W. Clement Ley: Nineteenth-Century Cloud Study and the European Jet Stream

1. Introduction

In his account of the times, personality, and actions of Carl-Gustaf Rossby, Phillips (1998) refers to the existence of strong upper-level winds being established from the motion of cirrus clouds by German meteorologists in the late 1930s, and actual encounters of jet streams over Europe by crews of high-flying aircraft during World War II.

As a further contribution to the history of jet stream studies, relevant findings of the nineteenth-century English meteorologist, the Reverend W. Clement Ley (Fellow, Royal Meteorological Society), are selected for discussion in the following article.

Like his coworker, H. H. Hildebrandsson at the University of Uppsala, Ley was a pioneer in the field of cloud study and was particularly concerned with determining, by means of nephanalysis, the relation between upper circulation patterns and the surface pressure distribution (Kington 1999).

2. Ley’s study of upper-level winds

Ley was convinced that the collection and analysis of observations of the motion of cirrus clouds would lead to a better knowledge and understanding of winds in the upper troposphere. As he himself expressed:

For more than ten years he [that is, the writer, Ley] made numerous observations every day, whenever the conditions of the atmosphere permitted, on the direction and rapidity of the upper-currents, and in this task he has been from time to time assisted by the labours of a few friends who have made simultaneous observations in other localities ... For a long period the results arrived at appeared highly variable and unsatisfactory, but ... certain laws appear distinctly traceable amidst the maze of the upper-current variations (Ley 1872).

In his efforts to establish a network of cirrus cloud observers, Ley made requests to meteorological observatories and societies in both the British Isles and mainland Europe. As a result, over 600 reports became available for analysis and, as he explained:

Six hundred and twenty observations upon the motions of cirrus clouds have been selected for examination, and these have been classified according to their position at the time of observation with respect to the neighbouring centres of high or low pressure ... It would seem that some depression systems, when in the earlier stages of development, and when not of very great geographical extent [developing polar-front waves], scarcely affect the motions of the upper-currents in their vicinity in any perceptible degree. The atmospheric disturbance is in these cases confined to the lower strata of the atmosphere, and the motions of the higher continue as they were previous to its development, and are dependent upon the distribution of the more distant and extensive pressure centres.

Velocity of upper-currents

The ordinary range of the actual rapidity of this current is about twice as great as that of the rapidity of the surface-winds, for while the latter, at situations most fully exposed to their violence, rarely attain, in Europe, a velocity of more than 60 or 70 miles an hour [27 or 31 m s⁻¹], the most elevated clouds not uncommonly traverse a distance of 120 miles an hour [54 m s⁻¹], and occasionally much more. The majority of instances in which very high velocities have been observed over the British Isles were in autumn, winter, and spring, and
occurred when great but distant depressions existed in the northeast, in Scandinavia or Finland, and when the direction of our upper-current was from [the] northwest or north-northwest. The backing upper-current . . . existing over the eastern arc of an advancing depression [upper-flow warm ridge pattern] is also sometimes of extreme velocity when from a north-westerly or westerly point, and appears to be especially so when the depression is of great intensity, and therefore when the equatorial surface-wind [maritime tropical air mass] is about to attain a very high force (Ley 1872).

Twenty years or so later, in his “study on the structure and characters of clouds,” Ley (1894) presented further findings based on his analysis of cirrus motion. These again show that he had identified distinctive properties of upper-air flow that are now related to the jet stream, such as the strong northwesterly upper current that frequently occurs over the British Isles ahead of frontal cyclonic weather systems:

*Cirro-filum* [fine cirrus], occurring as it does at an average altitude of 30,000 feet [9 km], moves generally with great velocity. Over the British Isles it is not an uncommon thing for the cloud to travel from [the] northwest in front of a disturbance with velocity of 150 miles per hour [67 m s⁻¹] (Fig. 1). This velocity is more than twice that which is experienced, except in the most violent gales, in the lowest beds of the atmosphere (Ley 1894).

Ley realized that cirrus clouds streaming out ahead of an approaching depression gave an early visual indication of its approach and he foresaw the concept of upper-air steering when he stated:

And, on the whole, we may arrive at the conclusion, so far as our present knowledge extends, that the path of a cyclone is partly dependent upon the prevailing direction of the great upper currents (Ley 1894).

After this promising start, half a century elapsed before any further progress was made in understanding the role of the jet stream. Although cloud observations made prior to World War I confirmed Ley’s conclusion that strong upper-level westerly winds occurred in middle latitudes, the reports were not pursued (Reiter 1963). It was not until 1945, when Rossby and colleagues at the University of Chicago, having analyzed upper-air charts covering the entire Northern Hemisphere, discovered that the flow aloft, especially in middle latitudes during winter, “is suggestive of a broad stream of air meandering eastward around the hemisphere in wavelike patterns, with the energy of motion concentrated in narrow bands of high speed . . . Rossby called the newly discovered current ‘jet stream’” (Riehl 1962).

### 3. Concluding remarks

The place of Ley’s cyclone model (Fig. 2) in the history of meteorology has been discussed by Ludlam (1966) and Kutzbach (1979). Also, in his contribution
Ley’s equally remarkable insight in detecting salient features of upper-air flow from the analysis of cirrus motion also deserves to be more fully recognized in the history of meteorological ideas.

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The main article on Ley, “Meteorologist’s profile—William Clement Ley,” commissioned by the Royal Meteorological Society’s Specialist Group for the History of Meteorology and Physical Oceanography, is to appear in the June 1999 issue of Weather.

References


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FIG. 2. Model by Ley (1894) of a mature progressive cyclone with the position of the surface center indicated by an inner round isobar. The distribution of cloud and weather is given, and the arrows show the direction of the upper winds, as indicated by the motion of cirrus clouds. The central calm aloft lies to the rear of the cyclone center at the surface, whose path is shown by the large straight arrow toward the northeast.

to the Rossby memorial volume, Bergeron (1959) has drawn attention to the 40-year hiatus in the development of synoptic analysis due to the neglect of work by FitzRoy and Ley:

had the ideas as to models of such men as FitzRoy and Clement Ley been followed up whole-heartedly, no doubt to-day’s main knowledge of weather systems and their structure at the earth’s surface ... could have been gathered long before 1900. Already in about 1890, budding Aerology would then have begun to look rationally into the three-dimensional structure of these systems. ... Aerological stations (with kites and sounding balloons) could have formed a real network in Europe and N. America already in, say, 1905, instead of (with radiosondes) in 1945.
A recent article by Rom-Kedar and Paldor (1997, hereafter RKP) explaining periods of meridional flow observed by constant level balloons in the Tropical Wind, Energy Conversion, and Reference Level Experiment (TWERLE) is addressed. The explanation is based on a stability analysis previously developed by the authors, called “near-flat parabolic resonance.” My purpose here is to question both the need for and the physical validity of their explanation.

The authors refer to observational analyses presented by Julian et al. (1977) and Levanon et al. (1977, hereafter L77). They paid special attention to the balloon trajectory shown in L77. Figure 1 (adapted from Fig. 3 of L77) shows a balloon trajectory (TWERLE 766) tracked from 3 to 14 December 1975, over Antarctica approximately along the 15-kPa pressure level. Except as marked, times along the trajectory are mostly not determinable from the original figure. Antarctic land and ice edge contours are indicated as light lines. Figure 2 (Fig. 2 of L77, redrawn) is a synoptic analysis of the height field at 15 kPa on 11 December constructed with major aid from several balloon trajectories. Note a 30° counterclockwise rotation and an increase in the spatial domain from Fig. 1 to Fig. 2. The synoptic analysis was done just after the balloon crossed its earlier path on its second counterclockwise loop, having nearly crossed the South Pole. From the X mark at the head of the wind barb it was then located at about 85°S, 45°W (315°) and was moving to the northwest at about 5 m s⁻¹. Two other nearby balloon trajectories and the South Pole sonde station observed similar wind vectors.

RKP state that the meridional track excursions of the L77 trajectory “contradict the Eulerian observations . . . which show that the prevailing winds flowing around the globe are mainly zonal.” They also state that “there have been no other reports on observations of either such high meridional winds or of such low zonal winds except for those resulting from monitoring constant-level balloon trajectories.” Most of the sonde stations in Fig. 2 are in the strong midlatitude westerlies. Several of them show significant meridional flow. By consulting a few daily weather maps and the wind soundings from which they are produced, one may reliably assert that strong meridional motions are common almost everywhere in the troposphere and lower stratosphere.

During the last three days of its trajectory over Antarctica, balloon 766 moved northward and then eastward, roughly in agreement with the analyzed geostrophic wind on 11 December. A week earlier the balloon had moved southward across the 11 December height contours, but I know of no reason to assume that the 11 December height field was valid a week earlier. In this respect I believe that RKP have overestimated the time range of validity for a weather map analysis. Levanon et al. (1977) stated that the balloon trajectories were their principal source of data, and their analysis was presumably made using strong geo-

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**Fig. 1.** The traverse of TWERLE balloon 766 over Antarctica, 3–14 Dec 1975, adapted from Levanon et al. (1977).

**Fig. 2.** Synoptic map of the 15-kPa pressure surface at 1200 UTC 11 Dec 1975. Constructed from nine radiosonde reports and 32 TWERLE balloons (marked with “T”). The balloon marked X is 766. Adapted from Levanon et al. (1977).
strophic constraints. They stated the existence of six other “similar” synoptic analyses for the period of this trajectory, which I assume means they are similar in domain, resolution, and accuracy, but not necessarily in synoptic features. The counterclockwise loops might be inertial circles, for which the wind is not geostrophic but usually light. Other trajectories, mostly in lower latitudes, are presented by Julian et al. from TWERLE and by Morel and Bandeen (1973) from the Eole experiment, when balloons flew at about the 20-kPa pressure level. Periods of meridional flow and occasional flow reversals or loops were observed, but Morel and Bandeen also used their trajectories as the basis for synoptic analyses in the Southern Hemisphere, assuming quasigeostrophic flow fields. While RKP are certainly entitled to seek an alternative explanation for the observed trajectories, I believe that they are burdened with grounds for rejecting the conventional quasigeostrophic flow field explanation.

Regarding the title of the RKP article, a parcel can follow a largely meridional trajectory for long times and distances within quasigeostrophic flow. It is not especially unusual to see a height contour (geostrophic streamline) extending from Alaska to Florida. In cases of strong blocking flow, such streamlines have been observed to be almost purely meridional.

After further personal correspondence with the authors, I believe that, besides the above observational interpretations, our primary differences can be largely summed by their communicated statement (not obvious in the paper but more explicitly stated in their reply) that balloons need not follow the motion of the air in which they are embedded. The nearly inextensible balloons used in Eole and TWERLE were expected to travel approximately along a constant density surface. I know of no plausible physical concepts that would allow such a balloon to be significantly diverted from the component of horizontal air motion along its appropriate density surface. A balloon or other object can only move through the air with aid of a propulsion device and at a continued cost in stored energy.

In summary, the authors’ stated need for an explanation for balloon meridional motion does not, in my opinion, survive a careful look at the L77 data or a casual look at other atmospheric environments. Their theoretical approach is unfamiliar to this reader in detail, but the implication that horizontal balloon motion can be greatly diverted from horizontal air motion appears contrary to Newtonian dynamics.

References


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Reply

1. Introduction

We thank D. Lilly for his comments (Lilly 1999, hereafter L99) in response to our recent paper entitled “From the Tropics to the Poles in Forty Days” (Rom-Kedar and Paldor 1997, RKP hereafter), which provide an opportunity for us to clarify the general approach and several issues that were discussed but not fully explained in RKP. The two objections raised in L99 to the theoretical results presented in RKP in an attempt to explain observed balloons’ trajectories are that all observations of balloon trajectories have already satisfactory explanations based on quasigeostrophic flow and that the validity of the Lagrangian model employed by RKP is questionable. Lilly’s (1999) criticism of the explanation put forth by RKP is not followed by any specific alternate explanation and, instead, it offers a tedious analysis of the synoptic map drawn by Levanon et al. (1977) along with a peculiar definition of the term “similar” used by
Levanon et al. (1977). By the same token, the doubts L99 expresses on the validity of RKP’s model are not substantiated by any specific recommendation of the terms that should be added to—or deleted from—the governing equations of RKP, or of a new set of equations that is more relevant to the problem of balloons’ flight along isopycnic surfaces. In the following sections we address, however, these two general issues, as well as other issues that were assumed self-evident in RKP.

2. Balloon trajectories and quasigeostrophic flow

Lilly (1999) claims that the occurrence of several-day-long periods during which balloon trajectories are directed nearly purely meridionally with little or no zonal displacement [e.g., Tropical Wind, Energy Conversion, and Reference Land Experiment (TWERLE) balloon 766 shown in Fig. 3 of Levanon et al. (1977) and the typical latitude excursion by Eole balloon trajectory shown in Fig. 7 of Morel and Bandeen (1973)] does not require an explanation. The hand-waving approach employed in L99 to account for the dynamics of balloons is that they are simply advected by the air in which they are embedded and that the flow of air is quasigeostrophic.

We certainly agree that there are times during which the prevailing winds at a given location are mainly meridional, but this does not change the fact that the mean circulation in the upper troposphere is zonal, which is the claim made in RKP; even in the case of a blocking, mentioned in L99, when a geostrophic streamline extends all the way from Alaska to Florida, the flow has nearly equal zonal and meridional components. By contrast, the latitude time series of a typical balloon trajectory shown in Fig. 7 of Morel and Bandeen (1973) contains nearly 16 northward swaths and 16 southward swaths in the two-month period 1 November 1971 to 1 January 1972, each swath lasting about 2–3 days, and the distance covered in each swath is as large as 2000 km. The transition from one meridional direction to the other is very abrupt and lasts typically a small fraction of one day. How can geostrophic flow turn its direction so abruptly?

The trajectory of TWERLE balloon 766, too, is very poorly explained by geostrophic flows: It is difficult to imagine the geometry associated with a system that is responsible for a balloon flying poleward about 15° latitude (1650 km), between 82° and 67°S, (along which the local earth’s radius has decreased from 2500 to 890 km) for two days, very nearly parallel to 15°E longitude! The 11 December height contours (Fig. 2 of Levanon et al. 1977) that, as L99 admits, balloon 766 had to cross during its poleward flight on 3–4 December are representative of six similar synoptic maps constructed for the entire period 3–4 December during which the balloon trajectory traversed Antarctica. (In the absence of additional data, the only logical interpretation of “similar” maps is that they are similar!) Thus, the claim made by L99, without any proof, that the motion of this balloon was indeed in quasigeostrophic balance with the observed geopotential height throughout its entire flight, is simply unfounded.

An additional point related to the dispersal of an ensemble of balloons rather than to individual trajectories and that seems to contradict the paradigm of balloons being simply advected by air that can move zonally or meridionally with equal probabilities is the following. The dispersal of Eole balloons expected if eddies are the main dispersing agent is nearly isotropic, while the observations clearly show (Fig. 9 in Morel and Bandeen 1973) a preference to a fast zonal dispersal compared with the meridional one. This preferential zonal dispersion is particularly significant at large distances (over 700–1000 km) where it exceeds the meridional one by a factor of 2. Similar preference to zonal dispersion is also clearly evident in TWERLE (Julian et al. 1977). The use of trajectory data to construct geostrophic streamlines (Morel and Bandeen 1973) does not prove that the flow is geostrophic. It is simply the only way to obtain approximations to streamlines that prevailed during the balloon’s flight.

3. The relationship of the Lagrangian model to airflow

The second question raised by L99 is, how likely is it for air parcels to exhibit the flat parabolic resonance suggested in RKP as a potent mechanism for ageostrophic (cross isobaric) flow of particles? Here we wish to adopt the safest avenue and adhere to strict scientific standards. The Lagrangian theory developed in RKP ignores continuity altogether. As such it cannot be applied directly to the study of any fluid dynamical problem. One can make a whole slew of conjectures ranging from the parabolic resonance is entirely irrelevant to fluid problem, to it is as relevant...
to fluid flow as it is to particle motion. As far as we are concerned, we simply do not know and therefore have chosen not to speculate. As a result of this scientific caution, the focus of RKP is balloon trajectories and not airflow. The approach adopted by L99 is that balloon trajectories are representative of airflow, overlooking the fact that airflow is constrained by the continuity equation. Anyone can make his/her own guess, but it is important to emphasize that RKP have only proven that the new mechanism, of flat parabolic resonance, exists in particle trajectories.

A related fundamental property that seems to distinguish the trajectories of balloons from those of air parcels is evident in the very fast rate of horizontal dispersal experienced by clusters of balloons. Morel and Bandeen (1973) estimate that mean pairwise separation of balloons is as high as 2 m s\(^{-1}\) when they are 100 km apart. Similar, but slightly higher, rates of dispersal were reported in the TWERLE balloons (Julian et al. 1977). Morel and Lercheveque (1974) estimate (their Fig. 9 and Table 1) that at small (less than 100 km) mean balloon separation the mean squared relative velocity of the balloons is significantly larger than estimates based on two-dimensional turbulence models of quasigeostrophic atmospheric motion. The existence of such strong horizontal divergence of balloons certainly does not imply a similar rate of horizontal divergence of airflow. Similarly, the existence of flat parabolic resonance in particles does not necessarily imply its existence in airflow.

If indeed parabolic resonance applies to particle motion only and not to airflow (as L99 has wrongly interpreted our cautious approach) then the question is raised by L99: How can trajectories of balloons, which are embedded in the surrounding air, diverge significantly from those of the air itself? Here we can only suggest that the frictional forces between a balloon and the surrounding air act on the scale of the particle’s size but are negligible on the four to five orders of magnitude larger synoptic scale. On this large scale, constant-level balloons respond directly to the same forces that move the air (if indeed air flows along isopycnic surfaces), but since they are free of the continuity constraint they can exhibit different trajectories in response to the same forcing. This is a very similar rationale to the one used when studying an inviscid theory for synoptic-scale flows and ignoring the viscosity that has a significant effect only in the microscale.

To further bolster the Lagrangian approach in the study of particle motion we note that recent calculations based on advection of particles by velocities obtained from Eulerian general circulation models (i.e., the Lagrangian advection method) show either pure zonal particle trajectories (e.g., Trounday et al. 1995) or mainly zonal trajectories with semiloops similar to the ones shown in Figs. 2b and 2c of RKP (e.g., Allam and Tuck 1984). These model-generated particle trajectories are very strongly linked to those of airflow so that, by definition, the deviations from geostrophy on timescales of 1 day have to be minute [e.g., see the comparison between the model-generated fluid parcels trajectories and those of particles in Trounday et al. (1995)]. It is no wonder, therefore, that no Eulerian calculation has ever successfully reproduced the rare observations of either the oscillatory meridional trajectories (Eole) or the ageostrophic meridional squirts (TWERLE). The Lagrangian dynamics approach adopted in RKP can successfully reproduce both observations as is evident in Figs. 1 and 2a of that paper.

4. Some clarifications regarding RKP terminology

We note that in RKP the dynamical system terminology regarding flows and their stability is used rather than the fluid mechanical/atmospheric terminology. Thus, the term “flow” in RKP designates the temporal evolution of the state variables (spatial coordinates and velocity components) and not the movement of air masses. The term “unstable” is used in the fluid dynamics literature to denote the occurrence of mean flows characterized by an exponential temporal growth of all state variables that represent small perturbations to the mean flow (e.g., baroclinic or barotropic instability). The energy contained in these initially small perturbations grows exponentially with time, too. In the dynamical system literature, the term unstable describes a fixed point (steady state) that has at least one variable along which the temporal evolution is exponentially away from the fixed point. In the absence of frictional forces the total energy remains unchanged and the increase in value of one variable is compensated for by a decrease in the value of another. We thank D. Lilly for attracting our attention (in previous letters and personal communications) to the fact that our letter may be completely misinterpreted by readers who are unfamiliar with the dynamical system terminology.

To summarize our reply: A hand-waving explanation (e.g., L99) that “a parcel can follow a largely
meridional trajectory for long times and distances within quasigeostrophic flow” is a plausible, yet un-founded, suggestion that heuristically employs classic transport mechanisms. The application of such a classic approach has never successfully reproduced the rare trajectories observed during TWERLE. We feel that discovering the existence of a previously unknown mechanism of singular transport (i.e., transport occurring infrequently in meridional squirts along narrow longitudinal bands) in the atmosphere based on a careful analysis of the governing dynamical equations is a worthy alternative. Needless to say, the quasigeostrophic dynamics is included in the RKP model near the stable equilibrium solution. The analysis done in RKP of the well-established governing equations, which uses recent advances made in the field of dynamical systems, has produced a dynamical feature that may be helpful to the readers of the Bulletin of the American Meteorological Society.

References


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corrigendum

In the article “Cloud Microphysics Retrieval Using S-Band Dual-Polarization Radar Measurements” by J. Vivekanandan et al., which appeared in the March 1999 issue of the Bulletin, the captions for Figs. 2 and 3 were erroneously interchanged. The caption labeled Fig. 2 should correspond with Fig. 3 and the caption labeled Fig. 3 should correspond with Fig. 2.

The Bulletin apologizes for this error.