentation, and it seems to me that we find ourselves with two types of programs that we are concerned about. The first is the research oriented program, which is developed by graduate students or by some of us for our own personal use. It seems to me we could dispense with discussing such research programs since they are intended primarily for the individual and his own needs, not of general concern to the

the broad community. We should focus our attention on the second type of program, those developed for distribution to others. For programs to be distributed to others, these are also of two categories: those already developed and those to be developed in the future. Most of our discussion seems to be focused on trying to collect and/or clean up existing programs, yet it seems to me there are limitations on what we can do with existing programs. We are best served to implement procedures to ensure that future programs are well developed. The discussion from Boeing dealt with how to manage the process to ensure that future programs are documented from their very inception. So that, to quote Boeing, "Coding is the last state of documentation." I dare say that most of us in this audience, when we have developed codes, have often written the code and then documented it. It seems to me that our attention should be first to close the barn door, and second, to look around at the horses outside, select the high quality ones and do something with some of them to recoup the resources we have invested in them. But we must work together to implement business like programming procedures which first develop documentation and second code, not vice-versa, as Steve said most ad hoc programmers seem to do.

Jim Raney, NASA Johnson Space Center:

I am one of those things that you all have been trying to define for several days around here as a software engineer. I am sort of out of place in this conference I guess. I am not an ad hoc programmer; I do not claim to be and never was (I interned for a few months as a graduate student, of course). What my group has to do is to build programs or systems (we prefer to call them) for the engineer or the group of engineers at our facility, that are willing to take the time. Now this is my primary point, that it is going to cost you something up front to get that barn door closed. You got to be willing to spend the time with us to define what it is you want to develop. Write down or help us write it down whatever it takes to define the barn that you are in. Then we will help you close the door, build the system that it requires on the computer to do the job that you want to do, document it as we go. But the trouble that we have with our people, even today, the good engineers are too busy many times to help us do that particular thing. And I think that if you really are serious about computer program construction in such a way that you will have a viable product that is disseminateable (if there is such a thing). That is the price you are going to have to pay.

Jim Johnson:

John Hendrick, of Santa Clara, directed a question to me, and I do not think I gave him the complete answer, although it was contained in my opening discussion. Bob Fulton reminded me of it in his comments. We are talking about large engineering software systems. First and foremost, to use one of these systems, one must maintain a certain level of capability within his firm. My estimate is that it takes two to five people in the company to maintain a large engineering software system, and that does not include maintenance and modification. That is just keeping it going in your house for someone else to do the maintenance and modification. So it is expensive. First of all there is an overhead cost, and as a customer user, to maintain a large system in-house. Ok, and I do not think that is what you were asking me in the question. Let us make the point that any large system for in-house use requires in-house support, and that is not maintenance and modification, but just keeping it running. The same group would implement any changes that are sent for updating or enhancements. That is once they are defined and shipped to you, someone has to enter these changes in the master code media. That is not maintenance. That is just updating your version. And this is why a current list must be maintained of all people, old and new, that receive your programs. Now, when you think of the cost of a professional, and you are talking about two to five people, you are certainly talking about $30,000 - 150,000 a year, depending upon the program just to maintain it in-house.

Anonymous:

Two comments I would like to make. One essentially is on the aspect of portable software. Our experience at the Bureau of Standards, essentially working with other federal agencies in this regard, is that unless portability is one of the initial design requirements of the software in the first place, we usually find that software developed for a particular application on a particular computer is only transportable among other computers of that product line. And if we are talking about true portability of software, essentially that has to be in the early design specification of that software. I think there is another alternative in this aspect, that as you develop software particular for your own internal use, you may hit upon something that you will find to be useful to others, and you want to share that. At that point of time you essentially inherit a different set of problems, in that you have to go back, redocument, and also call upon some technical competence essentially with experience in computer systems and their peculiarity. In order to tailor what you have developed to try to make it portable, this involves another economic consideration that has to be taken into account in the aspect of sharing software. Looking over the several years now that we have shared experience, both from the ADP standpoint and the user's standpoint, particularly in this area of application engineering where the users are becoming more sophisticated and are able to place upon the computer community their requirements, what they want in terms of performance quality, and certification, I feel that we in the ADP community are now ready to respond to that type of user specification. And I feel that it is only good for our industry and also good from the standpoint of the professions, other than the computer profession, that use computer services, that we now start to bound our computer services, our computer profession, with the type of requirements that only you are capable of laying upon us.

Dale Seamons, Information System Design:

My company provides computer services and in order to be most competitive in the marketplace, we find that custom programming should take full advantage of some of the unique capabilities if our hardware is required to be competitive. How do we play that against portability, that is an open ended question that I would like to direct to anyone on the panel who may care to respond.

Bob Nickell:

I would like to make a brief comment. I concur; we find that, at our company, custom programming for a particular computer is ideal as well. We have nothing but CDC equipment on site. But when we transfer our programs to other computers, IBM, or UNIVAC or some other system, we find that a complete recoding of certain modules is much easier than trying to make the program portable to begin with.

Anonymous:

Just to ask some general questions back at you. What percent of your code is really required to be machine dependent, and is it really necessary to architecture the software so that it is dependent on that machine? Is there really an advantage in doing that? Mainly we see that we can devise an architecture that is portable, which eliminates the large amount of portability problem. Secondly, some small percent of the code is generally machine dependent, which is generally there for the purpose of gaining efficiency. Could we not then design the software in such a way that we isolate that part of the code and hence then recode that part to make it portable? In other words, if we design the portability in the beginning by recognizing the need of it, and then avoid characteristics which are not of essential nature to the program, but which are only individual habits, for example, why use 10 H on 6600 machines when it is so darn difficult to get it over to IBM machines? Why not use some other format? This may solve the problem.

Dale Seamons:

I am aware of the trivial portable features, but what I am talking about are the really major architectural considerations for a big program. You mentioned 6600 or perhaps 7600 which has low-speed core, high-speed core, and you can take advantage of these system features. Similar features are new concepts on structuring the core, the way I see it, and almost dictate in many cases independent versions of programming.

Bob Nickell:

I think in those instances, that you make a deliberate decision to create code so as not to be portable.

Mike Gaus:

I think this really brings us back to a point that someone up here mentioned a little while ago. That is what the incentives will be for portability and if you are in the business of making custom software for a particular installation or customer, I would not think that there will be a big incentive to make it portable unless you could sell this to many other people. So this is one kind of situation. You have another kind of situation if you are looking at it from the standpoint of public domain software in which it is a governmental or federal investment being made. There the incentives for portability will be much stronger than the case of custom type installation. I think that the motivation might be different depending on what the viewpoint is.

Anonymous, ESA:

We have had a recent experience with a portable program that we had obtained from CDC. The CDC version ran on our machine for 96 hours. On an IBM 370/168, it took 36 hours of computer time just working on this particular job. The same program with some modification, $5000 worth of my work, ran in 5 hours. Since we are also in a competitive business, then why charge a customer $15,000 if it could run in $5000? Therefore, I would say portability at extremely high price is not a desired feature.

Jim Johnson:

I think now we are discussing some of the subtle problems facing our panel rather than the governing criteria. I would like to use this example to try to drive the message home. Those of you who have used CDC machines and have had some experience in SCOPE 3.2 and with SCOPE 3.3 with your COMPASS coding should know what customizing does for you. Because you started from scratch again and every time they change the SCOPE system, the tendency is to make it more difficult to upgrade to the new system to take advantage of it. I know of one program that went around this problem by building a special system within it, and every time you get the executable, you get the same system. And that is what Stan was talking about in the beginning. In your product development, insure that your specifications cover some potential growth or problems in the system. I think what we are talking about is whether you can build schemes for customizing at the product definition stage rather than downstream. I think what I would like to see addressed from the Panel is whether Stan or someone can define or isolate the useful life of large engineering software systems. You know, when you stop maintaining them, how do you amortize them over a period of time? How costly the job is, or what is the cost to be born. Stan, do you want to handle that?

Harry White:

I attended another seminar just a week ago, Monday and Tuesday, run by the Department of Defense. They have come to recognize that in many of their weapons' systems, now, software is the pacing item, both from the point of view of cost, schedule, and reliability. And they are getting to asking questions about what the full life-cycle cost of software is, recognizing that very often software shows up as a line in a procurement document without any requirement for a product to be delivered and no assessment of the maintenance cost of the software. And what they discover is that when the system actually goes on line, there are 65 programmers out there necessary to keep the system running, that nobody has ever talked about. They have found that for the same price of the system, software maintenance cost runs about three times the cost of hardware maintenance. There is a reason for that. Software systems tend to reflect the purpose and characteristics of the organizations that build them. Hence, if the organization producing a software system is interested in producing doctoral degrees, they may not be very interested in maintainability of that system five years hence by Sandia Corporation or Boeing. Therefore, maintenance does not show up as a design criterion in that system. If a research group is building a system, they may be interested in addressing a new formulation or concept; hence, maintenance is not a very high criterion in that system, nor is portability. You will not get in the system anything that was not placed in the design initially. And it is necessary, therefore, that you look at all the parameters you wish to consider in the full life-cycle cost of concern to you. Then you better look at those costs in your initial design. We know now, at least the Department of Defense costs were saying last week, that the full cost of software appears to be about 65 percent currently going into formulation, development, and validation, about 35 percent going into maintenance. The Department of Defense has tried to estimate the cost of their software each year and they only know that it is in excess of 3 billion dollars each year. They do not know the exact amount. When they went out to get their first estimates and got the figures back, someone went and looked at another place and found a single system that had more cost associated with it than all the systems they had up to that time. So they really do not know the full cost, but they know that maintenance runs approximately 35 percent of that cost. So these questions are becoming very, very important. Software systems' life cycles are now running in the order of 10 to 25 years. Of these the first 5 are spent to develop it, the next 5 to mature it, and then the next 5 to 15 years are spent in using it. Somewhere in that neighborhood is what we are looking at so we are moving to where we need a more professional approach to software.

Steven Fenves:

I would like to add some comments. The idea that Stan raised that the software products are a reflection of the milieu out of which they come, I see this happening all the time. Most of us in aero, civil, nuclear or whatever disciplines are represented, here, have had our engineering training on a project basis, where it was understood from the beginning that we are putting together, in a unique fashion, building blocks that existed before or possibly some new building blocks. But the emphasis has always been on projects and I think our software is developing that way. I see my colleagues in chemical engineering, for example, operating quite differently. Their chemical engineering has always been continuously process-oriented, manufacturing-oriented, long-term-production oriented. Their programs are written quite differently and their programming endeavor is managed differently than what we are familiar with. I think this extends not just to the question of the development but it goes beyond that to the question of verification, and certification as well. We are in an industry where there is no single responsibility, and that again reflects itself in the software. There is verification, quantification, possibly even certification on the part of the software vendor to the designer who operates as an agent of the owner. And then there is an entirely different process of the certification of the designer with his software tool being certified by the owner or by the regulatory agency. And sometimes when we talk about these topics of verification, I am not sure which side we are talking about, the vendor delivering a product to the designer who still has the professional responsibility for the end design or is it the designer with his tools delivering the product to the owner.

Again, if you look at some other industries where there is single management control and single fiscal control at the top, the problems are quite different. So we have to realize that we represent our own backgrounds, and the software we develop and the software we use directly reflects our background.

Avanti Shroff, URS-Madigan-Prager:

I have been sitting here for two days and there are a couple of things that are puzzling me in terms of what is happening. I feel there is a lag in terms of engineering profession and theoretical investigations and analysis. One of those things that I have found in my 10 or 11 years of experience, and I have used many software systems such as NASTRAN and STRUDL, is that the reason we are discouraged to use lots of these programs is the following:

> Analysis is an assortment of facts based on assumptions. Engineering and design are not as much a fact as they are improvization of

code and using the judgement and eventually coming up with something that is going to stand in the field. When I go out and use a software system I get a very interesting analysis coming out of this system, but then I have to take these results and somehow program them into my own design program. This is because, so far, I have not found a single software package which could give me a design as comprehensive as most consulting engineers would like to have. Now when I find out that I have finished my first cycle of design I have to go back to these people for another round of analysis. This I can avoid by two means. One, if I can get these people who are developing these various packages to give us a design which is very much up-to-date with respect to the code, which very rarely happens, the assumptions involved are very gross because the people who are writing these programs are not as much involved with the design processes as they are involved with highly theoretical analyses.

The second alternative is to write our own process programs with effort of our own, which, as Dr. Fenves pointed out, is not very software oriented, and then recycle our own design methods into these programs and come out with our own answers. Now, this answers one of the comments Dr. Gaus made that you take a civil engineering office with 12 to 14 engineers and very few people are using NASTRAN. This, perhaps, may cast some light on why these people are not using NASTRAN or ICES-STRUDL.

I do not want to mention any names, but I was in a design seminar where one of these software companies was describing the design capabilities of their outfit and I asked a simple question as "What K value do you use for column design?" And he said, "Well, K is equal to 1", K meaning the slenderness ratio effect in column design. This is a very important effect if you design high-rise structures, main frames, or no matter what you are designing. And I said, "Have you considered the fact that this K value has been a very controversial feature in the last five years and you really should not be using a K value of 1?" He said, "Well, we are not involved that deeply in that kind of controversy." Yet I know of many organizations who will use this program and come up with the final design answer that uses 8WF36 for a column. If they do not know on what assumptions these 8WF36 have been selected, it would be very difficult for that person to present this to the client, put his signature to it, and find out later that the column failed in the field.

Therefore, I feel that there is a complete lag between these two domains and I hope, and I had hoped, that we are going in that direction, that some effort is made to somehow relate analysis processes with design processes in terms of the latest code revisions and everything else. This, again, points out to the fact that we are discussing what is the life cycle of a software package. And I think it all leads together to see that all these things are put together into software packages as we go along in this software community.

Bob Nickell:

I would like to comment that what you are addressing is the development of postprocessing systems that are directly related to specific code requirements. I would like to say this in defense of the software developers. Most software packages are too general in terms of amount of application you can make of these programs to justify the developer to get into the business of furnishing a postprocessor for each kind of code design for which the package will be used. Therefore, we find that it either has to fall back on your shoulders as a user or a third party has to get into the act, either a service bureau or a government agency or someone like that, who will furnish postprocessors for specific sets of analysis packages. Let me give you an example. If the user community has enough muscle, they will force the development of postprocessing packages that will interface not only one but, perhaps, a whole family of analysis modules. Then your problem will be solved. But I think you have to develop a specialty clientele, in that case, so that if you want a Section 3 design, you are going to get a Section 3 postprocessor that will interface with ANSYS, with MARC, or any other package you are using. In your case, if you are looking for a civil engineering type module, you are going to have to get together with another group of people and somehow get it done, either through the government or common funding.

Stan Hansen:

Your description of software packages reminds me of how I felt when I went through university training where I discovered enormous quantities of lecture material on analysis and hardly anything on design. And there was a reason for that, I discovered after a while. And that is that analysis can be formalized to some large degree and it tends to be objective, whereas design tends to be a very subjective act dependent upon a great deal of vocational information relative to individual projects. Hence, there tends to be a reluctance to place within a hardwired software system parameters which we would consider to be design and normally would come from the mind of the person doing the design. I think that we are moving towards the time when we can see that certain design processes are more routine and objective in nature and can be hardwired into software systems. But I think we find that they are a small set of the total set and the larger set is very dependent on the vocational knowledge of the people local to the project.

Pat Norton, Control Data Corporation:

I have several comments. First, in this business of pre- and postprocessors, I think this is an excellent place to advertise our programs because, quite frequently, with a 50 or 100 line program, you can get a lot out of a program provided you have some understanding of the program and know where to look.

In your comment earlier, Mr. Johnson, on using Output 2, I recall that one time I was attempting to use some NASTRAN output and I could not find anything in the User's Manual directly on any of those output files. I finally wound up directing the output to a punch file and treating it as a BCD file rather than binary in order to postprocess it. This is definitely a weakness in NASTRAN's Manual, but there is a lot to NASTRAN that is hard to find.

On the subject of cost, it was mentioned that there is probably more investment in software cost than there is in hardware cost. An interesting observation that I stumbled upon in Datamation is that a rule of thumb in programming is a man-hour of work per line of source code, from the start of programming process until you have a debugged code. And I do not think that this includes the initial analyst's work or the time to document the program. And several people since have told me that the above is a good number. It makes it pretty darn expensive. So, well-organized programming is highly desirable. Unfortunately, I have seen many examples of not too well-organized programs.

On your observation that it takes 2 to 5 programmers/analysts to support a large package in-house, this is again often times brought on by companies who go out and buy a large system for a $200 copy cost and they find out that what they have done is to buy a black box with permission to open it up. To this end, many companies who really do not, but should, think about going to service bureaus like CDC, Cybernet, McAuto, etc., and take advantage of their bags. This may cost you a small amount more per CPU minute to run this way, but it avoids many headaches. In addition, you can usually get a warrantee from your service companies as to the acceptability and usability of the program.

F. McVey, McDonnell Aircraft Company:

There is a class of computer programs that I think would require a new form of documentation. We have a computer-aided design and graphics program that serves our engineering division. It is an extremely large and very interactive program. This program is on two 370/168's supporting approximately seventeen 2250 terminals. We have those terminals operating something on the order of 14 hours a day to generate design data for drawings for the F-18 program. Those data are then used in a hectographic program. So the users do not have to know in detail what is in the code, but they have to know how to use the program. If the program has a lot of flexibility, there are so many options in the way the user can apply it to a problem that you need to have some form of training. What we do is to provide approximately 24 hours on-line training to a scientist, and we have trained approximately in the order of 250 engineers. I can see that in the future, systems like this are used to the extent that there will be need for higher level documentation. This might be a video-tape lecture series, might be something involving tutorial on-line operations.

Bob Nickell:

I guess I would agree that there is a future in what we refer to, at Sandia, as an interactive, interrogative, preprocessor. Is that what you have in mind? That is what I call a level of documentation that is very useful as opposed to a lot of documentation that is not very useful.

Harry White:

I was hoping that someone on the Panel would ask a particular question, but since nobody has done so, I will. We hear a lot about verify, validate, qualify, but I have not heard much about what one verifies, validates, or qualifies against? You cannot validate something against which you have no standards. And you cannot qualify something to which you have nothing to qualify it for, nor a standard to measure against. I have had a bad experience trying to do that. We have found that without software system design, we would not validate the code except by sampling (i.e., just run problems through it). We could not qualify software systems against real life injury problems without some statement as to what those real life injury problems are supposed to be, or for what the software system was built. And it led me to believe very strongly in the process I talked about here, that you must get your requirements and your specifications in design down in order to validate software against something.

Stan Hansen:

Your statement about sampling is exactly true. That is the way it is done. Verification is a sampling process by which you examine the theories upon which the particular piece of software is based, and you sample selected portions of the system to see whether it is in fact coded as stated. The process of qualification is generally a configuration control problem and no one as yet has come up with a qualification program that attempts to cover the complete map of configuration controls that the program is capable of. In fact, all they do again is sample a number of configurations for the particular package that should solve most of the problems that will be done in that organization. But it is definitely a sampling process.

I think there is another way. We have validated programs for which we went into the subroutines and defined the purposes of that subroutine in an objective statement of what the subroutine is supposed to do; then make a statement of what the input parameters would need to be (that is, the parameters that are affected and the values of those parameters in order to exercise what that subroutine was supposed to do); then build those parameters up until you have a whole set of test cases. In this way, you have a cause and effect relationship between your testing and the software being tested. In so doing, we found extremely reliable software resulting. I guess, Ralph, you would not mind me telling this that the BUCLASP program that was delivered to NASA Langley received no award from NASA Langley for the quality of that system. In some 15 months of operation, I recall, I think there were 15 errors. In implementing another piece of software in which some 25,000 new statements were implemented, we went in and caused a one-to-one relationship to exist between test cases and the code being tested. That software was run over 300 executions a month from July through November with only three errors.

Deene Weidman, NASA Langley Research Center:

That method is practical, Stan, if you have structured programming. I wish I had one of my slides here. It shows that, for a simple program, if you tested a pattern every nanosecond, it would take 15,000 years to check all the patterns in the program. But if you have to search the program, you cannot do it.

With regards to INPUT 2, we are running into situations like that often where people have difficulty using some capabilities. We have 5300 pages of documentation at the moment. And we have more documents coming out. But sometimes you have trouble finding certain capability. And documentationwise, it is difficult to document all the capabilities and all the possibilities. When you have an element in

NASTRAN, you are not only having it for statics, you have it for dynamics, for buckling, vibrations, etc so that it is a problem for maintenance to have a program that anyone can have, that anyone can use. Jim is perfectly right that you have to have some staff who will keep running it for you. I can assure you that a program that runs now, in six months it will not. Not for any errors in the program, but because the system is changed. And we have many cases where we have spent many hours correcting a code to run under a new operating system.

Harvey McComb:

It is beginning to get around 5 o'clock. I would like to take one or two more questions.

David Perlmutter, COMSAT Laboratories:

We have a sort of a unique situation, and I am curious to find out if somebody else has the same situation. We discussed this afternoon having a staff to maintain a fairly large program. But we have NASTRAN, BANDIT, and a few other pre- and postprocessing programs that go with NASTRAN. We have absolutely no staff to support it. The engineering staff is struggling through NASTRAN trying to keep alive. If anybody else has this situation, I want to know how to remedy it. I am assuming that we are talking about a high priority engineering staff that runs around and buys large programs. Just last week I saw a demonstration upstairs in the labs of a half million dollar circuit design system that somebody bought but nobody knows how to use or what it is good for. We are fairly unique in that we have fairly large programs, and a fairly extensive library of them, and regular staff to support it. We have a computer center. We might do well to destroy it and pick up an outside service, but we do have a computer center and they are fairly insistent on staying in-house. We have a weird situation.

Jim Johnson:

Maybe I should answer your question with another question. Who runs your shop, the Computer Center or the engineers?

David Perlmutter:

The engineers.

Jim Johnson:

Then you should be able to get some programming assistance on your side of the house, some scientific computing people. Without that, your programs are going to die.

Anonymous:

One point that a lot of you are missing is that there are two kinds of maintenance. There is one kind to keep the program alive, the other kind to keep the engineer happy. Then you have got to have computer professionals to keep the big programs alive. And the way to do the other one is to realize that an engineer is dynamic, if he is doing his job right, and he must have a dynamic programming support to get it going. And I think that is where our problem is. We have got to admit that he is dynamic, and we have got to treat him as a dynamic quantity and go forth.

Harold Conner, Portland Cement Association:

This late time is an inappropriate time to bring up a controversial point, but I would like to just say that many of the problems that have been discussed here today, I think, have been addressed by this study of the National Institute for Computers in Engineering. I feel that if we continue to go in that direction, we will eliminate many of these problems.

Harvey McComb:

I am sure Mike Gaus, or Dave Schelling, or somebody will be glad to hand out some reports on that NICE study.

Mike Gaus:

I gave them all away.

Harvey McComb:

I guess that we perhaps wrap up our biennial discussion of this topic. I found that this has been very illuminating to me, and I hope that all of you have also gained something from the discussion.

There are two things that I want to do before we wrap it up and the first is that Nick Perrone has an announcement about his program this evening. Nick.

Nick Perrone:

I cannot help it also to add an anticlimatic footnote. In listening to some of these comments, it seems that this session might be labelled "The Confession Of A Frustrated Engineer, or Everything You Wanted To Know About Software But Were Afraid To Ask." But, perhaps, things have not really changed. I think that we have addressed more questions and suggested more problems than answers. I think that is healthy; that is a reason for a symposium. But I think there are still some major initiatives that would be welcomed and I think they are alluded to here.

Harvey McComb:

I would like to express my appreciation to the Panel members who have spent some time preparing these presentations for you. I would also like to express my appreciation to the audience who stuck with us here and contributed a great deal to our discussion. Thank you and good evening!

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15. Supplementary Notes

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Harvey G. McComb, Jr.: NASA Langley Research Center.

16. Abstract

As part of the Second National Symposium on Computerized Structural Analysis and Design, a panel discussion was held on Standardization, Certification, Maintenance, and Dissemination of Large Scale Engineering Software Systems. The panel discussion was divided into two parts, the presentation by panel members and the audience response and discussions. This panel discussion is presented as a verbatim transcript of the meeting.

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